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A

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EDITED BY

W. H. M. CHRISTIE.

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THE OBSERVATORY,

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No. 25. MAY 1. 1879.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Wednesday, 1879, April 9.

Lord Lindsay, M.P., F.R.S., &c., *President*, in the Chair.

Secretaries: Mr. J. W. L. Glaisher, F.R.S., and
Mr. A. Cowper Ranyard, M.A.

Mr. Glaisher reported that during the last month 65 presents had been received, none of them calling for special notice. Thanks were voted to the donors.

The President announced to the Society the death of M. Edmond de Chazal, of Mauritius, a gentleman who, though not a Fellow of the Society, had done very much for the science, who had placed his observatory in the hands of Her Majesty's Government, and whose interest in the work was only equalled by his kindness during Lord Lindsay's illness when in Mauritius.

Mr. Ranyard read a paper by Mr. Dreyer on an apparently new variable star; and also a paper from M. Trouvelot, of Cambridge Observatory, U.S., on some interesting observations by M. Trouvelot on a volcanic appearance in the Sun.

The President. It is a curious subject, and one well worthy of considerable attention to those who work much with the spectroscope: at the same time I think it is a great pity that M. Trouvelot never found anybody, when he was observing, to come and look through his instrument at the time, and to check the results which he obtained. It is a matter of great interest, and would have been of considerably *more* interest had that been done.

Mr. Common read a paper entitled "Notes on Large Telescopes, and Suggestions for Mounting Reflectors." This paper, after briefly alluding to the advantages and disadvantages of the two kinds of telescopes, the reflector and the refractor, advocates, as the best means to increase optical power, the use of the silver-on-glass reflector, and gives a number of conditions that ought to be fulfilled so as to get the best results from this kind of telescope when made of large size.

Captain Noble. It seems to me that Mr. Common's tenth condition is the one which will be most difficult to satisfy in future; *i.e.* as to what is the proper locality for those enormous telescopes. Every man who has had practical experience knows that on many nights when stars are sharply defined in a 3-inch telescope, a 7-inch will convert them into blobs and all sorts of things; and the larger the aperture, the more the extent of that difficulty increases.

The President. The subject which Mr. Common has brought before us is one of extreme interest. I should like to know what aperture in a reflector he would suggest as being equal to a 26-inch object-glass.

Mr. Common. I have taken it in this paper according to what Dr. Robinson has said—35-inch speculum-metal equal to 35-inch object-glass—the speculum metal having a reflecting-power of half that of silvered glass. It seemed to me that 30 inches would be the point at which the power would be equal.

The President. That would lower the power of the refractor to what I consider would be the correct amount. Mr. Common referred to the difficulty that he has in obtaining glass of large diameter. I can speak feelingly on that subject, because for my 15-inch telescope I had considerable difficulty in getting glass. In fact I had to return the flint lens three times to Chance before I got a sufficiently good one for my purpose. Since that time Mr. Grubb has taken it in hand to make a telescope of 27 inches aperture for the Vienna Observatory; and I understand he has also had considerable difficulty with his glass,—so much so, that the last chance is now being given to the makers of that glass; and if they cannot succeed with it, he will have to try in another place. There is considerable difficulty in getting a large disk of glass completely free from striæ in the manufacture. However, the production of glass for a speculum would not be so much affected by those difficulties in making. Of course in specula there is a great necessity for having an equal annealing of the glass when it comes from the cast. I believe Mr. Common can speak with true feeling on the subject, as I believe he has lost many of his children, which have suddenly burst after a good deal of work had been expended on them.

Mr. Common. Not his children, my Lord but his specula.

The President. I quite feel for him as a father. As to the question of the reflecting-power of speculum-metal being 0·46 and of silver 0·824, and the danger of silver losing its reflective power by tarnishing, there was published early last year, in the 'Chemical News,' a method of depositing metals by means of an induction-spark in a vacuum. I communicated with Mr. Crookes on the subject at the time. He did not know any thing about it; but he tried the experiment. He deposited some platinum and arsenic on glass in a vacuum, and got a most beautiful film of intense hardness—so much so, that it was impossible to draw on glass which had been prepared in this way. I believe the deposit of platinum on glass has become almost a commercial matter, and that they are beginning to introduce it in the hardware-shops to take the place of bright steel for the sides of fireplaces, in order to save trouble to housemaids. I understand that this also is prepared *in vacuo*; but I cannot speak with absolute certainty.

With regard to the question of supporting large disks of glass, some little time ago Mr. Bessemer brought forward a very interesting paper in which he suggested that the base of the mirror, or the reverse of the mirror, should be ground to a curve, and then be laid upon a bed of iron ground as nearly as possible to the same curve, and that this bed should be channelled or have grooves cut in it. Then he exhausted the air underneath, and so obtained a solid foundation for his mirror. It was only a suggestion: I am not sure as to its having been carried out.

As to the question of the size of the reflector, Mr. Common mentioned the possibility of getting a larger flat. That which is used by Lord Rosse with a 6-foot telescope is about 8 inches by 14, or 8 inches by 12.

As to the first condition of no tube, it is some little time since I have been at Parsonstown; but my impression leads me to say that Lord Rosse prefers the greater part of his tube to be open, as a lattice. I think I am right in saying that under many circumstances he found a decided improvement by placing a sort of dark cloth or a shield of blackened tin just over the part where the mirror comes. It need not be entirely covered up; it would be merely a sort of lattice-work with the power of laying a protection over the surface of the mirror.

Equatoreal motion of course is the most valuable; and I hope that in future no large telescope will ever be mounted without having that motion.

Of equal importance is Mr. Common's fifth condition, of having clockwork and circles, and a collimator for finding the true collimation of the mirror.

As to the question of stability and the amount of friction, I do not exactly know what amount of mercury will be required for Mr. Common's plan; but it seems that it would be rather a large bulk of mercury to carry a heavy telescope, and to relieve the friction of such a heavy telescope as he would suggest by his paper.

As to the question of resilvering (condition 8), I was not aware that silvering could be done with advantage according to the ordinarily accepted methods except when the face was downwards. If Mr. Common can show that it can be done face upwards, it will be an enormous advantage to every body who has to work with large mirrors, even with flats not exceeding 1 foot in diameter.

As to a convenient stand, there is nothing more difficult to make, or, I may almost say, more difficult to conceive, than to have a thoroughly convenient stand for working at all altitudes with the Newtonian telescope. I find that working with the 10-foot or 11-foot telescope the best plan is to have something like steps, pretty easily movable, and with a sort of chair attached to it, which can be moved to any height. As to the locality, I will leave that to Mr. Common for the present.

Mr. Ranyard. I have seen Mr. Common's 18-in., and was exceedingly pleased with the plan of nearly floating the axis in mercury. It did not hold as much mercury as might at first sight be thought. It is not necessary to have more than ⅛ inch in order to get the mercury fairly round the hollow tube. The pressure can be very delicately adjusted; the instrument certainly moved with very great freedom indeed.

Mr. Lecky. The great difficulty with respect to glass specula is in the thickness. You may remember Dr. Draper being here some years ago, and, speaking of his glass mirror, he said he had a 2-foot mirror which was only 2 inches thick. I put the question very pointedly to him to know whether it really was the case, and he assured the Meeting that it was so. I do not know what his mode of support was, but I think it must have been very difficult to support a mirror 2 inches thick without flexure. The difficulty of getting a glass mirror any thing more than 4 inches thick is very great. There is scarcely any possibility of annealing a glass mirror over 4 or 6 inches thick. I had occasion to get some sunshine-balls from Chance some time ago, and there was the greatest difficulty in getting a ball 4 inches in diameter. None had been made at that time. There are some of 4 inches now coming from Chance to the Meteorological Society.

As to speculum-metal there is no difficulty; you can get it of any thickness you choose up to 20 inches or 2 feet, only give it time enough. Mr. Grubb has given, in the 'Philosophical Transactions,' a very minute account of the manufacture of the Melbourne telescope, and shows the great care taken in annealing; but I believe in glass it is impossible.

The President. Those glass balls are required to act as lenses, and therefore they must be without striæ, as well as being without strain inside; whereas there would be no necessity whatever to have no striæ in the case of a large mirror for a reflector, as the one is a catoptrical instrument, and the other a dioptrical.

Mr. Lecky. I think it is impossible to anneal glass of that thickness; it is almost certain to burst.

The President. In Paris they have a 4-ft. reflector, and I think they only broke one glass mirror before they obtained two disks of sufficient thickness, something like 8 inches thick.

Mr. Ranyard. I had an opportunity of seeing Dr. Draper's 28-inch when I was in America last year, and I must say I was very much pleased with what I did see of it. I saw it on a high star and on a low star, and I did not see very much flexure which I could detect. He has a peculiar method of mounting with counterpoises. Loose pieces of glass are cemented on to the mirror behind, and little levers play upon them in every position in which the instrument is turned, so as to overcome the flexure.

Mr. Lecky. I think he said that the mirror was mounted on a bed of blankets.

Mr. Neison. Mr. Common lays particular stress on the difficulty of getting the mirror resilvered. I think it is probable that it would not require resilvering oftener than about once in every ten years. I had at Hampstead a silvered glass speculum which retained a fair working polish for a period of five or six years, and that at a place where there is plenty of gas round about. The difference in durability arises from the method by which the mirror is silvered. By one method, if you add too much ammonia, the silver will get tarnished in a couple of months, whereas by a proper method you will get a hard silver coating which will bear an enormous amount of rubbing, and will refrain from tarnishing in a most extraordinary manner. It will remain almost as bright as new for months at a time. I have had the 9-inch mirror, which belongs to the Society, silvered; it was carefully rubbed up, and it was almost as good when I came to fetch it in the early part of the year (that is, four years after it was silvered) as at the time when I had it done.

With regard to the relative brightness of reflectors and refractors, I think the amount of light given by a reflector is very generally under-estimated. Where you have a reflector in good polish and in good condition, it will come very much closer to a refractor than most people fancy. I know that a silvered glass 8½-inch reflector has proved itself a fair rival for an 8-inch refractor, which is running it very closely indeed; so that, instead of the two telescopes giving the same amount of light when they were 30 inches in diameter, you will probably find them nearly equal, if you take them as small as 20 inches in diameter.

With regard to mounting a telescope, it is very important. However big you get your object-glass you can only support it round the edge; therefore there must be a tendency to have it bend in the centre. Some have suggested having an hermetically sealed tube, and pumping it full of compressed air; but anybody who has tried it will know how difficult it would be.

Mr. Thornthwaite. Might I ask what is the *best* way of silvering? Mr. Neison has said Mr. Browning's is not perfect. My own experience is that I cannot get a film that will last beyond three years. The Newtonian telescopes now made abroad have very much smaller flats as prisms than in England. The prism is, of course, an inch or more closer to the focus, and the eyepiece is a compound microscope.

The President. I had omitted, when speaking of the reflecting-power of silver and speculum-metal, that the 2 to 1 would only actually apply when the silver was chemically pure. I do not know whether Mr. Grubb has published the method which he is going to bring out for an improvement in the reflecting-power of speculum-metal.

Mr. Common. He hopes to get it 25 per cent. brighter. As to the observations of Lord Lindsay on the relative power of the refractor and the reflector, it does not matter if you put the silver and glass at 30 inches or less. As to the difficulty which Lord Lindsay speaks of, of getting disks of optically pure glass for his 15-inch, and the difficulty which Grubb found in getting disks for the 27-inch refractor that he is making for Vienna, this does not apply to the reflector, because, as you all know, the optical purity is of no account in a reflector. We only want homogeneity and the requisite thickness. As to the reflecting-powers of speculum-metal and silvered glass, I have taken them at what Herschel quotes them at; and making every allowance for this 25 per cent. increase in the reflective power of speculum-metal, and that the chemical deposit of silver is not so pure as pure silver, we have still a large balance in favour of the reflector of silvered glass. As to the process of depositing platinum or any other metal on glass, I know nothing about it; but with regard to the supporting of disks, what I should suggest would be the same principle that I have found successful with an 18-inch.

As to what Mr. Lecky said about the difficulty of annealing such a thickness of glass, I do not say that the difficulty cannot be got over. I do not see any greater difficulty in annealing glass than in annealing speculum-metal. I do not know why the 4-foot telescope in Paris has failed; but there is a disk now on sale which was in the Exhibition, and it is about 50 inches in diameter and 8½ inches thick; and at any rate that has not blown to pieces, for it is there and it is polished. Mr. Calver of Chelmsford and I were cutting a hole through a large disk, and when we had cut through about ½ an inch it blew up. Since then I have had another disk made. From the facility with which silver can be deposited on glass there is no necessity for a hole through; it is only necessary to filter the solutions, and pour them on the glass mirror face upwards. My mirror was silvered in this manner last year, and it is perfectly good now. I know that silver deposited is permanent to an astonishing degree. When I first used this 18-inch telescope I found every morning there was water standing on the speculum, actually running down in drops; towards mid-day, to my surprise, this water cleared off, and towards night there was none at all. I made a tin cap to fit closely over the mirror, and since then I have never been troubled with a symptom of dew. As to the relief of friction by mercury, of course one never gets the truth in this world, and of big telescopes especially. I do not know whether one half of the astronomical world knows it; but there has hitherto been great difficulty in moving large telescopes, and to overcome that difficulty it struck me that by the use of means at hand the friction might be so relieved that we might place the larger telescopes on an equality with the smaller. The idea occurred to me some eight or ten years ago, but I had not an opportunity of putting it into practice till 1875; and since then I have proved that it is possible to mount a refractor or a reflector in such a way, by having the polar axis large enough and floating in mercury, that the weight is taken off from both the top and bottom pivots to whatever amount you like, thus bringing the whole mass of the moving part under command, in the case of a mounting I am now making to the extent of 1¾ ton. Every one knows how easily a heavily-laden barge is shoved along by one man, and the beautiful smooth motion that is given to it. Something very similar occurs with this motion, when, instead of friction-wheels, you use the floating-power of a fluid like mercury. Mr. Lecky spoke of a glass of 2 feet of 2 inches in thickness. This thickness happens to be the limit of the thickness of commercial glass, and that is why manufacturers will not willingly undertake any thing thicker. As soon as you get to any thing thicker, the amount of heat that has to be brought to bear upon the glass destroys their beds, and they object to do it.

Mr. Brett. Would Mr. Common kindly tell us how much a ton of mercury costs?

Mr. Common. I said a ton and three quarters of lifting-power. In the case in point we have a cylinder 6 feet long and about 11½ inches in diameter working in another cylinder. I allow a thickness of something like $\frac{1}{16}$ inch between the two. I ordered a bottle of mercury, the cost of which comes to something like £6 or £7, and about two thirds of this was used to float the whole of the concern, so that that is not really a serious matter. Of course you may extend your cylinder to any size. Mr. Neison remarks that he has had silver on glass for the last ten years; the utmost limit that I have used a speculum without resilvering has been about 16 or 18 months, and that was through ignorance as much as anything, because I did not know that it could be repolished with such good effect, as I have learned since that it can; but I have not the slightest doubt that the life of a film of silver, properly deposited and polished carefully, extends to at least three or four years—that is quite as long as any speculum-metal would last.

Mr. Neison. I am not prepared to say which is the best method of silvering a mirror. The one which I found best was by the use of formic acid or tartaric acid. I said I thought a silvered mirror would last ten years; but I have never had one myself for more than about five or six years.

Mr. Lecky. The Silvering Company at Newington Butts used tartaric acid in silvering, and they found it gave the best deposit of film.

Major Tupman communicated his observations of Brorsen's comet.

He found that the comet, which is about as bright as a 7-mag. star, was half a degree south of the place indicated by Schulze's ephemeris.

Mr. Plummer. I found it quite possible to observe the comet on three nights, one of them the 4th of April. I found not the slightest difficulty in fixing upon a very distinct point; and I expect that the observations, when they are reduced, will be very accurate and satisfactory.

Mr. Markwick exhibited a diagram for the purpose of tabulating sun-spots, to show the increase and decrease in the frequency of spots.

Mr. Freeman. It is very useful to have this mode of representation of the frequency of the spots; but it seems to me that the question of the area of the spots is a very important consideration. The mere frequency is of very little concern; but the total magnitude is of very great concern in estimating the amount of solar photospheric disturbance; and the question I was going to ask was, what actual practical method was employed in ascertaining, with any degree of approximation, the total area of the group of spots.

Mr. Markwick. I did not say that I had laid down a curve representing the area of the spots. I only said it might be done very nicely on those lines. Of course it would be a work of enormous magnitude, and I have not attempted as yet to work on the area of the sun-spots.

Mr. Ranyard. The advantage of Mr. Markwick's method is that in one diagram you are able to show three forms of variation,—the frequency of spots occurring in a particular period, their size, and the way in which they are broken up. It may not happen that the greatest complication of groups happens at the same time when there is the greatest sun-spot area. In fact there are three different phenomena to observe and tabulate; and this method, if accurately carried out, would enable an observer to show in one diagram those three forms of variation at one view. I think the area of the sun-spots might be measured, as I believe has been attempted, by superposing small squares upon the spot, and counting the number of squares which fall within the nucleus; and besides the area of the nucleus it might also be an advantage to tabulate, and to give, a curve showing the penumbral area, which appears to be different according as the spots are complicated or not. The more complicated the spot, the larger the proportion of penumbral area. Whether this area varies with the number of spots I do not know.

Mr. Christie. Mr. Ranyard's suggestion was long ago carried out by Mr. De La Rue, and is, in fact, his recognized method of measuring sun-spots. There is no novelty whatever about it, the umbra and penumbra of spots having been measured and tabulated for many years.

The President. Mr. Ranyard did not offer that as a suggestion; he merely said that it would be convenient.

Mr. Common read a paper on the desirability of photographing Saturn and Mars at the next conjunction.

Mr. Ranyard. Those who have had an opportunity of examining the photographs made by Mr. Common will know the very great amount of sharp definition which they contain. I have taken a few, but have not succeeded in getting any thing nearly so sharp as he has done. It strikes me that possibly this may have some considerable physical interest at some future time, especially with regard to the planet Jupiter. There seems to be evidence that the colours of the equatoreal belt of Jupiter change. Whether in connexion with the sun-spots I do not know; but I think there is some evidence that they do. Sometimes the equatoreal belt of Jupiter appears to be lighter than at other times; and if photographs were taken at different periods the actinic power of the rays from the equatoreal regions might be compared with the rays from the polar regions.

Mr. Brett. May I ask Mr. Common what plates those were which he used for photographing Jupiter and Saturn? Were they dry plates which were so very quick?

Mr. Common. They were Mawson and Swan's.

Mr. Ranyard. The question is whether they are quicker than the wet plates for this purpose.

Mr. Common. Undoubtedly.

The President. I think in the transit of Mercury in May last year we found Mawson and Swan's plates far too quick for any work that we had to do.

Mr. Common. With regard to the question of the different actinic effects, I took several plates of Jupiter with a plain iodized collodion (that is to say, with a collodion that was less sensitive to the red rays which form those bands), and got a picture showing two perfect bands, more particularly visible in the photograph than with the eye; whilst with dry plates, which are acknowledged to be more sensitive to the red rays, the bands were not nearly so distinct. If astronomers will take photographs of Saturn and Mars together, with those plain iodized plates, and also with plates which are more susceptible to the red rays, they will, by comparison with the effects on those plates, afterwards be able to detect where the colour is. There will certainly be some result from it. I will not say what. I have laid on the table one plate of each kind, and you will see at once the remarkable difference there is between the two photographs.

The President. Mr. De La Rue, who first introduced me to astronomical photography in September 1870, took at that time a series of photographs of Jupiter with iodized cadmium collodion. Instead of the two blank zones, which Mr. Common describes, with a segment on each side and a belt in the centre, there was in these photographs no central belt, but merely one large blank zone with a segment on each side.

Mr. Common. I think the difference in colour may be attributed to changes in Jupiter. I think it would be very interesting if photographs were taken at different times; you would get more information about the changes that are taking place than by eye-observations.

Mr. Ranyard. You would want some constant to compare it with.

Capt. Noble. I am not a practical astronomical photographer, but I presume that the photographs are taken in the primary focus of the telescope.

Mr. Common. Yes.

Capt. Noble. But magnified by the eyepiece.

Mr. Common. No.

Mr. Ranyard. I am not aware that anybody has yet succeeded in taking them with the eyepiece. In the photographs which I saw of Saturn the different brightness in the ring and in the ball was very beautifully shown.

The President. In one that I have seen the ring is visible to the naked eye.

The following papers were announced:—

J. L. E. Dreyer. An apparently New Variable Star.

H. Sadler. Note on his Paper published in the January Number of the 'Monthly Notices.'

A. Marth. Ephemeris for Physical Observations of Jupiter, 1879.

A. A. Common. Notes on large telescopes, with suggestions for mounting reflectors.

A. A. Common. On the desirability of photographing Saturn and Mars at the next Conjunction.

A. A. Common. Note on Mr. Sadler's paper in the January Number of the 'Monthly Notices.'

Dr. A. de Gasparis. On some Formulæ for expressing the value of the Excentric Anomaly &c. in functions of the Mean Anomaly.

The Meeting adjourned at ten o'clock.

Schmidt's Lunar Map.

[Continued from vol. ii. p. 415.]

WHILE the author, like Lohrmann and Mädler, adopted a modified style of Lehmann's method of delineation, it appears that the various declivities of the hills are not represented with angles of inclination so true as they might be. To explain this Dr. Schmidt says that he knew that in process of time his sketched shadings would become faded; and as he was determined, since 1868, to have the work photographed, it seemed necessary to have faint heights and ridges in the *Mares* much more strongly marked than their real steepness required.

Hence the author explains that this chart does not give the correct impression of the individual characters of the lunar mountains which, he admits, are better represented by Mädler. He could easily have made his drawings nearer the truth if he had intended to have the work lithographed; but with photography it was different, and his object was that every feature should be sharply delineated.

The correct representation of the several forms of lunar mountains belongs to a higher department of art, and can have been hitherto but in few instances attained. With all his industry Lohrmann failed in this design, while he made the ridges too broad, and almost entirely misrepresented the character of the crater walls. Mädler was more fortunate, and his chart contains remarkable illustrations of individual forms. In Schmidt's chart the shadings present a general uniformity, and the desire to show characteristic differences was sacrificed on account of other considerations. Lunar drawings require for sufficient exactness several repetitions; and to spend the necessary time for a close portraiture of the various objects in a map like Dr. Schmidt's was obviously out of the question.

With regard to the colouring of the Moon, to show it properly in a topographic chart is impossible; and Dr. Schmidt, like his predecessors, confined himself to representing the grey tints of the flats and several dark spots. Light-rays he shows only in the *Mares*, and light-spots where they appear within dark areas. By photography alone we can fairly picture the full Moon, where the endless modifications of brightness are beyond the art of the draughtsman. The mountains about the limb occupied Dr. Schmidt's attention less than the interior formations; and with regard to the former, his map appears as defective as the maps of Lohrmann, and Beer and Mädler, though it is in reality more comprehensive. When the selenographical positions of the limb-mountains are accurately known, they can then, more properly than at present, be included in some future general chart.

Dr. Schmidt adopts the positions of the first and second order from Lohrmann, but he has laid down the intermediate detail altogether from his own measurements. He has excluded all written names from the chart, using only letters and numerals instead; but the nomenclature, old and new, is found in the descriptive text. In this he has omitted all mathematical explanations and formulæ such as appear in the works of Schröter, Lohrmann, and Mädler, as he regards it as essentially no other than a descriptive volume and an index to the objects that he has delineated. He explains, however, the methods of measuring lunar heights; and an original expedient of his own, where, for want of a micrometer, he measured by *time*, appears especially ingenious, and was well applied in those cases where the shadows ran parallel with the Moon's motion. Her increase in R.A., which would obviously affect the angle deduced from the time, was, of course, taken into account, and conveniently found in the duration of her meridian passage as it appeared in the 'Nautical Almanac.' The heights ascertained by this method number 109 in his list, while he records no less than 2894 as taken with the micrometer; and it may be added that for these results no less than 57,000 observations were made from 1853 to 1858, which, of themselves, would be sufficient to give a fair idea of the untiring patience with which Dr. Schmidt pursued his immense task.

There is no sea-level in the Moon from which to measure the mountain-heights, and the datum-plain must be found in the low lands adjacent. In the method of measuring by shadows, the land at the end of the shadow is taken as level with the base of the mountain. The height is then simply the difference between the land referred to and the summit. The altitude of the Sun above the horizon of the mountain is, of course, a chief factor in the question; and though this may be ascertained by calculation, it is not found practically much more advantageous than the method of directly measuring the distance from the terminator. The uncertainty, however, of defining the true terminator on a broken surface like that of the Moon must then be regarded as presenting a serious obstacle in such measurements; and while these seem sufficiently difficult even in the *Mares*, it is not easy to see how they can give much hope of a fair approximation in other districts where the serrated outlines of the grouped hills rise in all confusion from the darkness. Mädler recommends those measurements to be taken when the distance of the terminator from the mountain is from 10 to 12 times the length of its shadow, as the uncertainty of the terminator becomes less important according as it recedes from the object. Penumbral effects must render the definition of the true shadow less certain; and altogether it would seem necessary to leave a wide margin for error in those measurements of lunar mountains. Indeed this view seems justified by the various measures of the same objects over and over again, surveyed with inexhaustible diligence by Dr. Schmidt. The differences are startling; and as examples may be given the mountain Huyghens, where one measure gives double the height of another, and Curtius (of which Dr. Schmidt took no less than 80 measures), where the height by one measure over another is more than quadrupled. In cases of such extreme differences, a mean can scarcely be much relied on.

Mountains, when precisely on the limb, can be measured by their addition to the angular diameter of the Moon at the time of observation. Whatever this diameter is calculated to be, the measure will give the height of the mountain above it. It is plain that a position within or on the other side of the limb must subtract from the apparent height, and great care on this account is necessary. Some of these mountains are of great size, and may be compared to the loftiest of the Andes, or even the Himalayas. Yet their heights over the valleys are not so considerable as the height of the mountains of the earth above the depths of the ocean, though the difference seems not very great, as Schmidt found examples, along the eastern limb and at the south pole, where the hills rose about 6000 toises, or over 38,000 English feet, above the adjacent hollows, and Schröter measured heights of 4000 toises and depressions of 3000, showing a difference of level equal to near 45,000 English feet. Yet the aspect of those limb-mountains is not imposing. Only one of them might be compared in form to Chimboraço, while it was far from exhibiting the steepness of Cotopaxi and other volcanos of the earth. There are no striking elevations on the N.E. or S.W. limbs. On the N.W. there are some visible at times of favourable libration.

Dr. Schmidt gives a list of *light-rays*, including light-spots and craters surrounded with bright or dark *nimbi.* The bright borderings will be found to be formed of close and delicate rays, or, at least, they give rise to an efflux of rays that widen as they advance. The nimbus may be of a dark shade, like those surrounding Tycho, Aristarchus, and Dionysius; and it is easy to conceive that its light-reflecting qualities depend on the nature of the stuff ejected by the crater. Dr. Schmidt regards the nimbi as volcanic ash distributed, as in terrestrial eruptions, round about the orifice. The ash may be of various colours; but it is possible, and, in the case of *Linné*, very probable, that the deposited stuff was in a fluid condition.

When the light-formations are of small extent, they may not inaptly be referred to eruptions similar to those that are seen on the earth. Dr. Schmidt observed, in the innumerable eruptions of the Santorin volcano in 1866 and 1868, that bright-coloured pumice and light grey ash were deposited on the dark surface of the hill in most remarkable radial streaks; and, the matter being ejected on all sides, the streaks traced upwards coalesced into a uniform covering towards the summit, while lower down the hill they remained in separate lines of over 100 metres in length and from 2 to 10 metres wide. Here they consisted of the coarser materials, which rolled towards the foot of the mountain. Thus the crater, looked down on from a certain height, would appear surrounded by a bright nimbus, from which issued bright rays or streaks all round.

But who could find analogies between such phenomena and those of lunar volcanos, from which, uninfluenced by hill or valley, the light-rays extend for hundreds of miles, and even, like those from Tycho, to a distance equal to a fourth of the lunar meridian or more? Even recollecting the far inferior gravitating power at the Moon's surface, and admitting a much greater eruptive force, still much would remain unaccounted for, especially, among other matters, the excentric direction of many of the rays, which, traced backwards, run past on one side of the place from which they must have issued. And to this might be added other anomalies, such as the bendings and the interlacings of the lunar streaks like those from Tycho, Kepler, and Copernicus. It has long been known that the rays run indifferently over hill and valley, that they are visible over a bright surface, that with a low Sun they are not apparent, and that they throw no shadow; but to observe them closely would require many years' study with powerful means. This has never yet been accomplished, and, instead of profitable observations, we seem content with hypotheses. Only in the 36th year of his observations did Dr. Schmidt devote some evenings to these features, and then only to make himself acquainted with the principal ones.

Many remarkable forms observed by Schmidt are not found in Mädler, who, on the other hand, has noticed some not seen by the former. With high powers a vast number of minute light-spots may be found, and in many of them, as in Linné, are little craters. It may be conjectured that here are the most recent of lunar formations, and that changes on the surface are by no means to be classed among the most unfrequent phenomena. The great ray system must be mapped in detail, if in future times it may be ascertained whether changes occur there or not. It may be considered unlikely that any will be found; but to give up the search on account of such settled opinions, would be only yielding to a deplorable dogmatism, which, under the appearance of critical strictness, has been of little advantage to science.

Dr. Schmidt gives the positions of the light-rays, classed according to the 4 quadrants of Beer and Mädler's map. He declares, however, the incompleteness of his list, which may be easily improved and enlarged by future observers.

The *Rill* formations seem specially to have attracted the attention of Dr. Schmidt, who in 1866 published a catalogue of no less than 425 of those objects. In the present work he includes new observations of many of them, and describes several not previously noticed.

The effects of the earth-light on the Moon, or "*lumière cendrée,*" are described by Dr. Schmidt in a series of interesting observations. None of these served to show the existence of a lunar atmosphere. He devotes a long description to the crater Boussingault when just reached by the terminator. A certain illumination of the interior should be referred either to the refractive power of gaseous matter within, and reaching above it, or to the earth-light in addition to the direct sunlight striking the inside of the wall and reflected to the crater floor. Dr. Schmidt does not hesitate to adopt the latter explanation.

In comparing Lohrmann's sections with Mädler's list of positions in 'Der Mond' several differences will be noticed; and Dr. Schmidt seems always to agree with Lohrmann. In section XIV., for instance, the differences in the places of *Mason*, *Plana*, *Eudoxus*, *Democritus*, and *Burg* amount to about 2° both in latitude and in longitude. In Plana, indeed, there seems to be a difference of no less than 12° in latitude; but there is an evident misprint of 53° for 43° in Mädler's list. In Eudoxus there appears a difference of some 5° of longitude.

It is well known to observers how an increase of power will alter the aspect of a lunar formation, breaking up into separate parts and irregular outlines what appears with inferior means as a single, sharply defined mass. For this reason it might be considered that the objects in Schmidt's map might be more difficult of identification at first sight than those in Lohrmann's; but it is quite the other way; for, notwithstanding the cutting up by a profusion of detail, the forms, as they appear with moderate powers, are, in general, more speedily recognized in Schmidt's delineations. Take, for instance, the remarkable district in Section XVI. comprising Herschel II., Horrebow, South, Babbage, Pythagoras, Anaximander, &c. In Schmidt these are all identified at a glance, while in Lohrmann they are by no means so strikingly distinguished. In Lohrmann, and also in Beer and Mädler, South and Babbage are given very imperfectly. In Schmidt's map it may be observed that the deep shading of the central ridge of Anaximander renders the eastern boundary less prominent than it appears in the telescope, and, indeed, Dr. Schmidt seems to confine the name to the western side of the ridge. Horrebow appears but as a single crater in Lohrmann, while in Schmidt it is nearly divided into two. It is composed of two in fact, with a low dividing lamina, seen only under favourable circumstances. The very remarkable double interior hill in Pythagoras, which has not been overlooked by Schmidt, appeared only single to Lohrmann, and so also to Beer and Mädler.

It may here be noticed that several remains of ancient craters, which have been more or less obliterated by newer formations, seemed to have escaped both observers, though some of them are more or less traceable in the maps.

In Section XXIV. a large ancient crater including Fabricius and Argelander, and extending a long way to the east, is better defined in Schmidt than in Lohrmann; but the latter shows more distinct traces of two similar craters in Section VIII. The more southerly of these, extending from *Lexell* on the south, goes to *Hell* on the north-east, and quite includes that crater. It is bounded on the S.E. by a line of small craters given by both Schmidt and Lohrmann, and reaches Walter and Regiomontanus to the W. and N.W. Another ancient crater reaches from Purbachius to the northern end of "Straight Wall," which runs nearly down its centre. Thebit lies within it on the west. Both these are of great size, *like all the primitive craters*, and, though not equalling those like the *Mare Crisium*, they are far larger than Hipparchus. A third smaller, situated between the two, cannot be traced in either Lohrmann or Schmidt; but it may be noticed at the short periods of illumination favourable for observing such wasted or nearly obsolete features.

The incipient formation of rills by inosculating craters may be well seen in Schmidt's map. In Section III., about Römer and Posidonius, are found several good illustrations of crater rills; and in Section I. the single cleft of Hyginus shows, in its varied character, the relationship between rills with craters and those where craters are not distinguished.

Schmidt has added to his map a convenient epitome of the descriptive volume; and a valuable adjunct will be found in 'Der Mond' of Beer and Mädler, as their letters of reference are given by Schmidt in his sections. It must be said, however, that the letters are often obscure and not readily deciphered—a fault, perhaps, of the photography.

It is to be regretted that Schmidt does not give a list of positions, like Mädler in § 37 of 'Der Mond,' with the addition of references to the sections in which the objects are to be found; neither does he give any comprehensive index. His list of points of the first order falls far short of ordinary requirements; nor are even these easily found in the map without any indication of the sections in which they appear. A small index map showing the sections, and containing the most prominent features with the names, would be also very desirable; and, in its absence, it may be suggested that a spare copy (say from an old edition) of Webb's excellent little map in the 'Celestial Objects' can be well utilized as an index by dividing it into sections corresponding with those of Lohrmann and Schmidt, and adding with pen or pencil the names to a selection of the principal formations.

In even a cursory examination of Schmidt's map its completion by a single observer must seem almost incomprehensible to a man of ordinary powers; but it requires protracted study to well realize the extent of the work. Any person who tries with the aid of a 6-feet telescope to give a closely detailed delineation of even a small area of the Moon, will soon conclude that the period of thirty-three years was comparatively a very short one for the accomplishment of Dr. Schmidt's great task. It is, in all truth, a performance highly creditable to the age in which we live, and to Teutonic intellect and perseverance. It is a splendid example of great results from small beginnings. We have, first, the astronomer, as a youth of 14, viewing the Moon with a little telescope steadied by a lamp-post, and probably the laughing-stock of many a passer by; afterwards he is found, in maturer years, pursuing his favourite study under more or less difficult circumstances and in different countries, until, at length, as director of a national observatory, he completes the wonderful production of his truly inimitable labours. For this it required all the unswerving persistence that is ever a chief attribute of genius; and the pages of the 'Astronomische Nachrichten' and other scientific publications can testify to the large amount of other astronomical work performed by Dr. Schmidt simultaneously with his lunar researches.

It is to be hoped that his map will soon be well known and justly appreciated in this country. To every person interested in lunar topography it is certainly indispensable; and the observer who sees something in the Moon not shown by Lohrmann, or Beer and Mädler, may be saved from imagining a new discovery by finding that the object is represented by Schmidt. This may be easily understood when it is known that the number of craters shown by

Lohrmann is 7177, and by Mädler 7735, while in Schmidt's map they reach the extraordinary amount of 32,856. The rills number in Lohrmann 77, in Mädler 71, and in Schmidt 348. As regards other objects, such as hills, ridges, banks, crater-terraces, &c., Schmidt's map equally surpasses the others in comprehensiveness. Still, of course, the survey cannot be considered exhaustive even within the range of a 6-feet telescope; and although, by referring to Schmidt's map, an observer may find an object which otherwise, by its absence in other maps, he might be led to suppose was new, it would be plainly wrong to infer that a formation must necessarily be recent because it is not noticed even by Schmidt. In a work of such magnitude as his it would be manifestly senseless to imagine that even sufficiently obvious objects might not, in some instances, have escaped him. With a telescope not superior to the Athenian refractor I have observed a few things not indicated by him,—such as a second large crater in Gruithuisen's Schröter, a dilapidated diaphragm dividing Endymion into two, &c.; I should, however, be far from concluding on that negative evidence that they were not old formations. I believe the instances of Schmidt's oversights of rather obvious objects to be wonderfully rare; but at the same time, it is, I should say, in change of aspect in what he represents, rather than in the detection of something not shown in his chart (unless something very striking), that we are to seek for sure indications of continued action affecting the topography of the Moon.

J. Birmingham.

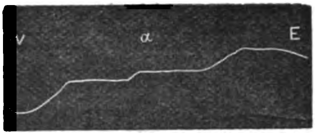
The Landslip in Plato.

The subject of changes occurring on the Moon's surface having of late attracted much attention, it may be desirable to place on record, in the pages of the 'Observatory,' an account of a landslip in the interior of the wall of Plato which was noticed by the Rev. T. W. Webb in the year 1866.

At the eastern extremity of the wall of Plato is a conspicuous rock marked ζ by B. and M., and immediately south of it is a triangular formation (easily seen between nine and ten days of the Moon's age), which has been regarded by the Rev. T. W. Webb as a landslip; under date 1866, April $25^d\ 11^h\ 0^m$, he recorded in his journal (with a perusal of which he most obligingly favoured the writer in 1868) the following:—"Plato. I never recollect having noticed before the curious appearance of an irregularly triangular landslip of considerable size from the east end of the wall towards the interior, divided from the rest of the wall by a bright crack of some width."

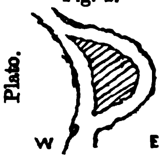
Again in 1867, Nov. $6^d\ 9^h\ 0^m$, $2^d\ 6\frac{1}{2}^h$ after 1st quarter, Mr. Webb records:—"The well-known triangular irregularity of outline in the south-east side of the ring is clearly a landslip towards the cavity, being bordered throughout by a white streak; it is also subdivided by a very narrow white line from north to south, showing that the west part has sunk lower than the east, and indicating some such section as this (fig. 1)." On Nov. 17^d 18^h 30^m, 10½ hours before last quarter, Mr. Webb writes: "The landslip is now cut off by a narrow black shadow where the light was before, and the narrow step (*a* in section) down from one portion to the other is marked by a very minute black line." On this the writer has the following note:—"Mr. Webb does not say on which side, east or west, the narrow black shadow is situated. If the triangular depression were in the same state as when I observed it in 1860, May 8, two shadows as well as the minute line should be visible."

Fig. 1.



In connection with this landslip the writer of this article forwarded the following communication to Mr. Webb:—"The difference between us appears to be in the dotted portion on the west rim of the depression formed by the landslip. In the printed notice (Report British Association, 1861, p. 185) I speak of the shadow of the curved rampart having been seen within the depression. I have looked for the original observation, but cannot lay my hand on it at present. I do not mention the side of the rim from which the shadow was cast. I have, however, an observation, under date 1860, May 28 8^h 15^m to 10^h 10^m, which is strongly indicative of the depression being enclosed on *all* sides by a rampart. The record is as follows, with sketch in margin (fig. 2):—'Moon's age 8·11 days, east exterior [of Plato]. A triangular marking formed of two projecting ridges of mountains; *the floor in shadow.*' At the same epoch the record of the state of the floor of Plato is as follows:—'Between 0·25 and 0·20 of the area on the western side in deep shadow; the outline of the shadow irregular. About 0·25 from the south extremity a peak (shadow of the mountain γ), with an indentation on the north indicating a depression, probably the depression through which the stream of light was thrown in Bianchini's observation of August 16, 1725. About 0·6 also from the south extremity, another peak (shadow of δ), succeeded by another indentation, the remaining portion of the outline of the shadow irregular.'

Fig. 2.

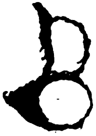


"From the two quotations it would appear that in 1860 the west boundary of the enclosure was elevated sufficiently above the floor to hinder the Sun from shining on it when his altitude was low.

"Now comes an interesting question. According to your section, there is nothing whatever to impede the light as the Sun rises upon the 'landslip;' and the fine white streak under the morning illumination strongly indicates the difference of level of the eastern and western portions. Was this the state of things in 1860, when my observations of the physical aspect of the crater Plato were made? If so, it is very curious that I should get a strong indication of a west rim to the landslip. If not, then we have another instance of probable change. The appearances which you describe are much more in accordance with a landslip than those which I describe. At the time of my observation, the terminator was about 0·75 of the length of Plato from the eastern wall of Plato. I think that it is not unlikely that a change has taken place in the western half, especially as I had some strong evidence that a landslip occurred on the south-west border, probably about 1862."

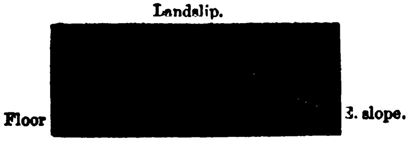
1868, Sept. 6^{d} 18^{h} 45^{m}, Mr. Webb examined this formation; the following is the record of his observations:—"In consequence of Mr. Birt's remarks (1868, July 15) I examined the landslip under the present opposite illumination; but the Sun had risen and the light was faint. The shadow clearly showed that the central part of its edge was not lower than the unbroken crater-edge S. of it; but that it ran down towards each end, and that the slope of its top was from the interior, as Birt asserts, seemed evident; but there was no trace of any ridge or bank along its edge, the mark in the sketch being the shadow of the longitudinal step upon it. The only place in the neighbourhood where such a bank could be traced was on the N.W. side of the projecting bluff ζ, which evidently overtopped all the neighbouring heights. It had a terrace on its S.W. face."

Fig. 3.



1868, Oct. 7^{d} 18^{h} 0^{m}. The writer has the following record:—"This formation is well situated for observing its character under the evening illumination; its appearance is given in the engraving fig. 3." Its section, so far as he could make out, is as in the engraving fig 4. Compared with Plato its depth is very shallow; but its surface slopes outwards.

Fig. 4.



After a lapse of 11 years since Mr. Webb's observations were made, and more than 18 since the writer first recorded his, this object presents advantages of no ordinary character for further investigation. The points requiring further elucidation are:—1st, the appearance as recorded in 1860, viz. a depression enclosed by comparatively low ridges, fig. 2; 2nd, if not, has the western ridge disappeared, as indicated by Mr. Webb's section (fig. 1)? and, accordingly, which of the two series of observations does the object at present most resemble, or is its appearance not in accordance with either? Observation only will answer these questions, and we hope that early attention will be directed to the eastern wall of Plato. W. R. Birt.

Double Stars for May.

The principal objects between 9^{h} and 10^{h} are as follow:—

Σ 1334. R.A. $9^{h}\ 11^{m}\cdot4$, Dec. $+37^{\circ}\ 19'$.

Mag. 4, 6·7. Colours, A greenish white, B blue.

A physical pair.

1876·3 237°·0 $2''\cdot82$ Wilson and Seabroke.

Σ 1338. R.A. $9^{h}\ 13^{m}\cdot4$, Dec. $+38^{\circ}\ 42'$.

Mag. 7, 7·2.

Binary: in rapid motion.

1876·3 151°·8 $1''\cdot63$ Dunér.

Σ 1356 = ω Leonis. R.A. $9^{h}\ 22^{m}\cdot0$, Dec. $+9^{\circ}\ 35'$.

Mag. 6·2, 7.

A very close binary. Doberck's elements (1877) are:— ☊ 148°·7, γ 64°·5, λ 121°·4, e 0·536, T 1841·81, a $0''\cdot890$, P $110^{yrs}\cdot82$.

1877·36 76°·6 $0''\cdot41$ Dembowski.

Σ 1374. R.A. $9^{h}\ 33^{m}\cdot9$, Dec. $+39^{\circ}\ 30'$.

Mag. 7, 8·3. Colours, A yellowish, B very blue.

Binary.

1872·81 284°·1 $3''\cdot11$. Dunér.

Σ 1377. R.A. $9^{h}\ 37^{m}\cdot2$, Dec. $+3^{\circ}\ 11'$.

Mag. 7·9, 11·1.

Probably binary.

1873·24 136°·8 .... Wilson and Seabroke.

Dembowski's distance in 1867 was $3''\cdot58$.

Σ 1385. R.A. $9^{h}\ 43^{m}\cdot 4$, Dec. +17° 8′.

Mag. 8·5, 10·7.

A difficult object. Probably in slow retrograde motion.

1863·5	351°·0	1″·10	Dembowski.

OΣ 208. R.A. $9^{h}\ 43^{m}\cdot 9$, Dec. +54° 38′.

Mag. 5, 5·5.

A binary in rapid motion. An occultation.

1873·4	96°·6	elongated	OΣ.
5·5	115°·0	„	„

Σ 1389. R.A. $9^{h}\ 45^{m}\cdot 5$, Dec. +27° 33′.

Mag. 8·9. Probably binary.

1863·6	316°·7	1″·99	Dembowski.

Σ 1386. R.A. $9^{h}\ 45^{m}\cdot 6$, Dec. +69° 28′.

Mag. 8·2, 8·2. A binary in slow retrograde motion.

1875·6	294°·0	2″·1	Gledhill.

A.C. 5 = 8 Sextantis. R.A. $9^{h}\ 46^{m}\cdot 6$, Dec. − 7° 32′.

Mag. 5·6, 6·5. Rapid binary. An occultation.

1877·30	round	....	Burnham.

JOSEPH GLEDHILL.

Meteor Notes for May.

EVENING observations may be made between the 7th and 25th, and morning observations during the last half of the month. On the first few days before sunrise a shower of *Aquariads* (326° − 2°) should be watched for, and at about the middle of the month there is a rich display of *Virginids* from a centre at about 207° − 8°, to which the brilliant fireball of 1878 May 12, at 8.53 P.M., probably belonged. A shower of *Draconids*, just S. of *o*, was observed by Zezioli on the 25th in 1867–8, and by the writer on the 17th in 1877; and there is a radiant at 354° +41°, visible in the mornings towards the end of the month, which has hitherto been but slightly observed. Mr. Greg, in his 1876 catalogue of meteor-showers, gives the following chief positions for May:—280° +34°, 310° +24°, 204° +56°, 289° +81°, 235° +45°, and 123° +40°. Large meteors should be expected on the 2nd–4th, 15th–16th, and 31st.

Recent Observations.—Several accounts of what appears to have been a detonating fireball have come to hand. Mr. Cornish received a notice of it from Haverhill as follows:—"On Feb. 21, at about $12^h\ 15^m$, there was a splendid illumination over this town, it being cloudy at the time. I at first thought it was a flash of lightning, but it continued too long for that. The brightest part was in S.E., and I should think it lasted 5 seconds or more. About ½ or ¾ minute afterwards there was a sound like thunder, which began apparently at 45° altitude and went rumbling down to N.W." The same phenomenon is described in the 'Bury Free Press':—"On Friday night (21st Feb.), a little before midnight, a very brilliant meteor was seen in the west. It moved slowly in a downward direction like a ball of fire falling to the earth, lighting up the sky vividly in its descent. A report was heard at the same time like the sound of an explosion." This detonating meteor is evidently different from the one described, in the 'Observatory' (p. 417) for April, as having appeared shortly before 3 A.M. on Feb. 24.

On March 2, $9^h\ 40^m$, Mr. Cornish at Debenham observed a first-mag. meteor, with a path from 184° +63° to 18½° +58°. It burst 12′ *n.p.* δ Ursæ. On March 3, $16^h\ 30^m$, a meteor (1½ mag.) traversed a short course of about 4° in Hercules, from 261° +38° to 259° +34°. If its path be traced backwards, it cuts the 50th parallel of declination at 269°, so that it may belong to the Draconids I. at 263° +50° (Greg, No. 47). On April 11 a watch was sustained by Mr. Cornish from $8^h\ 20^m$ to $12^h\ 15^m$, with the exception of a short break a little before 9^h, when it clouded over. 12 shooting-stars were seen, none of them very bright. At $9^h\ 40^m$ a faint stationary meteor was seen at 75° +45½° (close to α Aurigæ). This star is a well-observed point of departure of many showers occurring between August and February, but it had not been previously recognized as a probable radiant of April meteors, except perhaps by Heis. Mr. Cornish also registered two shooting-stars, with paths much foreshortened, close to a centre at 55° +75°, which afford indication of another new shower.

Mention is made, in our last month's "Notes," of the dates April 11–12 as a notable epoch of fireballs. Both nights were cloudy this year at Bristol, and the fact could not be verified; but the writer has just concluded the projection of more than 700 shooting-stars seen on April 9–12, chiefly by Zezioli and other Italian observers, in the years 1867–72. The major showers derived from this investigation are at 249° +51° (on the northern border of Hercules), 184° +59° (at δ Ursæ Majoris), and at 212° +65° (α Draconis), each indicated by about 45 meteors. There is also an apparently diffuse radiant in Coma (37 meteors) and others in Boötes, Corona, and Hercules, one of which, in the latter constellation at 257° +37° (25 meteors), is very well defined, and agrees with the showers (Nos. 49, 50, 60) deduced for April 2–23 by Schiaparelli from Zezioli's observations. W. F. Denning.

CORRESPONDENCE.

To the Editor of 'The Observatory.'

The Sadler-Smyth Scandal.

SIR,—

I attended the April Meeting of the R. A. S., in the confident expectation that something would transpire respecting Mr. Sadler's attack on Admiral Smyth. I was prepared to find an intimation from the Council that they had done something, whereas no announcement was made. If Parliamentary precedent had been followed, we should have been told either:—(1) That a committee to investigate the charges had been appointed; or (2) That to save trouble all round Mr. Sadler had resigned; or (3) That the Council were at sixes and sevens, and a majority or a minority had resigned. Any one of these three courses would have been in accordance with the manners and customs of public life and would in some measure have met the requirements of decency. But no announcement was made from the chair, and outsiders must presume that nothing is in contemplation on the part of the governing body, and that their policy is to whitewash Mr. Sadler at the expense of Admiral Smyth's reputation and the feelings of his family.

Is this to be tolerated? I say emphatically, No! unless the Fellows at large are to be deemed dead to all sense of honour and shame.

In language of the most unequivocal character (I will give him credit for that), Mr. Sadler has chosen to accuse the Admiral of wilful and wide-spread fraud and deceit, and has (inferentially) asserted that the Council who 30 odd years ago gave the Admiral a medal for his work were fools.

This being the situation, something must be done; the Council having done nothing, the Fellows must move.

At the moment of writing this I am not very clear as to what is the proper *modus operandi*, and I have not within reach any Fellows whom I can consult; but I would ask you to allow me to announce through your columns that at the meeting on Friday, May 9, I shall bring forward some such resolution as the following:—

"That this Meeting, having heard that accusations of wilful fraud, in connexion with the composition by the late Admiral Smyth of his *Cycle of Celestial Objects*, have been made by Mr. H. Sadler (a member of the Council), is of opinion that the Council are deserving of censure for not having taken steps to have these charges either proved or retracted, and calls for the immediate resignation by Mr. Sadler of his seat on the Council."

I wish to say as little as possible at this moment on the action of the Council, because that matter may be best discussed at the Meeting; but I think it well merits the consideration of the Fellows how far, even on general grounds, the present Council is a fairly representative body deserving of confidence. To my mind it is not such: an Astronomical Society ought to be governed by working Astronomers, and admission to the governing body should go in some degree by seniority; but where a man is selected who has no claims by seniority it ought to be because he has conspicuous claims by merit, or energy, as in the case of our present President. Mr. Sadler, having been elected Fellow in June 1876, has clearly no claims by seniority: as to his claim by merit, I need, under the circumstances, say nothing. By some kind of juggling, the mysteries of which are unknown to me, most of the influential Astronomers of England seem to have been juggled out of the Council and a batch of nobodies juggled in.

I am, Sir, your obedient servant,
G. F. Chambers.

Northfield Observatory,
Eastbourne,
April 18, 1879.

The Pulkowa Double-Star Observations.

Sir,—

I cannot endorse A. W. Downing's maxim, "My faith is large in means." He is, however, right in saying that "the tendency of modern astronomy is to admit more and more that the mean of results obtained by several observers is the most accurate that can be given, *assuming* that the systematic errors peculiar to each are thus eliminated." But it is far from being proved that this assumption is true.

Let us take, for instance, a very common observation, viz. the true place of a star in N.P.D. This brings us to deal with several unknown quantities:—The residual errors, A of collimation, B of azimuth, C of level; D, the exact nadir point, which my experience has shown to be variable; E, the error in latitude, which *may* depend on D; F, the true amount of refraction (a friend of mine has examined this point *ab ovo* and finds that *all* our tables are wrong); G, the flexure of the instrument which Löwy's apparatus observes in any angle, showing that it does not follow the law of sines; H, the personal equation, perhaps more variable than it is generally supposed; I, error of observation, so very hard to determine; J, authors of fundamental catalogues, when combining independent but conflicting results, must weigh them by the law of evidence, as careful historians do. K, to the foregoing ten must be added residual error brought in, perhaps, by more than one cause, at present unknown, and which may be pregnant with new theories. It is not safe to assume that an arithmetical mean covers and compensates exactly all these causes of uncertainty.

A mean is nothing more than a convenient summary of different results. Strictly speaking, a mean is an untruth unless when it agrees precisely with one of the combined numbers which help to form it; and even then that single one may be wrong. Conversely, every individual number used to eke out a mean may be right, for they cannot be all obtained simultaneously, and several causes, undiscovered as yet, may come into play from time to time. As chemists have often done, astronomers may elicit new ideas by sifting carefully residual errors.

Should you find something wholesome in my criticisms, I beg to add a word about the method of least squares. This device for furbishing up results, so much practised in Germany, was always condemned by Le Verrier. In many cases it may be compared to the outward and beautiful varnish of a clock, whose inner works are limp and faulty. Your memory can recall cases where subsequent truthful observations have shown the necessity of a correction exceeding greatly the probable error determined previously by long calculations. These are generally useless as regards the method of least squares; and I sincerely thank all observers who publish their several results even without taking MEANS.

I remain, truly yours,

Paris, 1879, March 26. ANTOINE D'ABBADIE.

The New Binary, 99 *Herculis.*

SIR,—

I have just read your April No., and hasten to send you a remark on "A new Binary," p. 420, which may perhaps interest your readers.

It does not yet appear certain that this double star really forms a binary system, although this conclusion is rendered probable by the proximity of the components. In fact this star is (see my Catalogue, p. 166):—Flamsteed 2474; Bradley 2278; P. xvii 385; Armagh 3686; Greenwich 1864, 2002; and the adopted Proper Motion is, in R.A. $-0^{s}{\cdot}005$, in N.P.D. $-0''{\cdot}08$; Motion in R.A. $\times \cos \Delta = -0''{\cdot}038$, Resultant $=0''{\cdot}088$, Direction $=334°$.

Now it would suffice that the distance measured by Mr. Burnham in 1878 should be a little less than he indicates, or that that of Alvan Clark in 1859 should be a little greater, for the relative motion of the small star to be precisely parallel and opposite to the proper motion of the principal star. It is true that it seems less; but if we remark that the proper motion of 99 Herculis is not very certainly determined, and that the determinations are not very accordant, we cannot help concluding that the observations do not suffice to prove this couple to be physical, and that it might be optical. The difference of brightness of the components (5 and 10 mag.) supports the latter conclusion, rather than the former.

I am, Sir, yours faithfully,

Paris, 1879, April 2. CAMILLE FLAMMARION.

Conjunction of Mars and Saturn, June 30th, 1879.

SIR,—

The January number of your valuable periodical has just come to hand; and I was much gratified to read your account of the discussion, at the Royal Astronomical Society's December meeting, which followed the reading of the Astronomer Royal's paper on the remarkable conjunction of Mars and Saturn for June 30th, 1879. I was the more gratified because the subject had been previously treated by me in a paper read before the Astronomical Section of the Royal Society of N. S. Wales on the 1st November last. The remarkably close approach of the planets, and the favourable position which we in Australia occupy for the observation of this important phase of the conjunction, were especially insisted on. According to my calculation for the geocentric phase, the nearest approach of centres would occur at $5^h\ 31^m$, Sydney civil mean time, in the morning of July 1, and the corresponding distance of centres would be about 71″.

I did not enter into the question of parallax, for it would have very little effect on the result. Its effect, as I pointed out, would really be to slightly increase the apparent geocentric distance; and the tabular errors, it now appears, would produce a much greater effect. Mr. Marth's remarks, in connexion with the satellite Iapetus, add no small degree of interest to the phenomenon. I trust those observers in this part of the world who are provided with telescopes of moderate power will be able to render a satisfactory account of the phenomenon.

I am, Sir, yours faithfully,

Observatory, Windsor, N. S. Wales, JOHN TEBBUTT.
1879, Feb. 20.

Solar Photography.

SIR,—

In No. 24 of the 'Observatory,' in the article "Visibility of the Corona in full Sunlight," the author adds that I consider "that photography is incapable of reproducing the *ensemble* of the phenomena during an eclipse, just as it cannot reproduce the granulations." This expression does not agree with what I have published; there is a word wanting. In fact, according to my article, 'Mem. degli Spettrosc. Italiani,' vol. vii. p. 147, the author should have concluded "that photography is incapable of reproducing the *ensemble* of the phenomena during an eclipse, just as it cannot reproduce the *ensemble* of the granulations." If the granulations are visible at the centre of the Sun's disk, as well as at the limb, that does not imply that photography is equally perfect in all the different parts of the disk. In fact I have convinced myself of this by an examination of the magnificent photographs of M. Janssen; and when at the Observatory of Meudon, in September 1878, I had the honour of offering similar considerations to the illustrious director, he answered that I was perfectly right, and he told me that certain exposures very suitable for the centre of the Sun's disk would be insufficient for the parts near the limb. You see, then, that my conclusions are not "without sufficient examination of the subject."

I am, Sir, yours faithfully,

Rome, 1879, April 10. P. TACCHINI.

[We are sorry to have unintentionally misrepresented Prof. Tacchini's meaning. At the same time we cannot agree with him that photography is *incapable* of doing what he says; for he still seems to us to have overlooked the possibility of taking several photographs, with different exposures, each suited to bring out some special feature.—ED.]

Projection of Mercury on the Sun's Corona.

SIR,—

The circumstances under which the planet Mercury was seen projected upon the Solar corona by Prof. Langley were so entirely different from those under which it was seen by myself that I am induced to make the following remarks, in the belief that they may prove interesting.

Prof. Langley states that he observed Mercury through the clearest atmosphere of the year, and attributes the fact of his seeing it at all to this state of sky. In other words, the transparency of the atmosphere was such that the slight difference of light intensity was in no way masked by scattered light. The circumstances approached those that prevail at high altitudes and during a saturated state of the atmosphere at the sea-level, namely, definition moderate only or bad, but the quantity of light received by the eye at its maximum. Practical astronomers will not need to be reminded that for observing nebulæ and comets, for glimpsing faint objects barely within the range of the telescope employed, and for most descriptions of spectroscopic work these circumstances are most favourable and desirable.

At this observatory the reverse was the case. The sky was hazy to an extent that rendered the observation beforehand apparently hopeless; but a certain amount, or perhaps description, of haze is eminently helpful to definition, and, aided by the fine optical qualities of the telescope, the disk of Mercury was most sharply cut against the corona. These circumstances are precisely those most favourable for judging slight differences of light intensity, and in photometry are always artificially produced before making comparisons. It is to this I attribute my success. These conditions, so diametrically opposed to those which exist at high altitudes and within the confines of a cyclone, would appear to be most favourable for micrometric measurements and for general astronomical work; and this will, to some considerable extent, account for the good work that has been done for so many years at Greenwich. Prof. Langley has wisely refrained from giving his own conclusions upon the subject which has engaged his attention in Europe; but it is possible that some of these remarks will agree with his experience, or even offer a clue to some of his perplexities. A more difficult question, however, still remains to be answered, namely, whether a site for an observatory can be found that shall combine both advantages, transparency of sky and steadiness of definition.

I am, Sir, yours faithfully,
JOHN I. PLUMMER.

Orwell Park Observatory.
April 18th, 1879.

NOTES.

LONGITUDE DETERMINATIONS IN FRANCE*.—MM. Loewy and Stéfan have recently presented to the French Academy a memoir on the determination of the longitudes Paris—Marseilles, Marseilles—Algiers, and Paris—Algiers. In the discussion special pains have been taken to evaluate the small errors of various kinds which might possibly affect the result; and in this connexion M. Loewy remarks, with great justice, that the accordance of the individual results obtained on successive nights cannot be taken as proof of the accuracy of the mean, since independent observations made under similar conditions may present a remarkable accord and yet be all affected by a large systematic error. Taking account of all conceivable sources of error, M. Loewy estimates the probable error of the final result for each of the differences of longitude Paris—Marseilles, Marseilles—Algiers as $+0^{s}{\cdot}011$. A check on this is furnished by the direct determination of the longitude Paris—Algiers; and Mr. Loewy finds that the agreement is quite satisfactory, the small discordances being of the order of the calculated probable errors.

The time of transmission of the telegraphic signals is discussed in an Appendix. The time occupied by the current in traversing the land-line 863 kilometres in length was $0^{s}{\cdot}024$, or at the rate of 36,000 kilometres a second, whilst for the submarine cable 926 kilometres long it was ten times as great, $0^{s}{\cdot}233$, or at the rate of 4000 kilometres a second. These results agree remarkably with those found by Dr. Albrecht in Germany from a land-line of 836 kilometres in length and an underground cable of 893 kilometres, the differences in the rate of transmission for the two land-lines being only $0^{s}{\cdot}001$, and for the submarine and underground cables $0^{s}{\cdot}002$. This remarkable agreement leads to the conclusion that the electrical conditions of insulation, conductivity, &c. must have been identical for the lines in the two countries. Widely different results have been found in other cases. Thus, in America from the determination of the longitude of Ogden, Utah (*cf.* 'Observatory,' No. 2), over a length of 2000 miles of land-line the velocity was only 6000 miles a second; and in the determination of the longitude of Cairo from Greenwich by the British Transit-of-Venus Expedition, through a submarine cable 3720 miles long without a break (the greatest length ever signalled through), the loss of time before the signal was visible was $1^s{\cdot}31$, corresponding to a velocity of 2840 miles a second.

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* Comptes Rendus, Vol. lxxxvii. No. 20.

End-on Illumination in Spectroscopy [*].—In a paper read before the Royal Scottish Society of Arts, Prof. Piazzi Smyth has pointed out a simple way of inducing "a very lion of trouble seated right across the special path of progress and research," which Prof. Smyth was desirous of pursuing, "to get up, make himself scarce, and not exhibit his threatening front." The "lion of trouble" consists in the difficulty experienced by spectroscopists in getting sufficient illumination out of a low-temperature flame. To raise the temperature in order to get greater brightness would, as Prof. Smyth points out, change the character of the spectrum; and he therefore proposes to utilize a greater thickness of flame by looking end-on, instead of transversely as is ordinarily done. He has applied this method with success to the blowpipe-flame and also to vacuum-tubes, which have been made for end-on view by M. Salleron of Paris. In addition to the greater thickness of incandescent gas, which contributes to the illumination in these tubes, there is the advantage of looking through the thin ends instead of the thick glass of the capillary.

In an appendix Prof. Smyth gives his observations of carbonic spectra and of the spectrum of air to show the superiority of the end-on view to the ordinary method. The positions of the lines are denoted by their "wave-number place per British inch," instead of in the ordinary scale of wave-lengths in tenth-metres used by spectroscopists. In the carbonic spectra a large number of lines are given which appear not to have been noticed before; and in the spectrum of air 221 lines are noted with the end-on view, in a space where with the same dispersion only 72 could be seen with transverse view. It appears that end-on vacuum-tubes were devised by Dr. v. Monckhoven about two years ago, but they appear to have been forgotten till Prof. Smyth reinvented them and pointed out their advantages.

Paper Dome for an Observatory [†].—An astronomical observatory has been recently erected at the Rensselaer Polytechnic Institute through the liberality of Mr. E. Proudfit, of Troy, N. Y.; and as a dome of the dimensions required, if constructed in any of the methods in common use, would weigh from 5 to 10 tens, Prof. Dascom Greene, who planned the building, bethought him of paper as a covering for a framework of wood made as light as possible. He applied to a well-known firm of builders of paper boats, and they carried out the undertaking with great skill and success. The dome is a hemisphere of 29 feet diameter, and the paper (which is about one-sixth of an inch in thickness when dry) was laid on in sixteen sections, being stretched when moist on a wooden model of full size, and allowed to dry in a hot chamber. The paper has a structure as compact as the hardest wood, which it greatly excels in strength, toughness, and freedom from any liability to fracture, and was thoroughly painted before being set up. The weight of the dome and appurtenances is 4000 lbs. It is supported on six 8-inch balls, and can be easily revolved by a moderate pressure without the aid of machinery.

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[*] 'End-on Illumination in Private Spectroscopy.' Edinburgh: Neill & Co., 1879.

[†] American Journal, 1879, January.

The Spanish and French geodesists Ibañez and Perrier intend joining their net-work by latitudes and reciprocal azimuths, observed in the neighbourhood of Oran and the Mulahacen range of mountains, the respective altitudes averaging 1000 and 3900 metres. As the distance is about 300 kilometres, the electric light and special optical apparatus are to be used. Should this succeed, the distance will be the greatest attained at once in geodesy. M. A. d'Abbadie's furthest was only 130 kilometres; but Zach *observed* Mt. Canigou from Marseilles, although 228 kilometres off. The Himalaya range has been *seen* at a distance of 390, according to Sir W. Jones; but sights like these are few and far between.

Editor.

Ephemeris of Brorsen's Comet (by Prof. Schulze).

Berlin	R.A.			Dec.		Log *r*.	Log Δ.
Noon.	h	m	s	°	′		
June 1	12	9	9	48	46·1 N	0·1017	9·9009
3		19	33	46	36·2	0·1107	9·9117
5		28	53	44	28·5	0·1195	9·9230
7		37	18	42	23·4	0·1280	9·9346
9		44	57	40	21·6	0·1364	9·9466
11		51	57	38	23·3	0·1445	9·9588
13		58	23	36	28·7	0·1524	9·9712
15	13	4	22	34	38·0	0·1602	9·9838
17		9	56	32	51·2 N	0·1677	9·9965

From Major Tupman's observations it appears that the comet is considerably to the south of the place given by this ephemeris, the correction being in Dec. −31′ on April 4, and −35′ on April 11, whilst that in R.A. is very small. From spectroscopic observations by Prof. Young and at Greenwich, it results that the brightest of the three comet-bands is sensibly coincident with the green band of carbon, and that consequently the spectrum of this comet no longer stands out as exceptional. Mr. Huggins's observations in 1868, which seemed to show that the bands did not coincide with those of carbon, were made under much less favourable conditions, the comet being much fainter then than at its present apparition.

The comet is now receding both from the Earth and the Sun.

Astronomical Memoranda, 1879, *June.*

Sun. June 1, sets $8^h\,4^m$, rises $15^h\,50$; June 30, sets $8^h\,18^m$, rises $15^h\,49^m$.

Equation of time:—Sun *before* clock, June 1, $2^m\,28^s$; June 11, $0^m\,43^s$: *after*, June 21, $1^m\,25^s$; July 1, $3^m\,30^s$.

Sidereal Time at Mean Noon:—June 1, $4^h\,38^m{\cdot}4$; June 11, $5^h\,17^m{\cdot}8$; June 21, $5^h\,57^m{\cdot}2$; July 1, $6^h\,36^m{\cdot}7$.

Moon. Full, June 4, $1^h\,36^m$; Last Quarter, June 11, $4^h\,56^m$: New, June 19, $8^h\,20^m$; First Quarter, June 26, $17^h\,56^m$.

Occultations:—June 6, $14^h\,31^m$ B. A. C. 6699 Oc. R. 6°; June 11, $13^h\,50^m$ λ Piscium Oc. R. 346°; June 15, $13^h\,50^m$ μ Arietis Oc. R. 302°; June 23, $10^h\,14^m$ ξ Leonis Oc. D. 66°; June 28, $11^h\,2^m$ 87 Virginis Oc. D. 44°; June 30, $12^h\,16^m$ *b* Scorpii Oc. D. 37°. The angles at disappearance (D.) and reappearance (R.) are reckoned from the *apparent* N. point towards the right hand round the Moon's circumference as seen in an inverting telescope.

Mercury is in superior conjunction on June 18.

Venus is an evening star, setting in the N.W. at $11^h\,16^m$ on June 1, and at $10^h\,35^m$ on June 30. Her disk is gibbous, the illuminated portion being 0·692 on June 1, and 0·568 on June 30, and the corresponding diameters 15″·9 and 20″·2.

Mars is a morning star in the east, advancing through Pisces. He is very close to Saturn on June 30 (*cf.* Observatory, No. 21, pp. 279–282 and No. 25, p. 26), the two planets being only 72″ apart at $7^h\,30^m$ G. M. T.

June 1, R.A. $23^h\,43^m{\cdot}7$, Dec. 4° 1′ S., tr. $19^h\,4^m$, rises $13^h\,21^m$.
June 30, 0 58 ·6, 3 41 N., 18 25, 12 1

Jupiter is a morning star in the S.E., in Aquarius.

June 1, R.A. $22^h\,52^m{\cdot}3$, Dec. 8° 19′ S., tr. $18^h\,11^m$, rises $12^h\,50^m$.
June 30, 22 48 ·1, 7 52 S., 16 23 , 10 59

Jupiter's Satellites:—June 1, $14^h\,29^m$ ii. Sh. I.; June 4, $14^h\,29^m$ i. Ec. D.; June 5, $14^h\,7^m$ i. Sh. E.; June 12, $13^h\,40^m$ i. Sh. I.; June 13, $12^h\,11^m$ iv. Ec. R., $14^h\,30^m$ i. Oc. R.; June 19, $14^h\,32^m$ ii. Tr. E.; June 20, $12^h\,45^m$ i. Ec. D.; June 21, $13^h\,40^m$ i. Tr. E.; June 24, $14^h\,24^m$ iii. Oc. R.; June 26, $14^h\,10^m$ ii. Tr. I., $14^h\,31^m$ ii. Sh. E.; June 28, $13^h\,12^m$ i. Tr. I., $14^h\,16^m$ i. Sh. E.; June 29, $12^h\,40^m$ i. Oc. R.; June 30, $13^h\,39^m$ iv. Oc. D.

Saturn is a morning star in Pisces.

June 1, R.A. $0^h\,52^m{\cdot}1$, Dec. 3° 6′ N., tr. $20^h\,11^m$, rises $13^h\,51^m$
June 30, 0 59 ·4, 3 45 18 24 12 1.

	Outer Ring.		Inner Ring.		Ball.
	Maj. Axis.	Min. Axis.	Maj. Axis.	Min. Axis.	Diam.
June 9......	38″·06	6″·49	25″·31	4″·32	15″·2
29......	39″·33	7″·02	26″·15	4″·67	15″·8

The south side of the rings is visible, the elevation of the Earth above their plane being 9° 49′ S. and 10° 17′ S. on June 9 and 29 respectively, and of the Sun 7° 18′ S. and 7° 36′ S.

Uranus is an evening star approaching conjunction, about 10^m *f.* and 82′ *s.* of Regulus. EDITOR.

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THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

No. 26. JUNE 1. 1879.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, 1879, May 9.

LORD LINDSAY, M.P., F.R.S., &c., *President*, in the Chair.

Secretaries: Mr. J. W. L. GLAISHER, F.R.S., and
Mr. A. COWPER RANYARD, M.A.

MR. GLAISHER reported that 52 presents had been received during the last month, the only one calling for special notice being a series of the 'Nautical Almanack' from 1853 to 1878, nearly complete, presented by Mr. Thomas Hill, a Fellow of the Society. Thanks were voted to the donors.

The President called particular attention to a work of M. Wolf, who sent to the Society a paper on the Pleiades, with a very fine map of the cluster containing 600 stars.

The President. Before proceeding to the ordinary business of the Society, I think it may conduce to the well-being of the Society that I should read the following Resolutions which have been passed by the Council, having been taken into consideration at their last Meeting and finally finished to-day:—

"Since the publication of the 'Monthly Notices' for 1879, January, the attention of the Council has been recalled to an article headed 'Notes on the late Admiral Smyth's "Cycle of Celestial Objects &c.," by Herbert Sadler, Esq.,' containing remarks on several of the star-measures given in that catalogue, and also containing a sentence reflecting on the 'Reference Catalogue of Multiple and Double Stars,' forming Vol. xl. of the 'Memoirs.'

"The Council, feeling themselves responsible for the contents of the Society's publications, cannot but express their regret that they should have authorized the printing of this article in its present form. While they desire to uphold, to the utmost, perfect freedom in the criticism of scientific works, they would, at the same time, enforce a general rule to exclude from the Society's publications any imputation on the personal honour or good faith of the authors; and they are sorry to observe in Mr. Sadler's article some remarks which are capable of being, and, to the knowledge of the Council, have been, construed in a sense which infringes this rule.

"The Council are, moreover, of opinion that Mr. Sadler was not justified in passing a sweeping condemnation on the 'Reference Catalogue,' which is irrelevant to the rest of the article, and is entirely unsupported by the citation of the instances on which his judgment was founded."

The Council resolved that these Resolutions should be printed in the next number of the 'Monthly Notices,' and that they should be read at the evening Meeting of the Society.

Mr. Chambers. It was not without very serious consideration that I took upon myself the step of giving notice of a motion of censure on the Council in connection with the charges brought against Admiral Smyth by Mr. Sadler. I felt sure that if the sense of justice of the Fellows were appealed to they would see that this charge ought not to have been brought before the Society unless some attempts were made to substantiate it; and that if no attempts of that kind were made, it seemed to me most necessary that some vote of censure should be passed which would, of course, fall in a secondary sense on the Council, but first on Mr. Sadler for having done as he has done. (Applause.) I was very glad to hear the announcement which has been made from the Chair on behalf of the Council respecting this matter within the last few minutes. It seems to me, therefore, that the ground is cut away from under my feet, because the censure which I, as a private Fellow, should have asked the Fellows of the Society at large to have passed has been passed so fully and so much more powerfully by the Council that I cannot for a moment doubt that when Mr. Sadler becomes acquainted with the action of the Council in this matter he will at once tender his resignation of his seat at the Council. (Applause.) That being so we shall have secured all that we desired. We shall have vindicated the memory and character of Admiral Smyth, as well as have relieved the minds of a very large number of Fellows of this Society. (Applause.)

Mr. Ranyard. May I be excused for interrupting? The Council does not by its resolution vindicate the character of Admiral Smyth: it has passed a resolution with regard to the character of the personal accusation. It does not say any thing as to the personal question. I think Mr. Chambers is unwise in attributing that position to the Council.

Mr. Chambers. I feel bound to quote the terms of the censure passed by Mr. Sadler, on the authority of Mr. Burnham, on Admiral Smyth:—"It does not argue well for the state of astronomical scholarship that such a 'stupendous fraud,' as one of the first of double-star observers has happily termed it, should have remained so long undiscovered." That is a quotation from the 'English Mechanic,' and it backs up and indorses the character of the accusations made in the 'Monthly Notices.' Therefore I am satisfied to consider the censure passed by the Council as virtually a censure passed on Mr. Sadler for having done a wrong thing, at the wrong time and in the wrong way. Under these circumstances I do not desire to proceed with the motion of which I have given notice, because I feel that Admiral Smyth's memory, and the justice, honour, and integrity of this Society, have been sufficiently vindicated by the action of the Council. If Mr. Sadler feels that, after the resolution which has been passed by the Council, it is not incumbent on him to tender his resignation as a member of the Council, all I can say is, that I, for one, should have great difficulty in comprehending the character of Mr. Sadler.

The Astronomer Royal. I rise at the present moment to express the extreme satisfaction with which I have heard the resolution of the Council. It is well known to you that I have felt very deeply on this subject. On the one hand, that the state in which things were left by the publication in the 'Monthly Notices' of January, without any thing following it, was very dishonourable to the Society. (Hear, hear!) And at the same time I very painfully felt the effort which would be necessary to make any correction in it. I am happy to say that that correction is made entirely to my satisfaction, and I am relieved of a labour which I cannot express to you. In this, I beg to say, I take no part in the controversy with Mr. Sadler. That was no part of my business. My business was with the position in which I, as a responsible Member of the Council, felt that the Council and the Society were placed by the publication of this paper. Upon investigation I am satisfied that there was a degree of inadvertence, and not of intention, in the publication; and all that I desired was an avowal by the Council that that was the state of things, and that when they came to look at the real meaning of the paper they would disavow it; that they would stand in the manly position of saying:—"We have done wrong to the public, and we will set ourselves right with the public." (Hear, hear!) I believe that that is done effectually by the resolution which your Lordship has just read as passed by the Council at their late Meeting. And, if I may make a slight atonement on my own part, I did take the step of saying that I could not remain longer a Member of Council if the former state of things was continued. I trust that the Council will allow me to recall that now. I still am prepared to act in perfect confidence with the Council. The inadvertency has arisen from a bad practice in the transaction of some of the Council-matters which has been growing up for years. In the days of old, when I was a more active Member of the Council than I have been for some time past, the process with regard to papers communicated to the Society was uniformly this—that they were referred to referees, and those referees gave their opinions deliberately in writing upon the merits of the paper. (Hear, hear!) And in verification of this I cited to the President of the Society thirty-seven instances in which I had given opinions in writing upon such references. It seems that this system has broken down in the lapse of time, and that references have been made in a very loose way indeed; and it seems, so far as I can ascertain, without asserting it as a fact, that in the particular instance which has occasioned this unhappy discordance in the Society, the paper in question was never sent at all to the first of the referees, but only to the second at a moment's notice, saying, "Here it is; look over it for five minutes, and then sign it." I believe that if the system to which I have alluded had been kept up there would have been no difficulty in this matter. I am fully persuaded that due form of that kind will be maintained in the future. (Hear, hear!) But be that as it may, I am entirely satisfied with the explanation which has been given by the President on the part of the Council, and I express myself as extremely happy that things have come to this pass. (Hear, hear!)

Mr. Barrow. As Mr. Chambers in his notice hinted at a censure upon the Council, I wish simply to make one remark. I am quite sure, if he had known what had taken place, nothing like a censure of the Council could have entered into his mind. The reason why no notice has been taken of this matter until to-day was simply this, that the Council, regarding it as an important point, approached the consideration of it a month ago; but they were so anxious to arrive at a proper conclusion, and to do fair and proper justice, that they found that their deliberations could not be closed on that day, and it was necessary to adjourn it until to-day; and it was on that ground alone that the Council might have seemed to Mr. Chambers as though they were taking no action in the matter. I think it is due to the body of which I have the honour to be a member that that explanation should be made.

Mr. Brett. May I ask why the paper was not referred to the referees? (Hear, hear!)

Mr. Ranyard. The paper was referred to two gentlemen, one of whom was not present at the time. The other was present, and it was examined by him during the Meeting, and reported upon in a perfectly formal manner; but, of course, all the notes in the paper were not read to the Council.

Mr. Brett. Who was he, Sir?

Mr. Ranyard. I am not free to be cross-examined in the matter.

The President. I must remind the Fellows that all the remarks of the referees are strictly confidential. If that were not the case it would be impossible to get any opinion from any one of them.

Mr. Proctor. I happen to know Mr. Burnham, and I feel sure that in using the words "stupendous fraud" he did not intend to imply that Admiral Smyth was guilty of personal fraud. There is a use of the word "fraud" in America which is quite distinct from any use to which we apply the word here. (Oh! oh!) It is so, I assure you. An English lecturer was giving a course of six lectures, and a friend of mine, who had gone to attend these lectures expecting a display of optical experiments, was disappointed to find that, although the lectures were admirable, they required a certain amount of thought; and he said to me, "They were a perfect fraud." I said, "Surely you do not intend to reflect upon Prof. So-and-So that he was fraudulent, that he came here with an unfair purpose?" "No," he said; "I do not mean that at all, only I was terribly disappointed." That is what an American often means by the expression in question. Mr. Burnham is a man of extreme modesty; but he thoroughly understands work on double stars. Prof. Newcomb told me he considers him one of the first double-star observers. Those who know him will not believe that he would attribute to Admiral Smyth personal fraud, only that his book was full of mistakes. He spoke with much energy upon that particular point. The book is very costly in America; it had cost him about £7 or £8; and he was thoroughly disappointed in it. In using the word "fraud" he probably meant that he had obtained the book with a special purpose and was thoroughly disappointed; and I believe it would pain him if it were thought that he had reflected on the honour of a man like Admiral Smyth, whose book we so much admire.

Mr. Chambers. I cannot bring myself to believe that Mr. Burnham used the word in any other than the English sense, and I cannot for one moment believe that Mr. Sadler used it in any other than the plain straightforward meaning of the word.

Prof. Pritchard. Did Mr. Sadler adopt the American meaning of the word fraud?

Mr. Proctor. On that point I am not able to express an opinion. I was speaking of Mr. Burnham, and not of Mr. Sadler.

The Astronomer Royal. We were not discussing the character of Mr. Burnham.

Mr. Proctor. But his character seemed to me to be indirectly involved in the discussion; and as he is absent, I thought it right to give what I believe to be the true explanation of his remark.

Mr. Ranyard. I think the Meeting is rather misled by its feelings. Of course we have no right to prevent Mr. Sadler making a charge of fraud; it is a right which every scientific critic has. (Oh, oh!) I do not think it is proper to try and silence any speech by such cries. The Society would, I am sure, wish, by this discussion, to arrive at a higher sense of what is true and fair criticism; and I hope that in future personalities may be more studiously avoided after the strong feeling that has been shown. But I cannot feel that it would be a good result if, in future, charges of fraud were never allowed to be made. (Hear, hear!) I believe everybody here will agree with me that there have been times when such charges have been of advantage to science; for instance, the observations of Father Hell and a remarkable paper of Mr. Hind, and the celebrated Chasles discussion, and many other cases in which such charges have really brought out very valuable results; and it would not be to the advantage of this Society if any expression of the opinion of the Meeting was given which tended to suppress such charges entirely in the future. It really depends upon the manner in which such charges are made, and whether they are made in improper and unparliamentary language.

Mr. Taylor. I think the complaint has been, not that the charges of fraud have been made, but that they have not been in any way substantiated.

The Astronomer Royal. I entirely agree that the examination of scientific papers ought to be perfectly free; but there is a very great difference indeed between that and bringing forward accusations of adoption without acknowledgment, and, above all, putting in an accusation which is irrelevant to the question and unsupported by evidence. I may say, my Lord, that if we had sanctioned, or if we had not absolutely repudiated, such a system as that, we should have put ourselves in the position of some of those current papers in the metropolis which may properly be called "scientific sewers," which are willing to receive any amount of foulness for their own articles, and to distribute it as extensively as possible. It was purely that feeling which urged me so strongly in acting as I have done in this matter. I hope that no further question will be raised.

Mr. Freeman. I consider that the action of the Council should be deemed quite satisfactory to the Society. Mr. Sadler, as a young man, has expressed himself perfectly freely. The friends of Admiral Smyth know what he was; but at the same time we must limit this discussion to the paper as published in the January number of the 'Monthly Notices.' We are not concerned with what is printed outside the publications of the Society.

Mr. Talmage. Might I for one moment say, as I think I brought the matter originally before the Meeting of the Society, that those friends and relations of Admiral Smyth with whom I have been in correspondence feel most perfectly satisfied with the action of the Council, and they hope that the matter will now drop, and that in future we shall be better behaved.

Mr. Neison. It will be in the recollection of the Society that at the March Meeting I took a view which was rather opposed to the general feeling of the Fellows then assembled. I have no intention to say any thing further upon that matter; but I think it is only right that it should be understood that it is only considerations connected with the prosperity of the Society which keep me silent. Further, I think it should be known that the resolutions of the Council were not carried without depriving that body of at least one of its members.

Prof. Pritchard. I have to read a few notes on Sir John Herschel's first catalogue. Those astronomers who are seriously engaged or otherwise interested in the computation of the orbits of double stars, or are engaged in furnishing data for such investigations, are informed, and are requested to bear in mind, that Sir John Herschel's references to the catalogues of the two Struves apply solely to the catalogues as originally published by those most eminent astronomers, and are independent of the corrections which they subsequently found it necessary to make to their original printed observations. To working astronomers it may perhaps be unnecessary to add that they will find the corrigenda on consultation of the 'Positiones Mediæ,' page 92, and in the Catalogue of the Pulkowa Observations, vol. i. 1853.

The Herschel Catalogue in question, forming vol. xl. of the Society's Memoirs, is, strictly speaking, only a *Reference* Catalogue; it refers chiefly to his own works, and to the works of the two Struves; and now that attention is called to the books and places in which these eminent astronomers give the corrections to their original works as cited by Sir John, no doubt or difficulty will exist in the minds of astronomers seriously engaged in double-star investigations. The corrections are accessible to any one who possesses the books; and no one can work at double-stars effectually without those books. And here let me make this observation:—This catalogue of Sir John Herschel was placed on the Council table month after month; and if there had been any doubt as to either the accuracy or the utility, the proof-sheets were on the table, and every Member of Council had an opportunity of seeing them; and if he had any thing to object, then was the time, as I apprehend, to object, and not afterwards.

Mr. Ranyard. What was the reason for not making the corrections, such as misprints and other matters that Struve had himself pointed out before?

Prof. Pritchard. If you possess the book, and will look, you will undoubtedly find it out yourself. Sir John Herschel was an original investigator, and I suspect he did not so much care for book learning. He took the 'Positiones Mediæ,' and he wrote them down. He is now gone. Practically I had no more to do with the correction of that catalogue than the man in the moon. It was done at the Radcliffe Observatory; assuredly I am responsible for it, but it was not done by me. It has, however, been done magnificently well; and I defy any man who is an astronomer to come forward to prove that it has not been most carefully edited. In such a book as the 'Positiones Mediæ' Sir John Herschel would expect to find the *corrigenda* at the *end* of the book or at the *beginning*. Would any person expect in the very thick of a big book to find *notæ criticæ*, and amongst these *notæ criticæ* to find that there were certain corrections to be made to the catalogue itself, as is the case here? Those corrections would only be found out by a person who read the book from beginning to end. They are really of very little importance as compared with any other catalogue that I know; and I have had some experience of catalogues. I am sure that no Greenwich astronomer would tell you that Sir John Herschel's catalogue is worse than the average, or, indeed, that it is not better than the average of catalogues. However, I have explained the cause of Sir John Herschel's errors. They are not the errors of Sir John Herschel; they are the errors of the two Struves. Every mortal astronomer that ever lived must make some errors. The mistake that has been made is in supposing that any long catalogue of 10,000 stars can go forth to the world without there being a number of corrigenda connected with it. It would be superhuman. The thing never was done.

Mr. Ranyard. Am I right in supposing that in editing that catalogue you did not refer to the corrections of Struve's catalogue, which were published along time before your catalogue was published?

Prof. Pritchard. I was told that there were some trifling errors originally made in this catalogue, but they did not amount even to one per cent. It is therefore idle for the Society to discuss a thing of this sort; and I would not now bring it forward if it had not been for the innuendoes that have been going forth. I know the sources from which they have come. The Astronomer Royal has expressed exactly the right word, which I will not repeat. That is the source of all this trouble. I may say, in answer to Mr. Ranyard's question, that there was, as it turns out now, a missing book, in which Otto Struve had corrected the errors that he himself had made. We could not find that book. It was my dear and respected old friend Main, who was in possession, we believed, of the best astronomical library in this country, who felt that there was a missing link—some book that could not be found in the Radcliffe Library. I took upon myself to write to Struve, and ask him for the book, if there was such a book; and he wrote back to me, stating that that book was in the Bodleian Library; but upon going to the Bodleian it was not there. Since then we have found the book in Sir J. Herschel's own library; and any one who will refer to it will be able at once to make the full corrections which Otto Struve himself found it necessary to give. Hence all this turmoil has been about nothing at all.

Mr. Ranyard. Did you refer to our library for the book? We had it.

Prof. Pritchard. Yes, I know you have got it *now*, and that is how I found out the source of the error; and any one seriously engaged in the matter could have corrected it, if he had had the book.

Mr. Ranyard. The editor first of all.

Prof. Pritchard. When you have 10,000 stars to look out? But we did not undertake to do any thing of the sort, but merely to reproduce what Sir John Herschel had produced. We took for granted—at least I did—that he was accurate.

Mr. Ranyard. The printer could have done that.

Mr. Brett. I think we have had enough of this.

Prof. Pritchard. I only say that the accuracy of the catalogue is far beyond the accuracy of catalogues in general; and its utility is, to my mind, quite equal to the utility of star catalogues in general.

The Astronomer Royal. I think we are all gratified with the perfectly sufficient explanation which Professor Pritchard has given, but I hope it will be put in writing.

The President. I wish to say, with regard to the errata alluded to by Prof. Pritchard, that it is a matter of great difficulty to get hold of a copy of Struve's corrected work. I thought I was fairly intimate with the writings of Struve, and of most places where his works appeared; but when I published the first volume of the Dun-Echt publications, which is a *résumé* of Struve's great work on double stars, I had not the slightest idea of the existence of those corrections, and consequently they did not appear. But at the same time I must say that in all the criticism which I have seen, I have not been abused for not having found those errata. They will appear in a future volume.

Mr. Knobel. With regard to the book referred to by Prof. Pritchard, Mr. Ranyard thinks that we have it in the library. So we have; but I think I found that book stowed away in the top gallery some three or four years ago, tied up with some other papers, where it must have been lost sight of.

Mr. Christie read a paper on the spectrum of Brorsen's Comet observed at the Royal Observatory, Greenwich. He remarked that a special interest attached to the spectrum of this periodical comet, as Dr. Huggins, in 1868, found that it was different from that of other comets. Mr. Maunder and he had compared the positions of the three bands in the comet's spectrum with three bands given by a vacuum-tube containing vapour of alcohol, which, according to Dr. Marshall Watts, gives the second spectrum of carbon. Using the "half-prism" spectroscope with a dispersion equivalent to four prisms of 60°, the agreement of the two spectra was sensibly perfect. According to these observations, the comet's spectrum would be identical with the second spectrum of carbon, and a similar result had been found in the case of Coggia's comet of 1874. But Prof. Young had found that the spectrum of Brorsen's comet this year agreed with the spectrum of the blue base of a flame or first spectrum of carbon; and the interesting point arose as to which of these two spectra of carbon represented the cometary spectrum.

Mr. Christie explained on the black-board the difference in the positions of the bands of the two spectra of carbon, and pointed out the extent to which, in the Greenwich observations, the width of the slit might have affected the question of coincidence between the comet and the comparison spectrum. He gave reasons for concluding that the uncertainty in the comparison of the principal band could not amount to 10 tenth-metres, or less than one third of the distance between the corresponding bands in the two spectra of carbon; and therefore came to the conclusion that the cometary spectrum was probably the second spectrum of carbon, and not the first or flame-spectrum.

The President. The reason that I asked Mr. Christie if he could give the wave-lengths was because I have just received a note from Dr. Copeland on this very subject of Brorsen's comet, giving the result of his observations at Dun Echt.

The chief matter of interest is that, in that which seems to be the best defined line, the central one, Dr. Copeland's measures give for the wave-length 5150 as against Mr. Christie's 5190.

Mr. Christie. I do not think there could be so much uncertainty in our measures. I estimated the uncertainty at the time as a small fraction of the breadth of the occulting bar, which is 30 tenth-metres.

The President. I do not wish to claim for these measures any extraordinary accuracy, for this reason, that the instrument which we are using at present is of rather weak construction, and we have been obliged to solder certain tin plates on the base of it.

Dr. Schuster. As the spectrum of carbon has been mentioned, and some misunderstanding seems to exist with regard to this, perhaps I may be allowed to make a few remarks. Mr. Christie has mentioned the spectra of various orders of carbon; but I think he has used the terms spectra of the first and second order in a different sense from that in which they are generally understood*. There exists a spectrum of carbon of the first order no doubt, but I do not think Mr. Christie has seen it. At least it does not appear in these drawings, and it is got with some difficulty. There are two other spectra he mentions; one of them which you see at the base of any blue flame of a hydrocarbon. I do not enter into the question whether that is the spectrum of any hydrocarbon or of carbon vapour; but the bands are made up of single lines. Then there is another with bands near those, which he calls the spectrum of an alcohol vacuum-tube. It is the spectrum of carbonic oxide. The bands are slightly displaced. There is another spectrum, that of carbonic acid, which is slightly different again; but in the spectrum of carbonic acid I do not know whether the green and yellow bands are altered at all; the red and the blue bands are altered, I know. The carbonic oxide bands are sharply defined at one edge, and gradually fade away on the blue side. In what I believe to be the true carbon spectrum the green band really breaks up into two strong lines; and when the light is weak, with an ordinary Bunsen burner I do not think you would be able to see more than two lines.

With regard to the cometary band in the yellow, there are, in addition to the bands in the two spectra of carbon, quite a number of lines or bands which fall in that part of the spectrum, and which cannot with certainty be distinguished from each other when a wide slit is used with small dispersion. If you take an oxygen vacuum-tube and observe the spectrum of the negative pole, you will see a band sharp not only at one edge, but at both ends. It is not at all difficult, with large dispersive power, to draw the difference between those two. One is slightly more refrangible than the other. Then we have got the aurora line, and we have got a line which has puzzled me very much, which appears sometimes in the oxygen tube, which at first I thought was coincident with the aurora line, but I now think the aurora line is slightly less refrangible. The spectrum with which Mr. Christie thinks the comet-spectrum is coincident seems to be the carbonic-oxide spectrum, and not the spectrum of the blue flame, which I believe to be due to pure carbon vapour, but which some people believe to be due to hydrocarbon.

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* [This is the sense in which Dr. Marshall Watts, in his index of spectra and in a recent letter to 'Nature' on the spectrum of Brorsen's comet, uses the terms. M. Thalén objects to the phraseology altogether, and considers that in any case the line-spectrum ought to be called the first spectrum, and not the third, as Dr. Watts and others have done. We do not here wish to express any opinion on the question whether the spectra are due to compounds of carbon differing in their chemical composition or in their molecular condition as affected by difference of temperature.—Ed.]

Capt. Noble. I should like to repeat a question which I have often put of late years, whether it would not be within the resources of science to issue an ephemeris of such an object as Brorsen's comet upon a postcard. In Mr. Carrington's time we used to have little postcards with ephemerides on them, but we never get them now.

The President. I have had this matter under consideration for some little time past, and I hope before the autumn to have some scheme ready to place before the Society.

Mr. Ranyard read two papers on Brorsen's Comet from Mr. Tebbutt and Mr. H. C. Russell respectively.

Mr. Neison read a paper on the "Determination of the Solar Parallax from the parallactic inequalities in the motion of the Moon, and on the Correction to Hansen's coefficient of the Annual Equation." It contains the result of the reduction of 1464 observations with the Greenwich Transit-Circle between 1862 and 1876, made with the view of determining the correction to Hansen's coefficients of the annual equation, the mean longitude and the terms depending on the distance and twice the distance of the Sun from the perigee of the lunar orbit. Mr. Neison pointed out that the parallactic term, depending on the distance of the Sun from the perigee of the lunar orbit, was well adapted for determining the solar parallax, being free from the liability to systematic error which so seriously interferes with the use of the parallactic inequality. The solar parallax determined in this manner comes out too great, viz. $9''\cdot 1$, a discordance which Mr. Neison hopes to be able to trace to its source. The results obtained in the paper show that Hansen's coefficient of the annual equation requires to be sensibly increased.

Mr. Inwards produced and explained a model of an observing-chair, which, as he stated, presented a series of more or less comfortable angles, and was under the control of the person sitting in it.

Mr. Common thought that the chair ought to be balanced on the centre of gravity so as not to involve the lifting of the body when it was required to give the necessary inclination to the head. If the fulcrum on which the chair rested was immediately under the part where the back joined the seat there would be no necessity for moving or raising the body every time the head was moved.

The President. No doubt it would be most convenient to have the motion of the chair about the position of the eyepiece so as to keep the eyes pretty nearly steady while the body moves up or down as the case may be. The head would remain in a constant position with respect to the telescope while the body was in comfort. It would be a very comfortable way of doing it, no doubt, to make the head the centre of gravity.

Mr. Freeman. As the convenience of the observer entirely depends upon how the chair works practically and the arrangements for raising it, and as Mr. Inwards does not mean to take out a patent for the mechanism, he would confer a benefit upon the Society if he would furnish working drawings for constructing it.

Capt. Noble. It seems to me that the chief objection is that you cannot get your eye higher than if sitting in an ordinary chair; and with a telescope of any considerable length, unless the star is pretty high up, say near the zenith, the chair would be of no use at all, certainly not if the star were near the horizon.

The President. I imagine that when a star is near the horizon it would be advisable to sit on an ordinary stool, and not to trust to the telescopic chair.

Mr. Inwards replied to the criticism of Mr. Common on the construction of the chair, explaining its action by the working model which he exhibited.

Mr. Brett. I ask leave to call the attention of the Fellows to the position of the planet Venus at the present time, which is particularly favourable for observations of specular reflection. Most people can see it without any aid, and for those who cannot the dark wedge will be found to demonstrate it very satisfactorily.

The President. As to what has fallen from Mr. Brett, I am sure the gentlemen of the Society who listen to him are only too anxious to be convinced by him. I wish I could get away from London in order to have an opportunity of looking through Mr. Brett's telescope.

Mr. Knobel. I should like to draw Mr. Brett's attention to the report of the United States observations. There he will see that their series of observations of Venus were made with a 26-in. equatoreal; and they make no mention at all of what Mr. Brett professes to have seen with a small telescope.

Mr. Brett. People have looked at Venus for the last eighteen hundred years and have not seen the bright spot; therefore it is not surprising that the Americans in Washington have not seen it*.

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* [The bright spot on Venus, referred to by Mr. Brett, has been seen on several occasions by several observers at Greenwich with the Merz 13-in. equatoreal. The difficulty in seeing it arises from the dazzling glare of Venus, which would be intensified in a large instrument, such as the Washington 26-in. equatoreal, and special precautions would therefore be necessary.—Ed.]

Mr. Neison said he was desirous of bringing a more interesting subject before the Society than his previous communication, as he wished to show some drawings of the region around the lunar crater Hyginus. When he brought the subject before the Society at a previous meeting, Mr. Gill made the pertinent remark that though he had informed the Society of what other people had seen, he had not told the Society what he had seen himself. At that time he had never had an opportunity of seeing this portion of the Moon under suitable illumination, on account of the persistently bad weather. On March 1 of this year, the day after the time when the crater would have been most favourably placed, he observed this region and saw a dark ill-defined spot in the place where Klein's new crater was said to exist. The next day the spot completely disappeared. This agreed with Dr. Klein's account of his new crater. On April 28 another favourable opportunity occurred for observing this region, and the crater was then distinctly visible as a large black spot with a nebulous edge. It was so large and distinct that it was impossible to overlook it, being far more conspicuous than any of the small craters and hills which have been long known to exist in this region. He would here show the meeting a copy of the drawing he had made. The other day he brought his drawing down to the Society's rooms and showed it to several Fellows, and there met Mr. Green, who had also brought down to the Society a drawing made on the same night. The two drawings agreed perfectly. There was no doubt, therefore, that there now exists in this region a large black conspicuous object where no such object existed before; for it is so large that it could not have existed and been overlooked by previous observers when they drew the much less conspicuous objects on all sides of it.

Mr. Green described on the black-board what he had seen when observing the same object. When he made his first observations in the month of March the large spot exhibited itself, and on studying the Melbourne photograph he had found a distinct mark in the position of the dark spot which he had seen. In the month of April, under better illumination, instead of a large round and vague dark spot, he saw in the eastern portion of it a distinct dark spot as Dr. Klein had stated, which seemed to be a portion of a large and dark depression.

The President. It is a matter of very great interest, and I am sure the Society will be very glad to hear of the results of work in this new field, and will return their thanks to Mr. Neison for his communication. We will now proceed with the ballot.

The following were balloted for and duly elected Fellows:—Capt. E. Barnes, the Rev. W. C. Bruce, M.A., Geo. T. Gwilliam, John Penn Hartree, M.A., Charles Augustus Jenkins, William Edward Plummer.

The following papers were announced:—

J. Tebbutt. Observations of Brorsen's Comet, February 1879.

W. F. Denning. Meteor radiant-points of April 9–12.

W. F. Denning. Catalogue of 222 stationary meteors.

H. C. Russell. Observations of Brorsen's Comet.

Lord Lindsay. Observations of Brorsen's Comet.

J. J. Astrand. Two short and easy Methods of correcting Lunar Distances.

Prof. C. Pritchard. Note on Sir John Herschel's Reference Catalogue of Double Stars.

E. Neison. On the Determination of the Solar Parallax from the parallactic inequalities in the longitude of the Moon, and on the correction to Hansen's coefficient of the Annual Equation.

O. von Struve. Order of publication of successive Volumes of the 'Observations de Poulkova.'

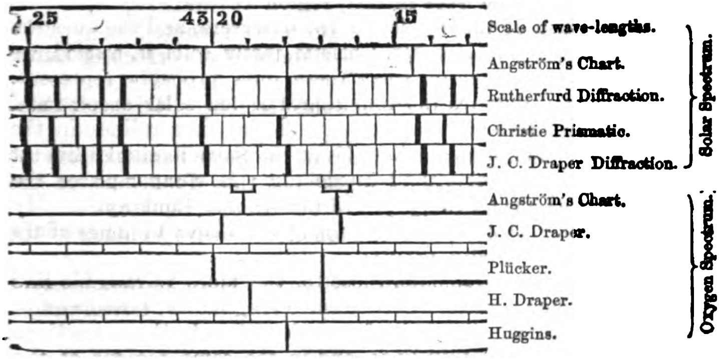
Astronomer Royal (communicated by). On the Spectrum of Brorsen's Comet observed at the Royal Observatory, Greenwich.

The Meeting adjourned at ten o'clock.

On the Dark Lines of Oxygen in the Solar Spectrum on the less refrangible side of G.

In a paper on the lines of oxygen, published in the 'American Journal of Science' for October 1878, the solar lines closely coincident with those of the electric spectrum of oxygen were given. The measurements of the solar lines, in that instance, were taken from photographs made in March 1878. Since that time, under more favourable atmospheric conditions, I have succeeded in obtaining finer photographs on the less refrangible side of G. These show many faint lines, not visible in the photographs of March 1878, while many other lines are distinctly subdivided. Three of the recent photographs were taken on the same day during the last week in November, and two have been taken since that time—one during the last week in January, and one during the first week in February. In all five of these photographs faint lines are visible between λ 4316 and λ 4320 of Angström's scale; and they all show them in the positions indicated by the diagram and table. The fact that all the photographs agree in their representation of these lines, is, I think, proof positive of the correctness of the positions assigned.

To place the question of the oxygen lines in this region as clearly as possible before the reader, I have devised a diagram which represents the facts regarding this portion of the solar spectrum, together with the wave-lengths of the oxygen lines obtained from the electric discharge in air and in oxygen gas. The final drawing of the diagram was made for me by my assistant, Mr. Sickels, from the wave-lengths given in the table which I had prepared. The drawing was on the scale of one centimetre to each unit of wave-length (a tenth-metre); this was photographed down to one half its size by a photo-engraving operation. From the plates so produced the diagram was printed.



The first space of the diagram beneath the scale of wave-lengths gives the positions of the solar lines of Angström's chart.

The second represents measurements made from one of Dr. Rutherfurd's paper prints of this region. These measurements were made after I received Mr. Christie's paper, with a view to the detection of any difference between Dr. Rutherfurd's photograph and my own.

The third space contains a representation of the lines of this region as given by Mr. W. H. M. Christie, of the Royal Observatory at Greenwich, in the 'Monthly Notices' for June 1878. This I did not receive until the latter part of last January, when I was much pleased to find the close coincidence of our results, although they were obtained by entirely different methods and without any knowledge of each other's work. Line by line Mr. Christie's map is the counterpart of the spectrum I had mapped from the photographs taken during the last week in November and since that time. Not only do these maps agree very closely in the placing of the lines, but the similarity in intensity is also marked. The presence of these fine lines in Mr. Christie's prismatic spectrum settles any objection that might be raised on the ground of their being interference lines in the diffraction spectrum.

The fourth space presents the measurements of my recently taken photographs. The accuracy of adjustment to Angström's scale is shown by the coincidences in the lines λ 4312·8 and λ 4325·2. The comparison of this with my reading of Dr. Rutherfurd's photograph, as given above, while it shows slight differences, is a very satisfactory evidence of the reliability of the photographic method of investigating this problem. It is also a refutation of the objections raised by some to my system of measuring and studying the lines of solar and other spectra. Indeed, with proper lenses, and in competent hands, the process I have followed is not only very reliable, but the apparatus may be truly regarded as an instrument of precision. Of this I hope to give ample proof hereafter.

The position of the lines in this region of the solar spectrum being established by three independent observers, and the question of primary importance, as Mr. Christie justly calls it, settled, we pass to the examination of what these lines probably represent. First, Oxygen. Beneath the four charts of the solar spectrum, I have placed five sets of measurements of the oxygen lines in the vicinity of G. Beginning below, Huggins gives a single line at 4318. The other observers all divide this; H. Draper places the two branches at 4317 and 4319, Plücker at 4317 and 4320. My own readings are at 4316·50 and 4319·75. Those of Angström's chart are at 4316·57 and 4319·15.

In the above measurements, those for the more refrangible line are all within one half of a unit of wave-length. The readings of the other lines are within a unit of wave-length. In these positions there are, therefore, oxygen lines, and in the same regions of the solar spectrum Dr. Rutherfurd, Mr. Christie, and myself agree in finding faint dark lines. More than this, granting that the forked symbols of Angström's chart indicate (and I cannot understand what else they mean) that the observer believed that the line λ 4316·57 is divisible into two lines, viz. λ 4316·20 and λ 4316·95, and the line λ 4319·15 into two, λ 4318·85 and λ 4319·45, we find that the three charts of the solar spectrum give lines which are very nearly coincident with the readings of these forked symbols. It is not often that the results obtained by so many independent investigators agree as closely regarding a problem to be solved only by observation and experiment; and, considering the smallness of a tenth-metre, and the difficulty of making such measurements, the agreements must be regarded as favouring strongly the identity of these feeble lines of the solar spectrum with the lines of the electric spectrum of oxygen.

Among the obstacles in the way of comparing the electric lines of oxygen with those of the solar spectrum, and which, in part, explains the discrepancies that exist in the measurement of the lines of oxygen, is the difficulty in making exact measurements of these lines. Of this difficulty Plücker speaks in a memoir on the oxygen spectrum in the 'Phil. Trans.' for 1865. In the same memoir he also discusses the variation in the breadth of the lines, and the variation in this variation in different parts of the electric spectrum of oxygen. The positions given by Plücker and myself agree closely, although he gives no fractions. We both used two prisms of flint glass, though at times he used four. Plücker's results and my own were obtained in pure oxygen gas; while those of H. Draper and of Angström's chart were from the electric spark in air. This may possibly explain why they agree in differing slightly from Plücker and myself.

In the measurements of the lines of the solar spectrum made by Mr. Christie, the "half-prism" spectroscope was, he says, equal in dispersion to fifteen prisms. Dr. Rutherfurd's photographs and mine were made by means of Rutherfurd's gratings, of over 17000 lines to the inch, mine being by reflection and in the second order. The great dispersion thus brought to bear upon the solar spectrum is, I think, the explanation of the subdivision in that spectrum of the electric oxygen lines given as two by Plücker, H. Draper, and myself. I do not know what the dispersive power was, nor the exact electric conditions that gave the forked symbols of oxygen in Angström's chart; but whatever the dispersion, and whatever the electric and consequent thermal or other conditions, they seem to have produced results very close in their relation to those in recent photographs of the solar spectrum.

Discussing the existence of bright lines of oxygen in the solar spectrum at λ 4317 and λ 4319, Mr. Christie directs attention to the opposing fact that the spaces at λ 4315 and λ 4321·5 on Dr. Rutherfurd's photograph are both brighter than those at λ 4317 and λ 4319. This is also shown by my photographs, in which the slit was sufficiently narrow. I may add that there are in my photographs and in that of Dr. Rutherfurd, in the region from 4290 to 4301, many lines brighter than any in the spaces mentioned. Accepting these as being the true measures of brilliancy of the bright background of the spectrum in this region—and there is no reason why we should not do so—we are justified in saying that from λ 4314 to λ 4322, and particularly in the vicinity of λ 4317 and λ 4319, there is something in the solar envelopes which by absorption reduces the brilliancy of the spectrum. In addition there are also faint lines which occupy the positions of certain electric oxygen lines.

If these faint dark lines are not the results of the action of oxygen, to what other cause are they to be attributed? Beside the well-marked lines of calcium and titanium, no one places lines of any element here except Huggins, who claims a weak strontium line at λ 4319, and a carbon line is given at λ 4320 in Watts's Index. The space from 4315 to 4318 is free from any claimant, and here there are lines representing the position of the lines of oxygen, upon which nearly all the measurements closely agree. It appears to me that this want of any other explanation is a fact of importance by no means to be overlooked in arriving at the true solution of the problem of the oxygen lines.

In Dr. Rutherfurd's photograph, I think several of the lines are subdivided. An inspection of the original photograph of the spectrum itself is required to decide this matter, as the enlarged paper print in my possession is not entirely satisfactory, giving only the appearance of subdivision as I have recorded.

Conclusions.

1. The regions in the solar spectrum, λ 4317 and λ 4319, claimed as bright lines of oxygen, are not as bright as others in their immediate vicinity.

2. The solar spectrum shows faint dark lines in the region about λ 4317 and λ 4319.

3. Oxygen is the substance which can produce dark lines in this region; therefore we must attribute them to the presence and action of that element. J. C. Draper.

Mira Ceti.

The following are the results derived from my observations of Mira Ceti, made at Cambridgeport, Mass., U.S.A., during its maximum, Oct. 1878. The extremely low altitude of Mira prevented convenient observations until after it had passed the limit of visibility; so that when first observed, Sept. 22, it was found to equal a fifth-magnitude star in brightness, and so rapid was the increase of light that three days after (Sept. 25) it was nearly equal to δ Ceti of the fourth magnitude; and this rapid increase continued up to Sept. 30, on which date Mira was observed to equal ν Ceti of the third magnitude. The increase of light thus appears to have been remarkably rapid, much more so than was noticed at its last maximum, Dec. 1877, the increase from the fifth to the third magnitudes occupying only eight days, against fourteen days as recorded last year. From Sept. 30 to Oct. 6, the changes of light were very slow, hardly more than two steps.

The observations were rendered less certain, however, after Oct. 1, owing to the moonlight, which unfortunately prevailed at about the time of maximum brilliancy. The increase of light after Oct. 6 was certainly very small, not exceeding, at most, more than one or two steps, and this amount would be included within the limits of the errors of observation. The slight hollow in the light-curve noticed by Dr. Schmidt last year appears to be represented by my observations as occurring between Oct. 28 and Nov. 12, although very little reliance should be placed on this doubtful fluctuation recorded, as the decrease was certainly not more than one step, and the observations were too few in number, and too widely scattered at this important point in the light-curve, owing to a succession of cloudy evenings, to warrant the acceptance of this result as sufficiently demonstrated. From Nov. 12 to Jan. 10 the decrease of light was slow and quite regular. Unfavourable weather unfortunately prevented a good series of observations after Dec. 24, although the few observations obtained evidently show a rapid decrease. The observations terminated Jan. 20, on which date Mira was found to be about 1 or 2 steps fainter than a sixth-mag. star. The observations were made by Argelander's method of step estimations, and in their reduction his scale of light values (B.B. 7) has been employed, and the average value each evening taken as representing the brightness of Mira. A low-power opera-glass was used throughout the series, and, when practicable, observations were also made with the naked eye. Leaving out of account the doubtful fluctuation recorded, I find the maximum to have taken place about Oct. 20.

The following statement regarding Mira was made to the writer about the middle of Sept., by Mr. W. K. Greely, of Boston; and owing to the great improbability of such an occurrence, and the greater importance attached to it if true, I have thought it of sufficient interest and value to give the details in full; and I trust that the phenomenon has been seen and confirmed by other observers.

About the middle of August (exact time uncertain) Mr. Greely, on his way home early one morning (his occupation being such as to keep him until late at night), in looking for Saturn through the clouds, saw a star, which, for a moment, he mistook for the planet; but its scintillations soon showed that it was not Saturn, but something unusual in that quarter of the sky. The clouds cleared off soon after, giving him an opportunity of noting its brightness, which he says was fully equal to any second-magnitude star. Being somewhat curious to know what star it was, he carefully memorized its position, and, on reaching home, by means of a star-chart identified it as being Mira. It remained cloudy for several nights after; and when he again saw it, it had greatly decreased in brightness, and shone as a fourth or fifth magnitude star. It was about a month after (or the middle of Sept., as stated above) before Mr. Greely was able to see me (knowing that I was interested in this branch of observation) and acquaint me with the facts. I placed very little reliance on his statement at the time, merely regarding it as a case of mistaken identity, knowing Mr. Greely to be unaccustomed to astronomical observations, and also as I was aware that the ephemeris gave Oct. 11 as the date of maximum brilliancy, and that no such irregularity in the period of Mira had before been recorded. I told him so at the time; but so positive was he that he was not mistaken, I thought it best to mention the matter to Mr. Seth C. Chandler, jun., a variable-star observer, and get his opinion, which I did at the first opportunity, happening soon after. Mr. Chandler rather doubted the statement, as I had; but still he thought it of sufficient importance to follow the matter further and get the best data obtainable. On the evening of Sept. 23 (a day after I had commenced my observations of Mira, and was unable to tell as yet whether it was on the increase or decrease) I met Mr. Greely again; and as this fortunately happened at a time when Mira was quite well situated for observation (although rather faint, being equal to a fifth-magnitude star), I was thus able to satisfy myself whether Mr. Greely was acquainted with the position of Mira or not. As soon as I asked him to point out the Constellation Cetus, he almost immediately identified the Constellation, and also the position of Mira. I was then fully satisfied that Mr. Greely's former statement was correct; and, impressed with the importance of getting as accurate information regarding the phenomenon as possible, I have since corresponded with Mr. Greely in regard to the matter, but am sorry to state that the rough data furnished me at the first interview with him is the only information obtainable regarding this unaccountable and remarkable increase in the brightness of Mira (two months before the predicted time of maximum brilliancy), as Mr. Greely had made no notes of his observations, such as a practised observer would have done.

I shall wait impatiently to hear whether the phenomenon has been seen by any European observer, and especially by Dr. J. Schmidt, of Athens, who no doubt has kept steady watch on this star. EDWIN F. SAWYER.

Recent Observations.

THE dull weather this spring has considerably obstructed astronomical observations, though "aqueous meteors" have been tolerably abundant. It is possible, however, that the following jottings may interest some of your readers.

1879, *January* 15. Sirius being extremely brilliant and near the the meridian, I went into a dark room with a small south window, and found that it cast a pretty distinct shadow on white paper, though of course not nearly so plainly as Jupiter or Mars do in opposition.

March. This month was notable for meteorological phenomena, especially halos and parhelia. On the 4th, besides these, the circumzenithal arc was seen, although the great halo was invisible. On the 16th the ordinary halo with a very wide tangent at the top was bright, the great halo faint. Mock suns were noted on six days this month.

April 1. A very fine display of halos. The ordinary one faint with a vivid coloured arc above, the ends depending on each side to a level with the Sun; between these ends and the halo were distinct parhelia, the tails of which were prolonged into the parheliacal ring quite round the sky.

April 11. Made my best observation of Brorsen's comet, although it was rather low when the clouds cleared off. It had a very narrow tail about 20′ in length. This was seen with a 4½-inch reflector. I was unable to reobserve this tail, as the weather was bad.

April 17. A good-sized spot on the Sun, the first group observed for months.

April 19. Watched all night, from 9 till 3, for Lyrid meteors, but was considerably disappointed, as they were quite scarce. Of 47 meteors recorded, only 13 were conformable to the usual radiant. The centre of the rather diffuse tracks was at 275° +37°. Only 2 to 3 Lyrids were seen per hour. There is not much colour apparent in these meteors; the 1st mags. seem pure white, yellow being occasionally seen in the 2nd mags. A few, about 15 per cent., leave narrow streaks.

On the 18th and 20th one and two Lyrids were seen in an hour on each date, but the shower seems to have gradually weakened.

April 28. A bank of cloud on the horizon, which had the quality of causing "columns." As the sun set, a partial one formed over it, and later on the Moon had one through it, as a patch of the cloud rose up over it. But the most interesting phenomenon was, that Venus and the stars in that part of the sky had telescopic columns through them. That through Venus was distinctly visible with the unaided sight, but in the telescope it was very bright and curious.

May 2. Rose at 2 to watch for the Aquariad meteors discovered by Major Tupman some years ago. Only two of them were seen, at about 3 o'clock. They are noteworthy from the fact that the radiant being near the horizon, their tracks seem very long, about 20°. They usually leave fine streaks.

May 3. Brorsen's comet passed over a star about 8th mag., and again on the 4th over another about 9th mag. In both cases the comet almost disappeared with the 4½-inch reflector, especially when steadily looked at.

Looked at Plato. The walls seem double for about ⅜ of the way round, and not only at the "landslip." On March 1, I drew it as a black spot between two bright walls, the terminator then passing close outside the crater. H. CORDER.

Writtle, Chelmsford.

Double Stars for June.

THE following are the most interesting pairs from 10^{h} to 13^{h} of R.A. The less important objects will be given next month.

Σ 1424 = γ Leonis. R.A. $10^{\mathrm{h}}\ 13^{\mathrm{m}}{\cdot}3$, Dec. +20° 27′.

Mag. 2, 3·5. Colours, A golden, B greenish red.

A very fine binary star. Doberck has recently obtained the following elements:—☊ 111° 34′, λ 195° 22′, γ 43° 6′, e 0·7327, P 407·04 years, T 1741·00, a 1″·98.

1877·41	112°·1	3″·32	Flammarion.
9·36	111 ·9	3 ·30	Gledhill.

There is a distant star of the 7th magnitude observed by Flamsteed in 1691. The following is the most recent measure:—

1877	292°·8	229″·3

A careful examination of the measures of this small star has led M. Flammarion to suspect some irregularity in its proper motion or in that of the principal star.

Smyth says there are "two stars in a line with A in the *n.p.* quadrant." One of these is no doubt the small star just mentioned, but it is not easy to identify the other.

Σ 1457. R.A. $10^{h}\ 32^{m}\cdot5$, Dec. $+6^{\circ}\ 22'$.

Mag. 7·4, 8·4. Colours, white.

1875	314°·9	1″·19	Wilson.
7	314 ·6	....	Doberck.

A physical system.

OΣ 228. R.A. $10^{h}\ 40^{m}\cdot7$, Dec. $+23^{\circ}\ 12'$.

Mag. 7·2, 8·1. Orbital motion.

1875	13°·2	0″·37.	Spörer.

Σ 1517. R.A. $11^{h}\ 7^{m}\cdot4$, Dec. $+20^{\circ}\ 47'$.

Mag. 7·3, 7·3. Binary. Considerable common proper motion.

1877	284°·8	0″·5	Hall.

Σ 1516. R.A. $11^{h}\ 7^{m}\cdot4$, Dec. $+74^{\circ}\ 8'$.

Mag. 7, 7·5. A remarkable pair. Rectilinear motion.

1877	91°·0	9″·51	Flammarion.

OΣ in 1858 detected a small star of the 12th mag., which forms a binary system with A.

The measures are:—

1858	294°·1	8″·187	OΣ.
75	299 ·4	7 ·48	Dunér.

Σ 1523=ξ Ursæ Majoris. R.A. $11^{h}\ 11^{m}\cdot8$, Dec. $+32^{\circ}\ 13'$.

Mag. 4, 4·9. This is the famous binary ξ Ursæ Majoris. The latest elements are those just published in No. 1 of the Observations of the Oxford University Observatory:—T 1875·26, P 60·80 years, ☊ 100° 13′, γ 56° 40′, λ 235° 0′, e 0·41590, a 2″·580.

1877·29	294°·1	1″·68	Knott.
9·36	278 ·9	1 ·62	Gledhill.

Σ 1536=ι Leonis. R.A. $11^{h}\ 17^{m}\cdot6$, Dec. $+11^{\circ}\ 12'$.

Mag. 3·9, 7·1. Colours, A yellowish, B blue.

Probably a binary system.

1877·36	68°·4	2″·54	Dembowski.

Both the magnitude and the colour of B are probably variable.

Σ 1670=γ Virginis. R.A. $12^{h}\ 35^{m}\cdot6$, Dec. $-0^{\circ}\ 47'$.

Mag. 3·3. Variable. A famous binary.

1877·43	338°·4	4″·96	Flammarion.
9·36	338 ·6	5 ·00	Gledhill.

Such stars as γ Leonis and γ Virginis are best measured before or very soon after sunset. JOSEPH GLEDHILL.

Mr. Edward Crossley's Observatory,
Bermerside, Halifax.

Meteor Notes for June.

EVENING observations may be made between the 8th and 25th, and in the mornings during the last half of the month. Shooting-stars are often abundant in June. The average positions of the chief radiants visible are given as follows in Mr. Greg's catalogue (1876):—317° +62°, 299° −7°, 301° +8°, 282° +60°, 264° −11°, 285° −13°, 235° +23°. There is also a radiant at the end of the month at 168° +55° (near β Ursæ Majoris), and others should be looked for near η Serpentis, ζ Cygni, and ε Sagittæ. Zezioli observed many meteors on several nights in June 1867. On the 15th, between 10^h and 13^h, he noted an extraordinary number falling amongst the stars ν, θ, η, ζ Draconis. On the 16th before and after 10^h he saw 20 others, hardly visible and with directions imperceptible between Lyra and Ursa Minor, but chiefly amongst the stars β, γ, ν, and ξ Draconis; on the 19th, 10^h to 11^h 30^m, he numbered more than 26 falling stars, and the point of departure seemed η Ursæ Majoris. Later the same night he recorded 15 falling stars, amongst which were two of the 1st mag., but of brief trajectory (perhaps 1°); the others small, and yet with short courses, started between λ Boötis and η Ursæ Majoris, from 12^h to 13^h. Between 13^h 5^m and 13^h 20^m he registered the paths of 16 meteors (1 per minute) and saw many others, indicating an exceptional abundance. Zezioli also recorded many shooting-stars on June 29, between 11^h and 13^h.

Large meteors may be expected on June 6–7, 12, and 29–30. On June 7, 1878, several fireballs appeared in the evening twilight.

Recent Observations.—Mr. H. Corder at Chelmsford saw 1 *Lyrid* in an hour's watch on the evening of April 18. On the 19th he watched from 9^h till 15^h; and out of 47 meteors seen, only 13 were conformable to Lyra, and not more than 3 were seen in the same hour, and they fell off towards the morning. The next evening (20th) there was an hour pretty clear; and of 8 meteors, 2 were *Lyrids*. Thus 16 *Lyrids* were seen altogether; 7 left narrow streaks. As to colour, the 1st mags. were white, whilst the 2nd were yellow generally. Radiant diffuse, probably at 275° +37°, but it seemed double this year. Concerning other meteors, one was imperfectly glimpsed by Mr. Corder at 11^h 10^m on April 19 = Sirius, at 158° −24°. It seemed directed from Leo. There was a radiant of 8 short meteors at ζ Ursæ (200° +55°), another at Arcturus (210° +23°), and a large region of radiation in Draco, but no marked point; there were several almost stationary meteors in that constellation. Also a suspicion of a shower from the N. part of Serpens.

Prof. Herschel, at Newcastle-on-Tyne, saw no Lyrids in half an hour's watch on April 20 (11^h to 11^h 30^m); but of 3 small meteors seen, one very short, between Corona and the head of Serpens, confirms the radiant suspected by Mr. Corder at the latter point. The other two were from Draco.

Mr. V. Cornish, at Welling in Kent, registered 8 meteors between 9^h and $11^h\ 15^m$ on April 19, including several *Lyrids.* At $9^h\ 21^m$ a very fine meteor = ♀, but low down and in haze, was noted from 45° +49° to 44° +41°. At $11^h\ 15^m$ one = Arcturus fell from 212° +44° to 217° +30½°, and evidently belonged to Mr. Corder's radiant at ζ Ursæ, which is a strong shower in April and May, and No. 55 of Mr. Greg's catalogue, with an average centre at 204° +56°. Mr. Cornish also observed 3 *Coronids* with a radiant at 246½° +20½°.

The writer at Bristol observed a fine meteor > 1st mag. on April 18, $12^h\ 50^m$, remarkable for its extreme slowness of motion. Path from 310° +51° to 263° +48°. Length 30°, which it traversed in about 6 seconds, casting off a bright train of yellow sparks; radiant probably in Lacerta. A small meteor seen at the same time had an extremely short path in Serpens, and near a radiant at about 234° +11°. On April 20 a watch was maintained from $10^h\ 45^m$ to $11^h\ 30^m$ (after which a dense fog obscured the stars), and 15 meteors were traced, of which 4 or 5 only were *Lyrids.* Three bright and rapid meteors, leaving streaks, were seen apparently from a radiant near β Cygni at 286° +23°, which had previously been deduced as a shower of some intensity from Dr. Weiss's Austrian observations, with a centre at 288° +22° (20 meteors, April 19–23). It is further confirmed by a stationary meteor (2nd mag.) seen by Palisa at Troppau on 1870, April 19, at 289°·4 +26°·4.

A bright long-pathed meteor was observed by Prof. O'Reilly at Dublin, on April 18, at $8^h\ 52^m$. It shot from Arcturus to Procyon (about 65°) in 5 or 6 seconds.

A large meteor was seen at many places on April 21 at $8^h\ 43^m$. Mr. J. L. Stothert of Bath writes that "whilst walking from Tiverton station to Bath in a direction nearly E., my attention was arrested by a sudden brightness in the sky. On turning round I witnessed the explosion of a very fine meteor, which burst about 3° or 4° S.E. of α Canis Minoris, the course leading from α Leonis." The same meteor is described in a Bristol newspaper:—"At 17^m before 9 o'clock, a meteor of unusual size and brilliancy was seen from many points of this city. It descended from a considerable altitude in the southern sky obliquely towards the west. It was as bright as Venus, of a beautiful blue colour, and left behind a train of orange-coloured fire."

W. F. Denning.

CORRESPONDENCE.

To the Editor of 'The Observatory.'

Spectrum of Brorsen's Comet.

Sir,—

I have just this evening, after a long interval of cloudy weather, obtained some observations of the spectrum of Brorsen's Comet rather more satisfactory than those of April 2 and 3, which I reported in 'Nature.' The new observations confirm the former ones completely.

The central band is the brightest, the most refrangible one is next, and the least refrangible one so faint as to admit of not much more than an estimation of its position, though three different settings agreed very fairly. The spectroscope is the same used April 2 and 3—one prism, with scale and movable pointer adapted to feeble light.

The least refrangible edge of each band was observed, being pretty definite, and the wave-lengths come out as follows, expressed in millionths of a millimetre:—468 ± 2, 517 ± 1, 558 ± 3.

A direct comparison with the flame of a Bunsen burner showed an agreement between the bands of the comet-spectrum and three of the bands of the flame-spectrum, the coincidence appearing to be perfect up to the limits of the instrumental power.

For Encke's comet in Dec. 1871, I got 470·2, 517·45, and 557·5, so that there would seem to be little, if any, doubt as to the identity of the spectra of the two comets.

I am, Sir, yours very truly,
C. A. Young.

Princeton College Observatory,
New Jersey.

Visibility of the Corona in full Sunlight.

Sir,—

I have been much interested in the discussion which took place at the March meeting of the Royal Astronomical Society in regard to the visibility of the corona. In this connexion permit me to say that Mr. Upton, who observed the late eclipse of the Sun with me at Schuyler, near Denver, Colorado, saw the Moon's limb $5^m\ 8^s$ before the beginning, and $7^m\ 13^s$ after the end of totality. I myself saw the coronal rays distinctly for 23 seconds after totality ended. A flood of light then suddenly filled the field of my telescope, and the rays could be no longer distinguished, although the Moon's limb was sharply defined. I have no doubt that if I had moved the telescope so as to throw the limb of the Moon to the edge of the field I should not have been disturbed so soon by the sunlight, and might have seen the coronal rays for at least some seconds longer.

I am, Sir, very respectfully yours,
Ormond Stone.

Cincinnati Observatory,
1879, April 15.

Method of Least Squares.

[With reference to the question raised in M. D'Abbadie's letter in last No., the Astronomer Royal has forwarded to us the following letter, which he addressed to M. Le Verrier some years ago.]

My Dear Sir,—

In the last number of the 'Association Scientifique' which reached me, I read your denunciation of the method which is unfortunately called the Method of Least Squares. The subject is very important at the present time (in reference to the Transit of Venus), but it is still more important as a general method applicable to all discussions of complicated problems which may occur in future; and for this reason I take the liberty of addressing you upon it.

Now, my honoured friend, permit me to express my perfect confidence that, on full consideration, you will entirely change your opinion on this subject. You are, at present, in the same position in which I was many years past. At that time, I had seen the method "of least squares" employed in a most careless manner, leading to results which would evidently be erroneous; and I *published my opinion* that the *method deserved no confidence.* Subsequently I became better acquainted with the Theory of Probabilities (I have written a small book on it, which perhaps you have not seen) and I learned what was the point omitted in the delusive applications which I had remarked in early instances. And I am now certain that when the Theory of Probabilities is properly applied, it leads distinctly and incontrovertibly to that form of application whith has received (unfortunately) the name of "least squares."

I will state to you the principal heads of reasoning upon which it depends.

1. It is taken as foundation of the system that, if x be the magnitude of an error in determination of a simple observed quantity, the number of errors which occur between x and $x+\delta x$ is proportional to $\delta x \times e^{-\frac{x^2}{c^2}}$, where c is the "modulus," a number peculiar to each system of errors. It matters not whether this formula is derived from Laplace's reasoning: it is certain that it represents facts in general. The "probable error" is nearly $= \frac{c}{2}$.

2. This law being understood, it is easily shown that (see my book), if we have two quantities subject to error, the two quantities being *absolutely independent*, then calling these quantities p and q, the (probable error of $\overline{p+q})^2$ or the (probable error of $\overline{p-q})^2 =$ (probable error of $p)^2 +$ (probable error of $q)^2$. This is the fundamental theorem, which enters into every succeeding step of investigation.

3. The whole object of the Theory of Combination of Observations is, to discover what is the rule of combination which will produce a result whose probable error is the smallest possible. I must here omit some steps, and here state only the result.

4. Every equation connecting the various elements which are to be deduced from an observation (which has an estimated probable error) is to be so multiplied that the probable error of the equation $=1$ (abstract number). Then it is demonstrated by the theory of combination of observations (*not* by any consideration

of least squares) that the peculiar process known as the "method of least squares" does produce those values of the elements sought *whose probable errors are the smallest possible.*

Now, my valued friend, let me intreat you to consider this matter very carefully (the result will certainly be that you will adopt the Theory of Combination in its completeness) and that you will publish your mature convictions upon it. It cannot be permitted that a great error should be promulgated on the authority of a great man without receiving contradiction; and it is infinitely better that the correction should be made by you than by any other person.

I am, my dear Sir, most truly yours,
G. B. AIRY.

Royal Observatory,
Greenwich,
1875, February 5.

The Sadler-Smyth Scandal.

SIR,—

I do not wish to enter into a controversy respecting the Sadler-Smyth Scandal, as I do not thoroughly understand the merits of the case. I am, however, very pleased that, at the last meeting of the R.A.S., everything terminated so satisfactorily. But my object in addressing you is to offer a few remarks upon the concluding part of Mr. Chambers's letter. He there says, "I think it well merits the consideration of the Fellows how far, even on general grounds, the present Council is a fairly representative body deserving of confidence. To my mind it is not such: an Astronomical Society ought to be governed by working Astronomers, and admission to the governing body should go in some degree by seniority. . . . . . Mr. Sadler, having been elected Fellow in June 1876, has clearly no claims by seniority: as to his claim by merit, I need, under the circumstances, say nothing. By some kind of juggling, the mysteries of which are unknown to me, most of the influential Astronomers of England seem to have been juggled out of the Council and a batch of nobodies juggled in."

1. Now, Sir, I should like to ask Mr. Chambers what there is in the present Council which makes it a body not deserving of confidence. It was duly elected at the Annual Meeting in February last, and has done nothing, as far as I am aware, deserving of condemnation. And if any Fellow who was not a nominee of the late Council had been desirous of a seat on the board, all he had to do to become a Candidate was to be nominated by two Fellows of the Society (see Bye-Law 8, Sect. III.). As to the governing body being elected from the Fellows according to seniority, Mr. Chambers must first obtain the consent of the Fellows at the Annual General Meeting, or at a Special General Meeting, to alter Bye-laws 8, 9, and 10 (Sect. III.)—a course to which I for one should not feel disposed to agree, as there are many things, besides a practical

knowledge of Astronomy, which, in my opinion, qualify for serving on the governing body.

2. Again, Mr. Chambers's remarks about "juggling," "mysteries unknown" to him, "juggling nobodies in" and "influential Astronomers out" are very uncalled for and ought to be withdrawn. They, in my opinion, reflect upon the late Council in the preparation of the list of Nominees, upon the Fellows who attended the Annual Meeting, and upon the Scrutineers who counted up the votes. Now, Sir, I saw no "juggling" whatever, nor did I notice any thing mysterious in conducting the election of the present Council. The late Council duly prepared the lists; every thing at the Meeting was done above board; and, as one of the Scrutineers appointed by the Meeting, I can most positively deny that either "juggling" or "mystery" was practised in the counting of the votes. I think, therefore, that Mr. Chambers should be called upon to substantiate or withdraw his charges.

3. Finally. Will Mr. Chambers kindly inform the Fellows, for their guidance at the next election, who the influential Astronomers are who have been juggled out of the Council, and, at the same time, give the names of the batch of nobodies who have been juggled in?

I am, Sir, yours faithfully,
CHARLES HEAVEN.

The Vicarage, Tring,
May 19th, 1879.

NOTES.

COLOURS OF DOUBLE STARS*.—M. Niesten, of the Brussels Observatory, has recently discussed the question of a relation between the colours of double stars and the revolution of the companion. A somewhat analogous relation has been suspected in the solar system between the revolutions of the planets and Sunspots, and with these have been connected supposed periodic changes of colour in some of the planets. The question is full of difficulties, on account of the uncertainty of colour-estimations by various observers; but M. Niesten has collected the data available for 20 binary stars, in which changes from white to yellow or orange are indicated in the primary star, with more marked variations of hue in the companion, and has arrived at the following conclusions:—

1. That in systems in relative motion, whether rectilinear or orbital, especially in those of short period, the two components have ordinarily the same hue, yellow or white.

2. That the primary is white or pale yellow when the companion is at its periastre, and yellow, golden yellow, or orange at other times.

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* Bulletins de l'Académie Royale de Belgique, Tome xlvii. No. 1.

3. That the companion exhibits the same changes of hue and often in a more marked degree, being in most cases white at its periastre, like the primary.

4. That in optical couples, the companion is almost always blue.

This last circumstance might, according to M. Niesten, arise from the absorption of a great thickness of aeriform fluid existing in space, such minute optical companions being supposed to be situated at the extreme confines of the visible universe. M. Niesten, however, assumes that our atmosphere gives a blue colour to distant objects, whereas the opposite is really the case, as shown by the redness of the setting Sun. It is true that distant mountains show an intense blue in the shadows, but that is simply because the transmitted light is so faint that it is overpowered by sunlight reflected from the particles of vapour in the air. This latter is blue, since the air absorbs the blue rays and therefore also emits or reflects them. High lights in the distance and, in general, bright objects seen through a great thickness of moist air have an orange or reddish tinge.

M. Niesten gives a table showing the distribution in zones of 1ʰ in R.A. and 10° in Dec. of stars with blue companions, from which it appears that they are found chiefly in the constellations of Lyra and Cygnus and of Orion. The numbers which M. Niesten has given for each zone of 10° of declination should, in strictness, be divided by the area of each zone in order to make them comparable, the areas diminishing of course from the equator to the poles.

Thus we have (the last zone being of 5° breadth only):—

N.P.D.	15°	25°	35°	45°	55°	65°	75°	85°	95°	105°	112½°
No. of Stars ...	5	7	14	8	26	22	21	24	19	7	5
Frequency......	19	17	20	10	29	23	21	24	20	8	11

The first line gives mean N.P.D. of zone, the second line the number of stars as stated by M. Niesten, and the third the number × sec. N.P.D. representing the frequency. M. Niesten's conclusion is not materially affected by the change, though the two maxima at 55° and 85° become much less marked.

WASHINGTON OBSERVATIONS, 1875.—The present volume is in the same form as its predecessors; we can here give only a brief notice of the mass of valuable observations and investigations which it contains. In the section of the Introduction devoted to the Transit-Circle, we find a description with illustration of Prof. Eastman's ingenious apparatus for determining personal equation, brought into use in 1875. In the same year a new reflection apparatus was fitted up, by which stars can be conveniently observed by reflection and direct at the same transit, but only between the limits 18° and 25° Z.D., so that the important R. − D. correction is even now not fully determined. However, it is a great improvement on the former mercury trough, which was so defective and awkwardly constructed that it was used but rarely, and the number of stars observed by reflection since 1868 is consequently too small to obtain a trustworthy correction to the zenith-point. The other instruments noticed in the Introduction are the Meridian Transit, the Mural Circle, and last, but not least, the 26-inch Equatoreal. Then follow the observations with these instruments and the results, including a catalogue of 800 stars and the comparison between observed and tabular places of the Sun, Moon, and Planets. With the 26-inch Equatoreal there are measures of the satellites of Saturn, Jupiter, Uranus, and Neptune, and of a few double stars. Appendix I. is a determination of the longitude of Sayre Observatory, Lehigh University, Bethlehem, Pennsylvania, by Prof. Eastman, in cooperation with Prof. Doolittle. Appendix II. consists of Prof. Newcomb's Researches on the Motion of the Moon (noticed in No. 23 of 'The Observatory'). Appendix III. is a discussion by Lieut.-Comm. Davis of chronometer rates as affected by temperature; and the volume concludes with Prof. Asaph Hall's memoir on the satellites of Mars, of which we have already given an account ('Observatory,' No. 19).

GREENWICH OBSERVATIONS, 1876.—This volume contains the regular observations with the Transit-Circle and Altazimuth, with discussion in the Introduction of the corrections of various kinds required in their reduction, a catalogue of 1044 stars observed in the course of the year, comparison of the observed and tabular places of Sun, Moon, and Planets, occultations of stars, and phenomena of Jupiter's satellites. All these are in the form which was arranged by the Astronomer Royal in 1835, and which has been adhered to ever since. The Spectroscopic results which follow are a recent addition. They consist of observations of solar prominences, spectroscopic determination of the rotation of the Sun, and of motions of approach or recession of 32 stars, of the Moon and of Venus, spectra of variable stars and of planets, and spectrum of hydrogen at various pressures.

The Nine-year Catalogue of 2263 stars forms an appendix to the volume, and is also published separately. The Introduction contains discussions of the systematic errors in R.A. and N.P.D., the former being exhibited in curves. The corrections to R.A. (not applied in the body of the Catalogue itself) are derived from a comparison with observations of groups of clock-stars extending over 12 hours at least, and of circumpolar stars, and thus a table of standard R.A.'s of clock-stars has been formed. In the N.P.D.'s the corrections for error in the micrometer-screws (caused by wear) and for co-latitude have been investigated and applied in the Catalogue.

In an Addendum further corrections are given to reduce the N.P.D.'s to Bessel's system of refractions, as it appears, from a comparison with the Cape and Melbourne catalogues, and from the ecliptic investigations, that the new refractions introduced by Mr. Stone in 1868 do not represent the observations of southern stars or of the Sun. The co-latitude also requires to be increased from 38° 31′ 21″·40 to 38° 31′ 21″·90 (its old value), in consequence of the increase in the refraction.

A new minor planet of the twelfth magnitude (No. 195) was discovered by Herr Palisa at Pola on April 28.

Another, of the tenth magnitude (No. 196), was discovered by Prof. Peters at Clinton, New York, on May 17. It has been since observed by Major Tupman on May 19, 20, 21, and 25. Its position on May 25 was :—R.A. $12^h\ 15^m\cdot7$, Dec. +6° 25′·5, with a daily motion of 2′ south in declination. The motion in R.A. is very small, 3^s retrograde, the planet being near its stationary point.

Another minor planet of the twelfth magnitude was discovered by M. Palisa at Pola on May 21. Editor.

Astronomical Memoranda, 1879, *July.*

Sun. July 1, sets $8^h\ 18^m$, rises $15^h\ 49^m$; July 31, sets $7^h\ 49^m$, rises $16^h\ 25^m$. There is an annular eclipse on July 18, invisible at Greenwich. The central line passes over Central Africa.

Equation of time:—Sun *after* clock, July 1, $3^m\ 30^s$; July 11, $5^m\ 10^s$; July 21, $6^m\ 8^s$; July 31, $6^m\ 10^s$.

Sidereal Time at Mean Moon:—July 1, $6^h\ 36^m\cdot7$; July 11, $7^h\ 16^m\cdot1$; July 21, $7^h\ 55^m\cdot5$; July 31, $8^h\ 35^m\cdot0$.

Moon. Full, July 3, $9^h\ 38^m$; Last Quarter, July 10, $20^h\ 54^m$; New, July 18, $21^h\ 6^m$; First Quarter, July 25, $22^h\ 36^m$.

Occultations:—July 3, $11^h\ 43^m$ B. A. C. 6490 Oc. R. 295°; July 4, $12^h\ 42^m$ B. A. C. 6889 Oc. R. 356°; July 11, $11^h\ 23^m$ 101 Piscium Oc. R. 305°; July 14, $12^h\ 16^m$ 36 Tauri Oc. R. 308°; July 28, $9^h\ 37^m$ Antares Oc. D. 152°, $10^h\ 7^m$ Antares Oc. R. 200°; July 31, $8^h\ 52^m$ B. A. C. 6699 Oc. D. 20°. The angles at disappearance (D.) and reappearance (R.) are reckoned from the *apparent* N. point towards the right hand round the Moon's circumference as seen in an inverting telescope.

The occultation of the bright star Antares on July 28 should be observed specially, and any phenomena of projection or clinging to the limb at reappearance at the bright limb carefully noted. At reappearance the *comes* (7 mag., pos. 276°, dist. 3″·7) may be seen emerging 6^s or 7^s before Antares, thus affording a brief opportunity of estimating its true colour.

Mercury is an evening star, at greatest elongation E. (27°) on July 27, when he sets in the W.N.W. about 1^h after the Sun.

Venus is an evening star, at greatest elongation E. (46°) on July 16. She sets in the W.N.W. at $10^h\ 32^m$ on July 1, and in the W. at $9^h\ 8^m$ on July 31. The illuminated portion of the disk is 0·564 on July 1, and 0·404 on July 31, and the corresponding diameters 20″·4 and 29″·0.

Mars is a morning star in the E.N.E., advancing through Pisces and Aries. He is in quadrature on July 22.

July 1, R.A. $1^{h}\ 1^{m}\cdot 2$, Dec. 3° 57′ S., tr. $18^{h}\ 23^{m}$, rises $11^{h}\ 59^{m}$.
July 31, 2 13·0, 10 42 N., 17 37 10 38

Jupiter is a morning star in the E.S.E., stationary in Aquarius.
July 1, R.A. $22^{h}\ 58^{m}\cdot 1$, Dec. 7° 53′ S., tr. $16^{h}\ 19^{m}$, rises $10^{h}\ 55^{m}$.
July 31, 22 53 ·3, 8 31 S., 14 16 , 8 55

Jupiter's Satellites:—July 1, $13^{h}\ 9^{m}$ iii. Ec. R., $14^{h}\ 38^{m}$ iii. Oc. D.; July 3, $14^{h}\ 14^{m}$ ii. Sh. I.; July 5, $13^{h}\ 50^{m}$ i. Sh. I., $14^{h}\ 24^{m}$ ii. Oc. R.; July 6, $14^{h}\ 30^{m}$ i. Oc. R.; July 7, $11^{h}\ 47^{m}$ i. Tr. E.; July 8, $12^{h}\ 24^{m}$ iv. Sh. I.; $13^{h}\ 40^{m}$ iii. Ec. D.; July 12, $11^{h}\ 43^{m}$ iii. Tr. E., $11^{h}\ 48^{m}$ ii. Ec. D.; July 13, $12^{h}\ 56^{m}$ i. Ec. D.; July 14, $11^{h}\ 8^{m}$ ii. Tr. E., $11^{h}\ 16^{m}$ i. Tr. I., $12^{h}\ 31^{m}$ i. Sh. E., $13^{h}\ 35^{m}$ i. Tr. E.; July 19, $11^{h}\ 24^{m}$ iii. Sh. E., $11^{h}\ 47^{m}$ iii. Tr. I., $14^{h}\ 23^{m}$ ii. Ec. D.; July 20, $14^{h}\ 51^{m}$ i. Ec. D.; July 21, $10^{h}\ 40^{m}$ ii. Tr. I., $11^{h}\ 40^{m}$ ii. Sh. E., $12^{h}\ 5^{m}$ i. Sh. I., $13^{h}\ 3^{m}$ i. Tr. I., $13^{h}\ 31^{m}$ ii. Tr. E., $14^{h}\ 24^{m}$ i. Sh. E.; July 22, $12^{h}\ 32^{m}$ i. Oc. R.; July 25, $11^{h}\ 16^{m}$ iv. Sh. E., 14^{h} 59 iv. Tr. I.; July 26, $11^{h}\ 46^{m}$ iii. Sh. I.; July 28, $11^{h}\ 24^{m}$ ii. Sh. I., $13^{h}\ 1^{m}$ ii. Tr. I., $14^{h}\ 0^{m}$ i. Sh. I., $14^{h}\ 17^{m}$ ii. Sh. E., $14^{h}\ 48^{m}$ i. Tr. I.; July 29, $11^{h}\ 14^{m}$ i. Ec. D., $14^{h}\ 18^{m}$ i. Oc. R.; July 30, $10^{h}\ 35^{m}$ ii. Oc. R., $10^{h}\ 47^{m}$ i. Sh. E., $11^{h}\ 33^{m}$ i. Tr. E.

Saturn is a morning star in the east, stationary in Pisces.
July 1, R.A. $0^{h}\ 59^{m}\cdot 6$, Dec. 3° 46′ N., tr. $18^{h}\ 20^{m}$, rises $11^{h}\ 57^{m}$.
July 31, 1 2 ·0, 3 52 16 24 10 1

	Outer Ring.		Inner Ring.		Ball.
	Maj. Axis.	Min. Axis.	Maj. Axis.	Min. Axis.	Diam.
June 29......	39″·33	7″·02	26″·15	4″·67	15″·8
July 19......	40″·74	7″·40	27″·09	4″·92	16″·3

The south side of the rings is visible, the elevation of the Earth above their plane being 10° 17′ S. and 10° 28′ S. on June 29 and July 19 respectively, and of the Sun 7° 36′ S. and 7^{c} 54′ S.

EDITOR.

Publications received:—E. Dunkin, *Obituary Notices of Astronomers* (London, 1879)—B. Hasselberg, *Bearbeitung der Photographischen Aufnahmen in Hafen Possiet* (St. Petersburg, 1877)—B. Hasselberg, *Studien auf dem Gebiete der Absorptions analyse* (St. Petersburg, 1878)—E. von Asten, *Theorie des Enckeschen Cometen* II. (St. Petersburg, 1878)—W. Valentiner, *Astronomische Beobachtungen zu Mannheim*, III. *Micrometrische Ausmessung von Sternhaufen* (Karlsruhe, 1879)—L. Niesten, *L'Aspect Physique de la planète Mars* (Bruxelles, 1878)—Th. Bredichin, *Remarques générales sur les Comètes* (Moscow, 1879)—Rev. E. Ledger, *Intra-mercurial Planets*, Gresham Lecture, Feb. 14, 1879 (Cambridge Univ. Press, 1879)—Adm. Sir C. Shadwell, *Notes on Interpolation* (J. D. Potter, price 2*s*.)—J. Tebbutt, *Correction to Longitude of Sydney Observatory* (Roy. Soc. of N. S. W. 1878)—G. H. Darwin, *Precession of Viscous Spheroid and Remote History of the Earth* (Proc. R. S. 1878, Dec. 19)—Edward Rigg, *Compensation of Clocks, Watches, and Chronometers* (Society of Arts, 1879, March 12)—Rev. G. T. Carruthers, *New Solar Elements* (Nagpur, 1879). EDITOR.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

No. 27. JULY 1. 1879.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, 1879, June 13.

Prof. Cayley, M.A., F.R.S., &c., in the Chair.

Secretaries: Mr. J. W. L. Glaisher, M.A., F.R.S., and
Mr. A. Cowper Ranyard, M.A.

Mr. Glaisher reported that a number of presents had been received during the month, the only ones calling for special mention being two forms of apparatus, presented to the Society by Mr. Perigal, for combining circular motion.

Prof. Adams. I beg leave to present to the Society a paper by M. Souillart on the theory of Jupiter's satellites. In this paper, which is too full of mathematics to be read before the Meeting, M. Souillart undertakes to investigate anew the theory of Laplace, obtaining the results, however, by a different method. In such a complicated theory as that of Jupiter's satellites, it is very important that we should not be dependent on one method only. Getting out the same results by different methods affords by far the best check upon the accuracy of the work. Laplace, in his 'Mécanique Céleste,' calculates the perturbations of the coordinates of the satellites directly. M. Souillart, in this memoir, finds the same results by the medium of the perturbations of the elliptic orbits; and his final result is to confirm Laplace's work. In one or two points he has carried the approximation further than Laplace. For instance, in the principal inequalities affecting the first three satellites, where Laplace has contented himself with the first approximation, M. Souillart has found sensible terms depending on the second and the third powers of the disturbing forces, which it is important to take into account. This paper may be looked upon as an answer to an appeal made by the Council in their last Annual Report, in which they expressed a hope that communications between this Society and foreign astronomers would be more frequent in future than they had been in the past.

The Chairman now introduced Dr. H. Draper, of New York, who was received with very hearty applause.

Dr. Draper. I feel very much honoured indeed in being allowed to address the Royal Astronomical Society to-night on a matter which has interested me so much as the subject of the existence of bright lines of oxygen in the spectrum of the Sun. I have prepared some written remarks, which I shall use as a sort of text; but I shall take the opportunity to diverge from them upon such points as seem necessary for further explanation. There are two or three matters which will require a little explanation on the blackboard; and I shall take the opportunity of passing round amongst you some photographs which will show the results of my working. I wish to speak of the steps which led to the discovery of oxygen in the Sun, and to describe some improvements in the electric and optical apparatus, and finally to show the results and their subsequent confimation.

In 1857, after the Meeting of the British Association at Dublin, some of the Members were, by the kindness of the Earl of Rosse, invited to visit the 6-feet reflector at Birr Castle; and in this way I enjoyed the advantage of seeing the method by which that great instrument was produced; and on returning to America in 1858, I was prompted to begin the construction of a metal speculum of 15½ inches aperture. Soon after, by the advice of Sir John Herschel, the metal was abandoned in favour of silvered glass. Several mirrors were ground and polished, and a telescope was constructed especially for photography, and good results were obtained which in 1863 culminated in the production of a photograph of the moon 15 inches in diameter.

Dr. Draper then proceeded to give an account of his spectroscopic researches in connexion with the bright lines of oxygen in the Sun. He had at first worked at the spectrum of nitrogen; but the diffuse bands of this element were not so favourable as the narrow lines of oxygen for determination of coincidences with bright lines in the solar spectrum. The research was commenced in 1873, and more and more powerful induction-coils were used till at length satisfactory photographs were obtained in 1876 by the use of a Ruhmkorff coil capable of giving a spark of 17 inches, in connexion with a gramme dynamo-electric machine of 500 candle-power driven by a petroleum engine of two horse-power. In this way a thousand 10-inch sparks per minute can be produced; and these being condensed by a battery of Leyden jars, a spark of great intensity is obtained between electrodes in air. Plücker's tubes, which were tried in the early experiments, were discarded, on account of the deposit formed on the glass which soon obscured the light, it being necessary to use iron as one of the electrodes for the sake of having its lines for comparison with the solar spectrum.

Since 1877 a great advance had been made by the introduction of several improvements. The most important of these is the "spark-compresser," to confine the spark between the poles. Ordinarily the spark pursues a zigzag course; but by means of two plates of glass it was confined to a narrow space in a plane through the slit. The glass, however, would not stand the powerful sparks, and eventually soapstone was substituted. In a block of soapstone a cavity is formed in the shape of a Plücker's tube, *i. e.* two chambers connected by a narrow passage, and one side of the latter is cut away so as to form a narrow chink directed towards the slit of the spectroscope. The electrodes (one of them being of iron) are introduced into the two chambers, which form reservoirs of intensely heated air, and thus keep up the incandescence. With this arrangement sufficient brightness of spark was obtained to allow of the use of a large dispersion with a spectroscope having two bisulphide-of-carbon prisms, with a collimator of 26 inches and telescope or projecting lens of 6 feet 6 inches. Thus photographs had been obtained on four times the scale of those published in 1877. The latter were 2 inches in length from G to H, whilst those now exhibited were 8 inches, or half the scale of Angström's chart. The difficulties to be encountered may be appreciated from the fact that the light obtained even with these powerful appliances is only equal to that of a single candle illuminating a slit of 1-100th inch, and this is enfeebled by being spread out into a spectrum more than 2 feet long. For the spectra of metals, the spark taken close to the electrode was found to be sixty times as bright.

Dr. Draper then described on the black-board the arrangement of his apparatus, in which the spark taken in the soapstone "compresser" was placed close in front of the slit, which is 1 inch long, and the Sun's rays thrown by a heliostat on a comparison-prism so as to give a spectrum just above that of air. The apparatus was adjusted till the coincidence of the iron lines in the spark and solar spectra was perfect, for any slight derangement would cause an apparent shift of the lines.

Dr. Draper handed round two of his original negatives, and distributed a number of enlarged photographs 20 inches long, showing the solar spectrum from below G to beyond *h*, and the bright lines of oxygen and iron below.

Dr. Draper having resumed his seat amidst the applause of the Meeting, the Chairman said it was hardly necessary to express to him the thanks of the Society for his interesting paper.

Mr. Ranyard. After the reception given to Dr. Draper, I do not think I need say any thing about the importance of the research which he has undertaken. He has opened for us a method of studying the signification not merely of the dark lines of the solar spectrum, but of the interspaces between them. When Dr. Draper sent over his former photographs, which were on a much smaller scale, I thought that the probability of the proposition which he had laid down was very great indeed, amounting to some thousands to one; but I should like to point out how enormously the probability has been increased by his more recent experiments. If he has increased the dispersion four times, he has not merely increased the proof four times, but he has increased the value of every coincidence that he shows four times. On looking at the smaller glass photographs, which show the coincidences much more sharply than those which have been passed round, I counted 18 oxygen lines; and therefore the increase of probability on the present occasion is as 4^{18} to 1, a very enormous number, thousands of millions to one. There are two or three ways of looking at the probability of the proposition which Dr. Draper has laid down, namely, that the bright lines of oxygen coincide with the bright lines in the solar spectrum. In the first place, it will be found that in no case does a bright line of oxygen coincide with a dark line*. If the lines were thrown down at random on the spectrum, the chance that all the lines should lie opposite to bright parts or interspaces between dark lines of the solar spectrum would depend on the total breadth of the dark lines of the solar spectrum compared with the total length of the spectrum. As seen with a high-dispersion spectroscope, I should think one would be quite safe in estimating that there is a greater breadth of dark lines than of bright lines; and therefore the chance of any one line falling opposite to a dark interspace is more than one half; and that 18 lines should all fall opposite bright spaces, and none fall opposite dark spaces, would be more than 2^{18} to 1, a very high number indeed. In addition to this, there is the fact that the bright lines are certainly proportional in their brightness to the bright spaces opposite to which they come; in some cases very strikingly so. I would especially draw attention to the group of four lines close together; three of these are oxygen; and it will be seen that their relative brightness corresponds very exactly with the relative brightness of the corresponding interspaces. It will be noticed that some of the bright lines of oxygen do not appear to be quite of the same breadth as the bright interspaces; but this is no objection whatever to the theory of Dr. Draper; for there may be other bright lines in the Sun, lying side by side with the oxygen lines, which are as yet not matched with the bright lines of any other non-metallic body. Then it will be noticed that the sharpness of the edges is not in each case precisely the same; but it must be remembered that the solar spectrum is seen after having been absorbed and altered by its course through the atmosphere here and through the solar atmosphere, and different parts of it require different photographic exposures to bring them out. If after the examination of the photographs any body has any doubt, I think Dr. Draper would be very much obliged if they would state their doubts, and give him an opportunity of removing them.

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* [This statement is not in accordance with fact. At least two of the oxygen lines, as shown in Dr Draper's large photograph, fall on *dark* lines in the solar spectrum; another is so nearly coincident with a dark line, that micrometrical measurement of the original negatives alone can decide whether the coincidence is sensibly perfect; and there are several other cases which are at least doubtful. Mr. Ranyard's calculation of probabilities, being based on these and other unwarranted assumptions, thus falls to the ground.—Ed.]

Mr. Christie. Dr. Draper has very kindly given me an opportunity of examining his very beautiful photographs; and I cannot help expressing very great admiration at the splendid results which he has obtained in this research. (Applause.) He has produced photographs of the spectrum of oxygen which certainly, so far as my rather limited acquaintance with the subject goes, are far superior to any thing that I have seen; but that is quite another question from *proving* the existence of bright lines of oxygen in the Sun. I am afraid it is a very difficult question which he has got to tackle; and certainly if it could be proved, I think these photographs would go a very long way towards it. I do not see myself, as he has expressed it, that very much more is to be done in the matter; but I am sorry to say, at the same time, that there appears to me rather a hopeless prospect in view, because we are placed face to face with a very great difficulty. Dr. Draper was very kind in explaining to me his views on the point; and he did not at all shirk the difficulties of the case. I hope I may be justified in alluding to the points merely for the sake of getting at the truth, and trying to find out what the real state of the case is. (Hear, hear!) According to Dr. Draper's view, I believe, the solar spectrum is composed of a continuous spectrum with dark absorption-lines and also of bright lines on it. The ordinary idea was that we had to deal with a continuous spectrum with dark absorption-lines, and there was sufficient difficulty in determining its character; but when we come to superposing on it bright lines *ad libitum*, when we find that we cannot distinguish in the solar spectrum the brightness of those bright lines from the general brightness of the spectrum, it is evident that the problem becomes much more complicated. I do not wish to express any opinion one way or the other. I am in the position of a sceptic; and, so far, it is a very unpleasant position perhaps to take up; but still it is the true scientific position until the thing is proved and established.

Now, taking the position of a sceptic, I examine these beautiful photographs and look at the question of coincidences, in which certainly there is great difficulty. Dr. Draper has followed the bright lines of oxygen from the oxygen spectrum down on to the Sun; and in that way I fully admit that opposite, or *nearly* opposite, every bright line of oxygen you can trace a space in the Sun, which is either free or else occupied by a very faint dark line which is not very striking. But if you start with the solar spectrum and try to pick out the bright lines of oxygen, I think Dr. Draper will admit that there is great difficulty. He gets over this difficulty by saying that besides the oxygen lines there are other bright lines—which is a perfectly legitimate explanation; but, at the same time, until we have determined those other bright lines we can hardly consider that the existence of any bright lines at all is proved.

The difficulty, as it strikes me at the present moment, is this—we have in some cases a tolerably broad interspace between the two dark lines in the Sun, which is sensibly of a uniform tint. Opposite one portion of that space there is a bright line of oxygen; but the coincidence cannot be established with very great certainty on account of the difficulty of instrumental adjustment. But there remains, so far as our present condition of experimentation goes, another part of the space which is unappropriated, the remainder of that space being equally bright; and we have got to account for that in some way. If it is not the continuous background of the spectrum, it must be a bright line of some other substance. But there is the difficulty that the substances at our disposal are rather limited. And on account of the changes in the spectrum, induced by the increased temperature necessary to get a sufficiently bright light, Dr. Draper has found it practically impossible to apply the same reasoning to other substances; at any rate he has not done so at present. So that at the present time we are in this position—we have a number of lines of oxygen, which by hypothesis do not coincide with any of the principal dark lines upon the Sun, because it has been recognized by most spectroscopists that there are no coincidences established between the dark lines in the solar spectrum and the bright lines of oxygen; from which it follows that the bright lines of oxygen must fall somewhere or other on the interspaces between the dark lines. The whole question turns upon this—whether these bright lines of oxygen fill up these interspaces completely, and fall exactly in the middle of them, or whether they do not, and also whether the interspaces have the same general character of gradating towards the edges as the oxygen lines do. Dr. Draper meets the objection of absence of fuzziness at the edge by saying that the edges are cut off by the dark lines; but there may be some difficulty in accepting that hypothesis; for on calculating the probability that a fine dark line should in every case fall in exactly the right place to cut off the edges, we should find it very small indeed. I think, however, the theory of probabilities is very dangerous in cases of this kind, for it is apt to cut either way. You have only got to make your assumption to suit, and by a little slippery reasoning you bring out the result you desire to arrive at. My only object in making these remarks is to point out the great difficulties which beset the question, without expressing, however, any opinion on it. And really when I come to look at it in that point of view, it seems to me that if we accept this theory, that the solar spectrum is made up of a continuous background with dark lines and bright lines on it, we shall not be able to establish any thing whatever about the solar spectrum. We cannot assert that there are dark lines corresponding to the ordinary accepted lines of metals, because they may be simply interspaces between the bright lines, and we shall be completely in a fog about it. That, as it strikes me, is the present state of our knowledge. In order that we may advance beyond it, I think it is desirable that the ground should be cleared, and therefore we should have this point discussed and fairly considered as to what we are to look upon as the true state of the solar spectrum.

Mr. Proctor. There are one or two points which I am not sure that Mr. Christie has taken fully into account; one of which is the evidence derived from the utter absence of any dark lines corresponding exactly with one of those bright oxygen lines [see note, p. 68]. With regard to the general character of the solar spectrum, I should very much like to hear the opinions of those who have studied the matter, especially of one or two who are present to-night as visitors, as to the way in which the solar spectrum is really constituted. It seems to me that the idea that there is a simple continuous background is one for which we have no evidence. All the evidence we have tends to show that the solar spectrum is made up of a number of gaseous spectra (that is to say, of elements in a gaseous state), some of them, owing to the intensity of the heat, having broad bands instead of narrow well-defined lines. That, I think, is the nature of the background. I am not sure whether many have considered the bearings of the research by Mr. Croll of Glasgow, as to the evidence which the Earth affords us of the age of the Sun. It appears from that evidence that the Sun must have been at work for more than 80 millions of years, whereas the whole amount of force that the Sun could have derived from contracting to the present size of its photosphere would suffice for barely 20 millions of years at the present rate of giving out energy. There is one way of getting over this difficulty, viz. that the Sun has been formed by the conflict of several stars which were originally rushing through space with enormous velocity; but I am not inclined to accept this view. I think rather that we must suppose the Sun to be greatly compressed at the centre, and the real mass to lie far below the photosphere. Not only have we to study what there is at the surface, but for thousands and tens of thousands of miles below the surface. We have to penetrate by spectrum analysis to these gaseous depths. It seems to me that the evidence obtained by Dr. Draper points to this, that oxygen is so important an element of the Sun, and constitutes so enormous a part of the Sun's mass, that its spectrum prevails, so to speak, over all the other gaseous spectra, and indicates its presence by a coincidence of the lines which the oxygen is able to emit with the bright part of the solar spectrum. If so, it would be almost hopeless to seek for the evidence as to any other elements from their bright lines. It seems to me that there is an utter absence of any negative evidence. There is no case where a dark line of the solar spectrum agrees with a bright line of oxygen; and that affords, to my mind, very strong evidence in favour of Dr. Draper's conclusion.

Dr. Gladstone. I have listened with the very greatest attention to Dr. Draper's interesting paper, and have looked at those remarkable photographs of his. At first when the photographs came over about two years ago I was not convinced by them: my mind remained in a state of suspended judgment. Certainly this increase of power which has been given to the instrument, producing results of much greater beauty and size, has very largely indeed increased the evidence in favour of this being a real coincidence. Whilst, however, I would not go so far as to express a strong opinion that no other explanation of the phenomena is possible, I do not think that Mr. Christie's objections to them will altogether hold water. Of course there is the great question, which has been alluded to, as to the nature of the solar spectrum—whether it is a bright background, coming of course from incandescent solid or liquid bodies, crossed by those dark absorption lines, or whether there is a mass of bright lines, perhaps very broad, crossed by those finer dark absorption lines. I do not know that we can be very positive upon the matter. I am not at all disposed to give up the idea that we have a continuous bright spectrum throughout under those dark lines; but is it not certain that we have bright lines as well as dark lines? I do not say this because of their appearance, that may deceive us; but we know that if we look at the edge of the Sun we see the hydrogen lines bright, and we see sometimes the magnesium, and the sodium, and the iron lines, and so on. When we look at any other part of the Sun it stands to reason that we shall be looking upon a certain amount of brightness which will at the same time unquestionably overlap the dark absorption lines. We see that it is so in spectroscopic observations of the spark; so there is no doubt that that difficulty which Mr. Christie has alluded to actually does exist to a certain extent; and yet I do not think that, after all, it would throw our positive knowledge into such utter confusion; because, however much of the bright part of the solar spectrum was due to bright bands superposed one upon another, I should have no doubt of the existence of magnesium vapour in the atmosphere of the Sun from the evidence of the *b* lines. Of course the fact that there are difficulties is of no force as an argument against the truth or falsehood of an hypothesis.

There is, however, one point which I should like to allude to. It has appeared to me that, if we are dealing with a gas like oxygen, it is very strange, seeing there is an enormous gaseous envelope round the Sun, that we do not find evidence of this gas entering it as hydrogen does; for in that case the oxygen lines should be seen sometimes in the chromosphere. It was of course understood that this was not the case, and this used to appear to me a serious objection to the theory. Dr. Draper has explained that he has looked for them and he cannot find them. He is going to look again and see if he cannot find those bright oxygen lines appearing at the edge of the Sun; but if he looks he will probably not find them, for the very reason why they appear as bright lines at all in the solar spectrum. The fact is, the oxygen must be far down in the Sun, and must not extend high up, else the oxygen in the cooler parts of the atmosphere would appear as dark lines and not as bright lines. For a thing to appear as a bright line and never as a dark line means this, that it really exists down below those great clouds of condensing vapours of iron, magnesium, and other things which we suppose to be upon the surface of the Sun. Now have we any reason to suppose that the oxygen would never get up above them? It appears to me probable. When we consider oxygen gas passing through those iron vapours at that temperature the oxygen would seize upon the iron, and the iron would fall down as oxide, and no oxygen would ever rise through these magnesium and other vapours into the upper part of the solar atmosphere.

Dr. Huggins. Though the Society has received the communication from Professor Draper with such cordiality, still I think it may not be amiss for me to call attention to the extremely honest and thorough way in which Dr. Draper's investigations have been carried out. (Hear, hear!) I had the great pleasure of his company in my house, and I went thoroughly with him into the details of his experiments, and I was really overwhelmed with the extreme amount of careful and cautious experimental arrangement which he has brought to bear upon the subject; and on that account I think it behoves us to receive his results with the greatest attention. Personally, I confess, I must ask to be allowed to suspend my judgment a little longer. If the Sun had favoured us more than it has done during the last few months, I might perhaps have got some light upon the subject. I have now a very powerful spectroscope with a very fine grating ruled by Mr. Rutherfurd's machine, and an especially suitable form of collimator and photographic arrangements; and if we had only had the Sun I should have been able myself to have investigated this part of the spectrum. It may be perhaps a misfortune on my part to be of a temperament which in such cases, especially in the department of physics in which I have worked myself, makes me to wish to look into the subject personally. I must say that before expressing a decided opinion either for or against the existence of oxygen in the Sun, I should like to have the opportunity of a little sunshine on the matter. But I do think that whatever view we take we should be thoroughly impressed with the extremely honest, cautious, and thorough way in which Dr. Draper has carried out his experiments. (Hear, hear!)

Mr. Proctor. These results are not what are sometimes called observations, but what the photographic eye has detected for us. I think even the most cautious cannot look at them without feeling that they are more than mere descriptions of an observer.

Capt. Noble. It certainly does seem to me, looking at these photographs impartially, that if we are to deny the evidence supplied by some of these coincidences, and notably by this group of four lines in one part, and another in another—if we are to accept Mr. Christie's dicta in their integrity, all evidence of the existence of elements in the Sun derived from coincidences must be worthless, and we are reduced to the intellectual condition of the negro metaphysician, who said

"Supposing I was you,
And supposing you was me,
And supposing we both were somebody else,
I wonder who we should be;"

and that, in truth, we have no positive evidence whatever for the existence of a single element in the Sun.

Mr. Ranyard. In answer to one or two objections raised by Mr. Christie, I should like to say a few words. I understood him to say that there were a good many bright spaces which were not matched. I should like to remind him how many dark lines there are which are not matched. Many more than three fourths of the dark lines have never been matched with any bright lines of metallic elements. And as to some bright lines falling opposite interspaces which are broader than the bright lines, he seemed to think that the probability was very great indeed against there being two bright lines undistinguishable in brightness close together upon the solar spectrum. There is a point in connexion with this which should be taken into account, viz. that the bright parts of Dr. Draper's photographs are very much over-exposed, and consequently small gradations of brightness will be obliterated—that is to say, you might be able to see a difference of brightness between two bright lines by the eye, and not in the photograph. Then he seemed to assume that if you begin the other way you ought to find the brightest part of the spectrum matching with the oxygen lines; but this assumes that oxygen gives brighter lines than any other element in the Sun. We have no reason to know that oxygen gives the brightest lines. With regard to the probability of there being a continuous background to the solar spectrum, I should like to remark that, as far as I am aware, all gases give out a faint continuous spectrum as well as bright lines. Theoretically this must be so; for however rare the gas may be, after the impact of two molecules there will be a certain time during which all wave-lengths are given out. So that, however much the pressure may be reduced there will be a continuous spectrum corresponding to the time immediately after impact. This continuous spectrum becomes very faint compared with the brightness of the lines as the pressure is reduced; but it seems probable, from mere *primâ facie* reasoning, that if the Sun is a purely gaseous body at the height of the photosphere (that is, in the parts we see), there will be a faint continuous spectrum which may be interfered with by absorption, and the spectrum may also be added to by bright lines given out, either from a lower depth or from a greater height. We have evidence that the same element at various heights in the Sun gives a spectrum in which the relative brightness of the different parts varies greatly. Thus we know that in the corona the 1474 line is much the brightest of the bright lines, whereas we know that when the 1474 line is seen in the absorption spectrum it is relatively a much fainter line; and that may be the case with the other elements which give bright lines, but does not appear to be so with oxygen.

Mr. Christie. With regard to the question of coincidences, every thing turns upon the exactness of the coincidence; and whether these coincidences are actually admitted or not, I for one am not prepared on the spur of the moment to admit that they are by any means perfect, or that there are no coincidences with dark lines. On looking at the photograph I should say that there are coincidences with some of the dark lines; but I admit that that is a matter of judgment, and I should be very sorry to express a decided opinion either way. As Mr. Ranyard has said there are some perceptible differences of intensity which photography cannot represent, I would simply wish to ask him whether he has examined the spectrum with his eye and found that to be the fact. There is one particular part in the neighbourhood of G which I have both examined and photographed, and I find that the photograph gives, as nearly as possible, the same representation as appears to the eye; that is, that the whole space is of equal intensity. I do not think myself that there is much difference between the photograph and the eye in limited portions; but the object of my remarks was rather to clear the ground, and to point out what it is that we are assuming when we predicate the existence of oxygen as giving bright lines in the Sun—not as disposing of the theory, but merely as pointing out the difficulties that we have to face, in order that they may be fairly met, and that we may consider the matter from this rather novel point of view. I should be very sorry to prejudge the question in any way.

Mr. Capron. I would ask Dr. Draper whether he has tried any experiments with the tube-spectrum.

Dr. Draper. A very great many.

Mr. Capron. The question seems rather complicated in this way, that we cannot deal with the spark-spectrum with any certainty—in fact, we might have reason to presume, on the contrary, that the spark taken in air would hardly agree with the spark, supposing we could take it, near the Sun's body. I mention this because I have photographed side by side two spectra, one a tube-spectrum of hydrogen, and the other a spark-spectrum of air. The tube-spectrum of hydrogen showed four hydrogen lines perfectly sharp and distinct, but only one of those lines was represented in the spark-spectrum. So that it is perfectly possible, as it seems to me, that certain lines may be absent in one or the other spectrum. We know that that would probably be the effect, and certainly that these lines might be present or absent according to the circumstances of pressure and density—circumtances that effect the medium through which the spark is taken.

Dr. Draper. In answer to the last remarks, I would say I have taken the oxygen spark-spectrum under a good many different circumstances. I began originally with tubes containing such compounds of oxygen as carbonic acid and the like; but the difficulty with those is that you are limited to rather a small dispersion, because you cannot get light enough to use a very large apparatus. Then the difficulty, in consequence of having an iron terminal so as to be sure that you are having a good coincidence, becomes very serious. I wanted to be sure that I was not in any way fitting the one spectrum to the other, but that the coincidence was such as really existed. So when I went to the spark-spectrum I arranged a contrivance at the back that would enable me to retain the oxygen and other gases between the terminals; and it was not until I had made a great many of these experiments that I became satisfied that oxygen, as it is present in the air, was a pretty fair exponent of what the oxygen spectrum would do. It seems to me that oxygen is an exceedingly favourable substance for a research of this kind, for its lines suffer less by great heat and great pressure than any other that I have experimented on. I might extend that by saying that when you come to consider the exaggerated pressure in the photosphere of the Sun, particularly if you get a little way in, and the enormous temperature, you must expect that the oxygen spectrum may be modified in some of its parts; certain lines may become wider or narrower. The probability is that with increase of pressure you would fancy that the spectrum became entirely continuous. These are speculative considerations. The part which I have taken is this, that inasmuch as I think I have fairly shown that the bright lines coincide with the bright spaces, as in many cases they do, the minor variations that exist may be fairly attributed to such a change of condition as is seen in the Sun compared with the spark that you get in your laboratory.

A remark was made by Dr. Gladstone that probably we may not succeed in finding in the chromosphere, even in a very low layer of it, the bright lines of oxygen. That is precisely what I hope it will come to. I am going to look as hard as I can. I should like to see them if they are there, but I shall not be dissatisfied if they are not.

Mr. Johnson read a paper describing a new method of controlling the driving-clock of an equatoreal, which he had used for about three months. It was constructed on one of the numerous automatic systems, and though not so perfect as the one which Lord Lindsay was using, was very cheap and very useful so far as it went, and might be very readily applied by an amateur.

Mr. Plummer, of the Oxford Observatory, read a paper by Professor Pritchard on the photographic semidiameter of the Moon.

Mr. Christie. Might I ask Mr. Plummer what is the mean discordance between the measures on the separate photographs of any one well-defined point on the Moon? It would be a matter of some interest as regards the accuracy in the determination of the position of a crater with reference to the centre of the Moon from different photographs.

Mr. Plummer. The mean discordance is under 1″.

Mr. Neison. I ask permission to postpone what I have to say upon the paper which Mr. Plummer has read until a subsequent Meeting of the Society, after having seen it in print. All depends upon the measures and also upon the means which have been adopted to eliminate the very great systematic errors due to the fact that there are inequalities in the Moon's limb which, under the same conditions of libration, invariably present themselves in the same manner, and so tend to throw very great systematic errors on the determination of the Moon's diameter taken in this manner. I should be glad, therefore, if the Society would allow me, at the next meeting, to say something further upon this matter.

Mr. Redhouse read a paper "On the knowledge of the Zodiacal Light possessed in the East from of old."

Mr. Knobel. With regard to the question of the False Dawn applying to the zodiacal light, it is very well known that on every Oriental astrolabe there is the line of dawn, and that line is marked about an hour and a half (unequal hours) before sunrise; and I should like to ask Mr. Redhouse if that means the line of the False Dawn or of the True Dawn; and, further, what is his authority for applying the terms the "False Dawn" and the "Wolf's Tail" to the zodiacal light?

Mr. Redhouse. The astrolabe only gives the time of the "true dawn." The "wolf's tail" is another name for the "false dawn"; another name is "the dark stripe." It is a very curious thing that what is called the "wolf's tail" is in the French language called "*chien et loup.*"

Mr. Downing gave the results of some investigations of his into the Washington refractions. As it appears from recent investigations that the refractions of Bessel's 'Tabulæ Regiomontanæ' unaltered apply fairly well to the Greenwich observations, it became a matter of interest to inquire whether the N.P.D.'s of the Washington Catalogue, which have been reduced with the refractions of the 'Tabulæ,' are in accord with observations made in the southern hemisphere. For this purpose the Washington places have been compared with those of the Cape Catalogue for 1860; and the differences Cape *minus* Washington (for large zenith-distances) are:—for Z.D. 75° −0″·80, for Z.D. 80° −1″·07, for Z.D. 83¼° −1″·55, and for Z.D. 85¾° −3″·85. It appears, therefore, that the tabular refractions must be diminished in order that they may apply to the Washington observations. Now, as far as Z.D. 85°, the mean refractions of the 'Fundamenta' are those of the 'Tabulæ' diminished in the proportion 1 : ·996718; and by substituting the mean refractions of the 'Fundamenta,' the discordances are reduced as follows:—at Z.D. 75° to −0″·13, at Z.D. 80° −0″·15, at Z.D. 83¼° −0″·05, and at Z.D. 85¾° −0″·62. If, then, the Washington N.P.D.'s were reduced with the mean refractions of the 'Fundamenta' and the meteorological corrections of the 'Tabulæ,' they would agree sensibly with the Cape observations.

As far as can be judged from this investigation, then, the same refractions do not apply at Greenwich and at Washington; and the difference may, perhaps, be explained by a different conformation of the atmosphere in the very different situations of the observatories. It is probably impossible to frame a system of refractions suitable to every locality; and it is a question whether all great observatories should use the same system (an arrangement which gives peculiar facilities for comparison of results), or whether each observatory should use its own system, based on its own observations, which would probably give a close approximation to the true places of the observed objects.

Mr. Christie. Mr. Downing's remarks appear to me to be of great interest and great importance, because, perhaps, there is nothing of greater importance at the present time in mathematical astronomy than this question of refraction. But I do not know that I quite agree with his conclusion that we must suppose that there are differences in the refractions at different places, due to the conformation of land and water, because the theory of refraction is not by any means certain when we get to those low attitudes below 75° of zenith-distance, and above that limit the systematic error of a particular circle will really mask any uncertainty that may arise. In the Greenwich observations there are similar discordances below 82° Z.D., showing that Bessel's Tables will not apply at Greenwich throughout from the zenith to the horizon, though they apply fairly well as far as 80° Z.D. It is therefore not quite certain that the same constant of refraction will not apply to Greenwich and Washington.

Mr. Knobel. I think the fact that Johnson at St. Helena found a difference in the refraction north and south of the zenith, according as he observed over the sea or over the land, is very important.

Mr. Neison. It is well known that the approximations introduced by Bessel did appear to fail when he got as far as ·80° Z.D. He has never estimated what would be the effect of the omitted terms in the approximations used by him, which may be very great at very low zenith-distances. And besides this, we have to deal with aqueous vapour; and the effect of that is so different from that exerted by the ordinary atmosphere that until this is calculated we cannot say for certain that the tables in the 'Fundamenta' would really apply satisfactorily to greater zenith-distances than about 85°.

Mr. Green. As the next opposition of Mars will occur before the next meeting of the Society, I desire to call the attention of the Society to the fact that it will be the most favourable opportunity for many years of examining the details of equatoreal continents.

Mr. Ranyard. I have a paper here from Mr. Russell, of Sydney, on the model of a working-clock which he has sent us over from Australia, giving a very perfect uniform rotary motion. Mr. Hilger will show this clock to any one who is interested in the matter after the meeting.

Mr. Talmage. May I ask for some information as to when we may expect the 'Eclipse Volume'?

Mr. Ranyard. It will be out before the end of the long vacation.

The Rev. D. Ace was balloted for and duly elected a Fellow.

The following papers were announced :—

J. Tebbutt. Observations of Brorsen's Comet, February and March 1879.

Dr. T. Oppolzer. Determination of the Longitudes of Berlin, Munich, Leipzig, Vienna, Paris, and Pulkowa.

C. Carpmael. On the values of the constants in the equation ${}_r\mathrm{A}_r x^{(r)} + {}_r\mathrm{A}_{r-1} x^{(2-1)} + \ldots\ldots + {}_r\mathrm{A}_t x^{(t)} + \ldots\ldots + {}_r\mathrm{A}_0 - y_x = 0$, obtained by the method of least squares, from the $n+1$ values of y_x when $x=0, 1, 2 \ldots\ldots n$; n being greater than r.

D. Gill. On the value of the Solar Parallax derived from the observations of Mars, made at Ascension during the opposition of 1877.

G. M. Seabroke. Spectroscopic observations of the motion of Stars in the line of sight, made at the Temple Observatory, Rugby.

E. J. Stone. Note on the Cape Catalogue of 12450 stars. (Extract from a letter to the Astronomer Royal.)

R. H. M. Bosanquet and *Prof. A. H. Sayce.* Preliminary note on the Babylonian Astronomy. The Babylonian Calendar.

A. Marth. Note on the transit of the Earth and Moon across the Sun's disk, as seen from Mars on Nov. 12, 1879, and on some kindred phenomena.

Rev. J. Pearson. On a new method of Mounting an Equatoreal.

The Meeting adjourned at ten o'clock.

A Tidal Theory of the Evolution of Satellites.

If a planet were formed of fluid it would assume a spheroidal shape under the influence of its rotation; but if a satellite revolves in a circular orbit round the planet in the plane of the equator, tides will be raised in the planet such that the spheroid will become distorted into an ellipsoid with three unequal axes, and the longer axis of the equator will always be directed towards the satellite. Thus the shape of the planet revolves along with the satellite, whilst each particle of fluid rotates with the planet, and has therefore to rise and fall twice in every revolution of the planet relatively to the satellite.

Now suppose that at any instant the fluid becomes endowed with friction; then those particles of fluid which happen at that instant to be in the equatoreal protuberances cannot fall so quickly as they did before the friction began to act. Thus the particles linger in the protuberances longer they than should do, and the particles which ought to be rising do not rise so quickly as they should. The obvious consequence of this is that the equatoreal protuberances and depressions are carried on along with the planet's rotation, and the longer axis of the equator now points in advance of the satellite in its orbit. This condition is perpetuated as long as the friction lasts. Tides of this kind are said to lag; and the angle of lagging may be defined as the angle between the longer axis of the equator and the direction of the satellite.

Another result of the supposed internal friction or viscosity of the fluid is, that the oscillations of the particles are impeded, and that they have not time to move so far as they would have done without friction. Hence if the fluid be viscous, the tides not only lag but are reduced in height.

A calculation of the effects of viscosity shows that in a mass like the earth we must suppose an enormous degree of internal friction, before the tides are much reduced or lag considerably. Indeed to have much effect in altering the tides, the matter constituting the planet must be of so stiff a consistency, that it would be called a solid for ordinary purposes. In the theory of which I am here giving a short account, a viscosity, which must be regarded as not at all great, is such that, if we conceive a slab of the material an inch thick to have one of its faces held fixed, whilst to the other face is applied tangentially a force of 13 tons to the square inch, then after the lapse of 24 hours the upper face will be shifted relatively to the lower through $\frac{1}{10}$ of an inch. As far as our knowledge of the interior of the earth goes, it might quite well be formed of matter of a stiffness comparable with this. There is, however, reason for believing that the earth is now very much more rigid than this; for if its mass yielded tidally to any great extent, then the ocean tides would be largely reduced; and although the theory of ocean tides is as yet very incomplete, yet we know that the height of the tides cannot be very much less than it would be on a rigid nucleus *.

Although the bodily tides are probably now inconsiderable, yet there is much reason to believe that the earth was once a hot molten mass; and therefore it is probable that, in the course of its cooling, the materials of which it is constituted must have passed through all degrees of viscosity, from approximate fluidity to its present high rigidity. This, therefore, seems to justify the theory of which we are speaking.

We will now return to the general theory of a viscous planet and its satellite; and, in order to obtain a convenient phraseology, the planet and satellite will henceforth be spoken of as the earth and moon.

The next point to be considered is one in which the lagging of the tides shows its most important influence. If the fluid forming the earth were frictionless, the equatoreal protuberances would be in a straight line with the moon; but in consequence of the friction they are not in that line, and therefore the moon exercises forces on the protuberances which tend to bring them back. Then since the protuberant axis of the equator always points in advance of the

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* See Sir W. Thomson's investigation of elastic tides (Thomson and Tait's Nat. Phil.), and my paper on viscous tides, Phil. Trans. 1879, part i.

moon, therefore the moon is always pulf-day, three of nearly a
earth's diurnal rotation, and conversely the eariod of the moon or
rating the moon. The first result of this is that he diurnal, and the
is retarded. The second effect is that the moon t the "lunar semi-
accelerated; this acceleration clearly will prevent
moving in a path of so sharp a curvature as before, place to attempt
size of the circle described by the moon round the earth is ts affects
ally increasing. But then the moon cannot get round the largebe
circle so quickly as round the smaller one; and hence her angular velocity is retarded, although her linear velocity is augmented.

The two results are therefore a lengthening of the day and of the month; in the present state of the earth and moon the rate of alteration of the day would be a good deal greater than that of the month.

Now a terrestrial observer, who did not know that the earth is not a perfect time-keeper, would suppose that the moon was being accelerated, because the earth's rotation is slackening; on the other hand, a part of this apparent acceleration would be masked by the fact that the moon is really going slower. On the whole, taking the earth as our clock, there is an apparent acceleration of the moon's motion.

This was pointed out 20 years ago by Delaunay and Adams; and they have referred to this cause an otherwise inexplicable, although very small, secular acceleration of the moon's mean motion. They indeed attributed the friction to the oceanic tides; but there seems to be no reason why it should not be, at least partly, due to bodily tides in the earth, such as we are here considering.

Then adopting the theory of bodily tides as the sole explanation of this observed secular acceleration, we may obtain a numerical relation between the height and lag of the tides.

This relation shows that either the tides lag by a very small angle, or that their height is very small. The small lag would correspond to approximate fluidity or nearly perfect elasticity of the earth's mass; and the small height may correspond to either very great viscosity, or to elasticity approaching perfect rigidity. Then as the earth is not nearly fluid, and as it probably does not yield largely as a nearly perfectly elastic body, it is probable that, whether nearly perfectly elastic or very viscous, it possesses a very large effective rigidity. This conclusion is therefore comfirmatory of that deduced from the ocean tides.

There is nothing here, however, to negative the hypothesis that in past ages the rigidity may have been not nearly so great, and the theory of viscosity still seems quite legitimate.

We will now assume that the earth is viscous, and will trace the configuration of the two bodies backwards in time. In the present state the rate of diminution of the earth's angular velocity of rotation is much greater than that of the moon's orbital angular velocity; if the former were exactly 27⅓ times as great as the latter then the day and month would (in the retrospect), in a given time be

the friction lasts. Tides
of lagging may be d
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Another result
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portion. Now this, to a very rough degree the present proportion, and accordingly in the that when the day has fallen to 15½ hours the long, so that each of the two has been reduced primitive value.

er back we go the less true is this proportionality, n being much more rapidly reduced than the day, so that when the day has fallen to 6 hours 50 minutes the month is only 1 day 14 hours. With a certain assumed viscosity of the earth, namely that aboved defined, I found that this enormous change might have taken place in 56 million years.

If the changes be traced still further back, we ultimately come to a state where the month and day are identical and are both 5½ hours long. In this condition the moon was always opposite the same face of the earth, and there were only 5000 miles intervening between the surfaces of the two bodies. The conditions of the problem are such that it is not possible to give this period and this distance with any great accuracy, and it seems fairly probable that a more accurate solution would show the two bodies still more nearly in contact.

Now the conditions of stability of rotating fluid masses are very obscure; but it seems that if in consequence of rapidity of rotation a fluid planet could no longer subsist as a single body, and were gradually to separate into two, then immediately after separation the condition of the two masses would be closely similar to that to which we have traced back the earth and moon.

Hence there seems a considerable probability that the moon and earth once formed parts of a common mass, and that they attained their present configuration through the action of the frictional tides raised in the earth by the moon and in the moon by the earth.

In this retrospect we have found the day and month converging to identity; but if we look onward in time we find them again converging to a common period of about 50 of our present days*. One rather curious consequence of this is that the number of terrestrial revolutions in a lunar period can never exceed a certain amount; and it appears that this maximum is about 29, and that we have already passed through that phase. Thus in one sense tidal friction has done more than half its work.

We have hitherto supposed the earth's axis to be perpendicular to the lunar orbit; but in the actual investigation the obliquity of the ecliptic was tracked *pari passû* with the lengths of the day and month.

If the axis of the earth be oblique to the lunar orbit, the tides are complicated by the variation in the disturbing forces with the moon's declination. The tides may then be analysed into seven different waves, which are propagated with different velocities; of these, three have a period of nearly a half-day, three of nearly a day, and one has a period equal to a semi-period of the moon or fortnight. One of the semi-diurnal, one of the diurnal, and the fortnightly tide are very small; and of the rest the "lunar semi-diurnal tide" is by far the most important.

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* See Thomson and Tait's Nat. Phil. § 276.

In an article of this nature, it would be out of place to attempt to show how the attraction of the moon on the several tides affects the earth's axis of rotation *. It must here suffice to state that the obliquity of the ecliptic will increase if the viscosity of the earth's mass is less than a certain amount, but will decrease if it be greater. Hence it is most probable that at the present time the obliquity is diminishing, but with such slowness that thousands of years must elapse before the decrease can become sensible.

On the other hand, it is probable that in the earlier stages of the earth's history, when the earth was more nearly molten, the obliquity was increasing.

In order to determine how far this theory can go towards explaining the obliquity of the ecliptic, I took the most favourable case, viz. where the vicosity is small, and traced backwards in time the obliquity, and the lengths of the day and month from their present values. Of the day and month we have already spoken; but the obliquity was found also to diminish in the retrospect, so that when the day was 6 hours and the month 12 hours, the obliquity had decreased from the present 23½° to about 14°.

It is certain, however, that this is to some extent an overstatement, because the simultaneous changes of the inclination of the lunar orbit to the ecliptic were neglected, and I have reason to believe that in the earlier times that inclination might perceptibly modify the results. The changes of obliquity were not traced backwards beyond this point, because no account was taken of these parallel changes of inclination of the lunar orbit.

The fact that the obliquity ceases to vary when the month is twice the day, arises from a general theorem, that if the planet have a small viscosity, and if the period of the satellite is twice that of the planet's rotation, the rate of change of obliquity vanishes.

On the whole I conclude that some 8° or 9° of obliquity may probably be referred to the action of the tides.

In a second paper † I have considered a secular distortion of the earth's mass which must be a result of tidal friction.

The action of the moon on the lagging tides has been already likened to a brake. Now it appears that by far the larger part of the brake power is applied in the equatorial regions; and as the earth is by hypothesis viscous, it follows that the rotation of the equatorial regions must be retarded rather more quickly than that of the polar regions. Hence there must arise a screwing motion of

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* Some details are given in the abstract of a paper on "The Precession of a viscous Spheroid," in the Proc. R. S. No. 191 (1878).

†Of which an abstract will be found in the Proc. R. S. No. 191 (1878).

the earth's mass, the polar regions shifting from west to east relatively to the equator.

Numerical calculation shows that the rate of distortion must have been so slow in latter times, that no perceptible effect can have thus arisen within geological history. It is, however, just possible that the N. and S. trend of the great continents may be connected with this cause, for the continents may be the great wrinkles left by former twisting.

In a second part of the paper the amount and distribution of the heat generated within the earth by friction is investigated. It appears that in the course of the prolongation of the day from 5½ hours to 24 hours, enough heat must have been generated within the earth to heat the whole mass 3000° Fahr., supposing it to have the specific heat of iron and the heat to be all applied at once. It would seem at first sight that we have here an explanation of the observed increase of the temperature of the earth as we go downwards; but a more careful examination of the question shows that only a very small fraction of that increase can possibly be referred to this cause.

The dynamical investigation, of which a slight sketch has now been given, yields results which obviously point to a great modification of the nebular hypothesis of Laplace and Kant. It would as yet be premature to examine the solar system to see how far this tidal theory of evolution can explain the configuration of planets and their satellites; for it remains to investigate the secular changes of the inclinations and eccentricities of the orbits of satellites due to tidal reaction. I hope in a few months' time to have completed at least a partial investigation of these points; and the results already attained appear of considerable interest.

The proof or disproof of the theory will turn on numerical results, and to obtain those results in the cases of other planets will entail much work. At present I will only remark that the anomalous satellite of Mars, which revolves faster than its primary rotates, appears to present a case strongly confirmatory of these views; for it seems probable that its extreme minuteness has preserved it as a standing memorial of the primitive time of rotation of Mars round his axis. On the other hand, the system of Uranus is very perplexing; and so little is really known about that planet and his satellites, that it will be difficult to put the theory to the test.

G. H. Darwin.

Occultation of Antares.

The rarity of occultations of bright stars at any given place is well illustrated by the case of α Scorpii, this star having been covered by the Moon, as seen from some point on the Earth's surface, at every lunation for the past four or five years; whereas only once, viz. on the evening of July 28 next, between $9^h\ 37^m$ and $10^h\ 7^m$, is the phenomenon observable from our own country. On this occasion the emersion appears to take place about 1½ hour before the Moon sets. Occultations of Antares are of course rare in these northern latitudes, owing to the star's considerable southern declination. One is recorded in the 'Historia Celestis' of Tycho Brahe; this was on the evening of August 30, 1595, and the observer appears to have been Mœstlin; we are told that on this occasion "Cor ♏ prope cornu ☾ australe occultatum. Hor. 9 noctis, paulo antequam horizontem subirent." One occurred on August 27, 1800, and another on March 21, 1805. Nothing of much interest seems to have been noticed on these occasions; but on April 13, 1819, Prof. Burg, of Vienna, had the impression that the star had a curious appearance on the Moon's disk at emersion. He states, "At $23^h\ 3^m\ 17^s$ I observed the emersion of a star 6·7 mag., which, about 5^s after, suddenly appeared to me like a star of the first magnitude; and it is from this transition that I have dated the time of emersion. Perhaps Antares is a double star, and the first observed small one is so near the principal star that both, viewed even through a good telescope, do not appear separated." But no star of the brightest class was then known to have a minute close companion. Bode, therefore, remarks on the above, "Antares is no double star;" and he enters into an explanation about the vapours of the Moon's atmosphere to account for it, that "the star first appeared through the vapours of the Moon's atmosphere, at her dark limb, along which, however rare it may be, this atmosphere must have occupied a long track in the direction of the rays that entered the observer's eye from Antares, which, when clear thereof, suddenly appeared in its full splendour:" and he goes on to remark, "Had that limb of the Moon, behind which Antares emerged, been enlightened, the Moon's atmosphere would also have been enlightened and have become visible to us, appearing a continuation of the Moon's limb, but sufficiently transparent for us to have seen the star through it; so that after observing its emersion, we should still have seen it within what we considered to be the Moon's disk."

In the year 1856 there were three occultations of Antares visible at Greenwich; but the last of the three happening in daylight was of no great interest. At the beginning of the year the Astronomer Royal called attention to these, because, "in the accounts of observed occultations of Antares by the Moon, it had been remarked that the light of the star did not disappear or reappear entirely at one instant, but at two instants." There was no need now to resort to any ingenious explanations about the vapours of the Moon's atmosphere, as the small companion star had been detected by Grant in India in 1844, and confirmed by Mitchell at Cincinnati in 1846. The occultation of March 26, 1856, was watched by several observers through haze. In the 'Monthly Notices' for April 11 of that year will be found some of their recorded impressions. Dawes says, "The small companion appeared instantaneously at its full brightness; but from its diffused condition it was not possible to form any correct judgment of its magnitude, which, however, could scarcely exceed the 7th. Its bluish-green colour was very conspicuous. When I had counted about 18 beats of my pocket-chronometer, equal to $7^{s}\cdot2$, Antares itself suddenly blazed forth, and completely enveloped its companion in its enormously diffused image. Vision was even worse than at the disappearance; and I could not afterwards obtain a glimpse of the companion, though the strong twilight favoured its visibility." Mr. Whitbread "saw a small speck emerge from behind the dark limb, which I should describe as like a star of the twelfth magnitude, and in seven seconds Antares burst forth in full splendour." Captain Shadwell at Portsmouth says, Antares at emersion on March 26 appeared to "hang on the Moon's limb about two seconds." But on June 13 he says the disappearance was instantaneous. On this occasion there was the disadvantage of the Moon being at the full. It was considered about this time that accurate observations of the companion star could hardly be got in this country; but on July 20 and 30, 1856, Mr. Hippisley at Ston Easton, Bath, seems to have observed it fairly. A rough diagram of the position of the larger and smaller stars will be found in the 'Monthly Notices' for November of that year. Since 1856 no occultation of Antares has taken place that has been visible from this country. At the two earlier occultations of that year, the Moon's altitude was about 13°; at the coming one, on July 28 of the present year, her elevation above the horizon is less than that; consequently there will be more difficulty in detecting the small blue companion, the real point of interest in occultations of α Scorpii. S. J. Johnson.

Upton Helions Rectory, Crediton,
1879, May 17.

[The *comes* is of 7 mag., pos.-angle 276°, dist. $3''\cdot7$, and will reappear 6^{s} or 7^{s} before Antares (*cf.* 'Observatory,' No. 26, p. 63). Ed.]

Double Stars for July.

The most important pairs between 13^{h} and 16^{h} are as follows:—

Σ 1728=42 Comæ Ber. R.A. $13^{h}\ 4^{m}\cdot1$, Dec. +18° 10′.

Mag. 6, 6. Colours, yellow.

Binary. Common proper motion $-0''\cdot433$ in R.A. and $-0''\cdot18$ in N.P.D. (OΣ). Period about 25 years.

1876·41	195°·2	0″·42	Washington Obs.

Σ 1777. R.A. $13^{h}\ 37^{m}$, Dec. +4° 9′.

Mag. 5·8, 8·2. Colours, A yellow, B blue.

Binary. Common proper motion $-0^{s}{\cdot}023$ in R.A. and $+0''{\cdot}05$ in N.P.D.

1877·45	229°·4	3″·58	Flammarion.
78·40	235 ·0	·08	Doberck.

Σ 1877 = ε Boötis. R.A. $14^{h}\ 39^{m}{\cdot}7$, Dec. +27° 35′.

Mag. 3, 6·3. Colours, A very yellow, B very blue.
Binary.

1877·23	324°·7	2″·96	Doberck.
78·45	328 ·4	·94	„

Σ 1888 = ξ Boötis. R.A. $14^{h}\ 45^{m}{\cdot}8$, Dec. +19° 36′.

Mag. 4·7, 6·6. Colours, A yellow, B purplish red.
Binary. Doberck's elements (1877) are:—☊ = 26° 22′, λ 117° 46′, γ 36° 55′, e = 0·7081, P = 127·35 years, T = 1770·69, a = 4″·86.

1877·46	283°·0	4″·46	Wilson.
78·45	281 ·2	·13	Doberck.

Σ 1909 = 44 Boötis. R.A. $14^{h}\ 59^{m}{\cdot}8$, Dec. +48° 8′.

Mag. 5·2, 6·1. Colours, A yellowish, B bluish.
Binary. Doberck's elements are:—☊ 65° 29′, λ 1° 18′, γ 70° 5′, e 0·710, P 261·12 years, T 1783·01, a 3″·093.

1877·46	241°·8	4″·71	Flammarion.
78·51	240 ·2	·82	Doberck.

Σ 1932. R.A. $15^{h}\ 13^{m}{\cdot}2$, Dec. +27° 16′.

Mag. 5·6, 6·1. Colours, very white.
Binary.

1877·47	301°·5	1″·07	Wilson.

Σ 1937 = η Cor. Bor. R.A. $15^{h}\ 18^{m}{\cdot}2$, Dec. +30° 43′.

Mag. 5·2, 5·7. Colours, yellow.
Binary. Period about 41 years.

1877·48	81°·2	0″·78	Dembowski.
78·45	93 ·3	·61	Doberck.

Σ 1938 = μ^2 Boötis. R.A. $15^{h}\ 19^{m}{\cdot}9$, Dec. +37° 47′.

Mag. 6·7, 7·3. Colours, greenish white.
Binary. Doberck's latest elements are:—☊ = 173° 42′, T 1863·51, λ 29° 0′, γ 39° 57′, e 0·5974, a 1″·47, P 280·29 years.

1878·26	129°·4	0″·70	Doberck.
·47	137 ·7	·63	„

Σ 1954 = δ Serpentis. R.A. $15^{h}\ 29^{m}{\cdot}1$, Dec. +10° 57′.

Mag. 3, 4. Colours, A yellowish white, B ashy.

Binary.

1876·40	190°·0	3″·49	Hall.
78·48	189·1	·87	Goldney.

Σ 1967 = γ Cor. Bor. R.A. $15^h\ 37^m{\cdot}7$, Dec. $+26°\ 40'$.

Mag. 4, 7. Colours, A greenish white, B purple.

Binary. Doberck in 1877 obtained the following elements:—☊ 110° 24′, γ 85° 12′, λ 233° 30′, e 0·350, P 95·50 years, T 1843·70, a 0″·70.

1876·32	single	....	Flammarion.

Σ 1998 = ξ Scorpii. R.A. $15^h\ 57^m{\cdot}8$, Dec. $-11°\ 2'$.

Mag. 4·9, 5·2. Colours, yellowish white.

AB	1877·46	184°·3	1″·26	O. Stone.
$\frac{AB}{2}$ and C	1877·63	67·6	7·22	Flammarion.

Doberck's latest elements are:—☊ 12° 15′, γ 68° 42′, λ 89° 16′, e 0·0768, T 1859·62, a 1″·26, P 95·90 years.

JOSEPH GLEDHILL.

Mr. Edward Crossley's Observatory,
Bermerside, Halifax.

Meteor Notes for July.

EVENING observations may be made between the 12th and 27th, and in the mornings from the 15th to the 29th. This month is a very favourable one for meteor observations, and a large number of different showers are visible. Several of these as observed by the writer last year will be found in our "Notes" for September 1878 ('Observatory'*, Vol. ii. pp. 163–5). The two most important of these are at 341° − 13° and 32° + 53°, both occurring with some intensity at the end of the month, when shooting-stars are unusually numerous, the visible horary number for one observer being more than 20 during the period from the 25th to the 31st, 1878. The Andromedes I. are also a conspicuous shower in July, with a centre at 7° + 33°, which is Mr. Greg's average of 11 observations; but the writer found it a little further north, viz. at 6½° + 36½°, from 63 meteors observed in July–Aug. 1877–8, and there is a contemporary radiant close to α Andromedæ (3° + 27°). The Cygnids I. form another rich display in this month from 309° + 48° (Greg's No. 81). 20 of them were noted by the writer in 1877, with a centre at 315° + 50°; and Zezioli had seen it in 1867, July 10, for he recorded, "$12\frac{3}{4}^h$–$13\frac{3}{4}^h$, adjacent to the point 300° + 57°, 17 falling stars radiated in various directions;" and in 1868, July 19, 11^h–12^h he saw 20 small meteors from the region around the point 297° +57°. On July 31–August 1, 1878, a shower of 9 very swift streak-leaving meteors was observed at Bristol from an accurately defined point at 6° +11°; and just before daylight on the 29th, two fine meteors with long paths (31° and 41°) were seen evidently proceeding from a new radiant at 33° −20°. Some bright meteors were also recorded from a new shower at 234° +48° on the 21st, and there is another recently ascertained system near γ Andromedæ with a very exact position at 23° +41° (22 meteors, July 25–31). A shower at 317° −11° was observed in marked activity on July 20, 1877; and Major Tupman saw it at 320° −4° on July 21, 1870. Other streams hitherto but slightly observed at 354° +42°, 47° +25°, 134° +78°, and 96° +72°, require further watching in July and early in August; and among the more prominent showers already well determined may be mentioned those at 332° +50°, 293° +42°, 257° +36°, &c.; but the number of showers originating from Pegasus, Cygnus, Lacerta, Cassiopeia, Draco, and Aquila is very considerable at this epoch, and many additional observations are necessary to disassociate them and fix their accurate positions. There are very few, if any, of the true Perseids visible at the end of July; for of 349 shooting-stars seen by the writer during July 25–31, 1878, there was little certain indication of this shower. The 27th–31st appears to form a special meteoric epoch, remarkable alike for the number and brilliancy of meteors. The major showers of Aquariads (341° −13°) and Perseids II. (32° +53°) have been already referred to, and they should be reobserved at each annual return. A remarkable display of one or both of these streams may have occurred in 1848, when Heis recorded, on July 27–28, a "strikingly great number of shooting-stars, most of them with trains."

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* See also 'Observatory,' Vol. ii. p. 103, for a reference to July meteor-showers.

Large meteors should be looked for on the 11th, 20th–21st, and 25th–30th.

Recent Observations.—On May 25, 9^h 54^m, a brilliant meteor, estimated at 3 × ♀, was observed by Mr. J. H. Simmons at London. It appeared about 2° above and to the right of α Lyræ, passing exactly over β Lyræ, and the disappearance occurred 5° left of α Aquilæ. It had a fine train. Colour yellow. Duration 1 second.

A meteor-shower was traced by Mr. Sawyer at 206° −8°, April 12–26, including a 1st-mag. stationary meteor exactly at that point. This radiant agrees very closely with a position at 207° −8°, April–May, derived by the writer both from observation and reduction. It also accords with a radiant seen by Mr. Corder at 208° −6°; and the fireball of May 12, 1878, probably belonged to the same stream, for it had a centre at 214° −7°.

Mr. Sawyer has just published * a list of 32 radiant-points determined by him during the last two years. They are derived from the recorded paths of nearly 600 shooting-stars which he has observed at Cambridge, Mass. An important feature of this new list is that the peculiarities of speed, brightness, and length of path of the meteors of each shower are specified. Mr. Sawyer's radiants consist for the most part of positions which agree with previously observed showers, and he points out these accordances; but there are several which he regards as probably new, among them the following:—

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* American Journal of Science and Arts, Vol. xvii. June 1879.

Sept. 30–Oct. 2 ..	23°	+17°	7 bright, short, and rapid meteors.
Sept. 27–Oct. 30 ..	3	+21	7 „ „ „ „ „
Dec. 1–9	97	+16	6 faint meteors with rapid motion.
Dec. 31–Jan. 7 ..	97½	+17	8 „ „ „ „ „

The last two showers appear to be identical. Mr. Sawyer's position for the Perseids is at 43° +56½°, Taurids I. 60° +18° Taurids II. 81° +21½°, and Geminids 103° +34°. The latter is deduced from 10 meteors seen Dec. 22–29, and proves that this shower extends to the end of that month. One of the best displays noted in the list was that recorded on the early evening of Oct. 28, 1877, when, in a watch of ¾ of an hour, 8 meteors were traced from an exact radiant at 356° +40°, evidently confirming S. and Z. (170) 352° +41°, November 13, 1868. Mr. Sawyer also observed, on July 28, 1878, four bright meteors, with very slow, long paths, diverging from a point at 337° −33° near the southern star Fomalhaut. On the same night at Bristol a meteor (having a path of 33°) was registered from the same shower. Schmidt gives this radiant at 335° −27°, July 18–20; and Prof. Herschel, on July 28, 1865, saw several meteors, indicating a centre at 338° −28°, which, however, he has since pointed out, may have been Aquariads belonging to the special shower at 341° −13°. There are several well-defined showers appearing in this region at the end of July, of which the chief are at δ Aquarii and Fomalhaut.

W. F. Denning.

CORRESPONDENCE.

To the Editor of 'The Observatory.'

Tempel's First Periodical Comet.

Sir,—

I sent, on April 26, a telegram to the Imperial Academy of Sciences in Vienna, with the announcement that I had succeeded in discovering the periodical Comet II. 1867, on April 24; but it seems that this telegram has been stopped somewhere, for neither does the 'Astronomische Nachrichten' mention it, nor has it been received in Leipzig or Berlin.

I beg, therefore, to communicate to you the few observations which I have been able to obtain hitherto :—

	h	m	s			h	m	s		°	′	″
April 24,	14	30±		Arcetri M.T.	R.A.	16	50	58·79	Dec.	−13	32	0·8
May 1,	15	5	2			16	52	16·15		−14	20	42·2
May 13,	11	30	26			16	50	35·77		−15	58	6·8

This comet is extremely faint and small, and might well serve as a test for good telescopes, although I know of still fainter and smaller nebulæ.

It moves pretty exactly in accordance with Hypothesis I. of the Ephemeris given by M. Raoul Gautier, May 6·9416 (Corr. -16^{s}, $+1'\cdot6$). Since it will approach Jupiter nearer in the year 1882 than in the year 1870, we shall probably have difficulty in seeing it again, if we ever do so.

I am, yours very truly,
WILHELM TEMPEL.

Osservatorio di Firenze,
Arcetri, 1879, May 16.

[M. Raoul Gautier has obtained the following corrected elements from M. Tempel's 3 observations :—

Per. Pass. May 7·02 Berlin M.T.

			°	′	″	
μ	593″·18	☊	78	45	37·4	1879·0
ϕ	27° 35′ 0″·6	ϖ	159	25	52·7	
Log a	0·517880	i	9	46	31·6	

The comet is receding both from the Sun and the Earth, and is at the same time moving south, so that it will be very difficult to observe. Such observations, however, as may be obtained will be all the more valuable on this account. The following are the positions (during the absence of moonlight) from M. R. Gautier's ephemeris :—

		h	m	s		°	′
June 10	R.A.	16	35	16	Dec.	−20	44·7
14		16	33	7		−21	26·7
18		16	31	17		−22	8·0
22		16	29	51		−22	48·1

ED.]

SIR,—

On May 25, at 11.55, this comet was readily found by aid of the ephemeris published in 'Nature.' In an eighteen-inch reflector it appeared rather faint, with an apparent nucleus that seemed considerably out of centre towards pos.-angle 40°. This might have been a star seen through the comet, as clouds prevented further observation. It was again seen on June 10th, when it appeared fainter than before, round and diffused, about 30″ in diameter; at 12.2 it was exactly south of and 60″ distant from a 12-mag. star. There was just time to note that this star was 77^{s} preceding an 8½ and 10 mag. pair (pos. about 300°), and 259″ south of the brightest star. A night or two after, I looked in case it might be a nebula, but the comet had moved.

I am, Sir, yours faithfully,
A. A. COMMON.

Ealing, W.

Variable and Double Star.

Sir,—

I desire to call the attention of observers of variable stars to a naked-eye star near α Virginis, which I have recently found to be a close pair. The only information concerning variability I have found is in Webb's 'Celestial Objects,' where it is noted, "upwards of 5 m. to 8 m.?; discovered by Schmidt 1866: about 2° south of α, 10^m *f*." Heis gives this as a naked-eye star, 6–7 m. It is B.A.C. 4531 (= Virginis 550). It is now very easily seen, and brighter than most of the 6–7 m. stars of Heis. On Proctor's Atlas it is called Z Virginis. As a double star it is only moderately difficult. From two measures I find the angle and distance as follows:—

$P = 80^\circ{\cdot}4 \qquad D = 0''{\cdot}48 \qquad$ Mags. 6·2, 6·5 $\qquad$ 1879·4.

There is a distant companion, 12·5 m.:—

$P = 156^\circ{\cdot}5 \qquad D = 23''{\cdot}88.$

I do not recall any similar instance of a star, variable to any considerable extent, being found to be a close double. It will be easy to determine which, if only one, of the stars is variable.

Its place for 1880 is:

R.A. $13^h\ 28^m\ 28^s$, Decl. $-12^\circ\ 36'$.

Dearborn Observatory,
1879, May 18.

I am, Sir, yours faithfully,
S. W. Burnham.

What is the Chromosphere?

Sir,—

In a note at p. 390 of the March number of the Observatory, you refer to a letter of mine published in the 'Athenæum' as containing "the extraordinary statement" that "the rings seen by Mr. Lockyer in 1871 corresponded to the area of the chromosphere and not of the corona;" and you add, "with reference to this assertion, it is sufficient to remark that the chromosphere is only 10″ high, and that at the time of Mr. Lockyer's observation it was completely hidden by the Moon, being some 51″ below the Moon's limb on the one side and 26″ on the other."

It seems clear that your definition of the word "chromosphere" must exclude the prominences, many of which, it is hardly necessary to remark, reach a height of far more than 10″ above the Sun's limb. Several prominences are shown upon the photographs taken during the eclipse of 1871, which reach a height of nearly 2′. I know of no authoritative definition of the word "*chromosphere*;" but, as ordinarily used, I should define it as the region of gaseous incandescence about the Sun, which is observable with the spectroscope under ordinary daylight conditions. Strictly speaking, no doubt, the "*chromosphere*" should include the whole region, extending far up into the corona, within which bright lines can be recognized; but this certainly does not correspond with the colloquial use of the word. In the eclipse volume I have found it convenient to speak of the "lower chromosphere" and "upper chromosphere," defining the lower chromosphere as confined to the region observable under ordinary daylight conditions, and have subdivided the "upper chromosphere" into the "1474 region," the "hydrogen region," the "D_3 region," &c., each corresponding to the areas of the lower corona or upper chromosphere in which the corresponding bright lines are observable.

As Mr. Lockyer does not give the time of his observation, I am at a loss to understand how you determine with such accuracy the number of seconds that the Sun's limb was below the Moon's limb. The original description of Mr. Lockyer's observation, published in 'Nature,' vol. v. p. 218, runs thus:—"What I saw was four exquisite rings, with projections where the prominences were. In brightness C came first, then F, then G, and last of all 1474! Further, the rings were nearly all the same thickness, certainly not more than 2′ high."

If there were projections where the prominences were, it is clear that the rings were not as high as the prominences; and I think that I am justified in saying that the rings which Mr. Lockyer observed corresponded to the chromosphere, and to what, in the eclipse volume, I have referred to as the "lower chromosphere."

I am, Sir, yours faithfully,
A. Cowper Ranyard.

[Notwithstanding Mr. Ranyard's explanation, we fail to see how he is justified in saying that the rings which Mr. Lockyer saw corresponded to something which was actually hidden by the Moon's disk at the time, whether it be called "lower chromosphere" or simply "chromosphere." Any one who uses a spectroscope or who has observed a total eclipse knows that the chromosphere, as ordinarily understood, is a perfectly definite ring round the Sun, and that the prominences are *occasional* outbursts extending into the upper envelope, commonly called the corona. The word "prominences" in itself sufficiently indicates that they do not form any thing resembling a ring round the Sun, such as Mr. Lockyer describes.

The question whether it will be found convenient to disturb the accepted phraseology as Mr. Ranyard proposes, may well be left till the "Eclipse Volume" appears. Meanwhile the popular meaning of the word "chromosphere" is acquiring a prescriptive right from the circumstance that it has been adopted for so many years by all observers of the Sun.

The time at which Mr. Lockyer made his observation is indicated thus by him at p. 374 of his 'Solar Physics' (a work which Mr. Ranyard might have consulted with advantage before disputing the facts):—"Corona at 80″ from commencement." The duration of totality was 120 seconds; and throughout this interval, with the exception of a few seconds at the beginning and end, the whole of the chromosphere was hidden by the Moon, so that the exact instant is of no real importance.—Ed.]

The Sadler-Smyth Scandal.

Sir,—

Mr. Heaven commences a smart attack on me (or what is intended to be such) by an announcement that "he does not thoroughly understand the merits of the case." This is so palpably true, that I consider it releases me from all obligation to devote my time and your space to his letter. I will therefore only ask him or the Society at large to compare the names elected on the Council last February with any recent list of the 500 Fellows of the Society, and my meaning will be quite clear. There were names on that Council which had no claim to be there, contrasted with dozens and dozens of other names which had a far higher claim to be there. I decline to mention names in print, because I have no desire to embark in a war of personalities.

Mr. Heaven must be a very simple-minded man if he solemnly believes that "juggling" is unknown at the R. A. S., and that every thing there is "above board."

Mr. Heaven's letter seems to imply that I have cast reflections on him as a scrutineer, have accused him of bribing the voters, of corruptly miscounting the votes, and so on. I have done nothing of the kind.

I am, Sir, your obedient servant,
G. F. Chambers.

Northfield Observatory, Eastbourne,
June 17, 1879.

NOTES.

Obituary Notices of Astronomers*.—Mr. Dunkin has collected, in a very interesting volume, twenty-four biographical sketches of eminent Fellows and Associates of the Royal Astronomical Society, all of whom, with the exception of Sir W. Herschel, have died in the last eight years. Their memory is thus fresh amongst us, and we feel a personal interest in their career, which makes Mr. Dunkin's pleasant little book peculiarly welcome. Most of the memoirs were prepared by Mr. Dunkin for the R. A. S. Annual Council Reports, during his six years' tenure of the office of honorary secretary; and those who remember how complete a record of the progress of astronomy was presented year after year in these

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* Obituary Notices of Astronomers. By Edwin Dunkin, F.R.S., F.R.A.S. (Williams and Norgate, 1879.)

Reports under his able régime, will probably be glad to have in a collected form the biographical portion of his interesting contributions. The notices of Heis, Le Verrier, Littrow, and Santini first appeared in the 'Observatory' and have been revised for the present work, and one or two have been specially written for it, to complete the list. Many interesting mementoes of great men are here collected, and the author has fully availed himself of the peculiar facilities for acquiring information which his personal acquaintance with so many of them gave him. His notices of the great French astronomers, Le Verrier, Delaunay, Laugier, and Mathieu, have a special value on this account; and the account he gives of the Paris Observatory has the merit of being based on an intimate acquaintance with its personnel. To the astronomers we have just named, and to the two Herschels and Main, Mr. Dunkin devotes the lion's share of his space, the other notices consisting, as a rule, of a few pages each. A little surprise may perhaps be felt at finding the names of Murchison and Sedgwick enrolled amongst astronomers; but as Fellows of the R. A. S. they are entitled to some notice at Mr. Dunkin's hands. Miss Sheepshanks, too, though not strictly an astronomer, has done so much to promote the science by her munificent donation to the University of Cambridge, that we are glad to see her name in the list. Thanks to the personal traits of eminent astronomers which he has collected, Mr. Dunkin has produced a readable as well as an instructive book, and we heartily commend it to our readers.

ROYAL OBSERVATORY, GREENWICH.—The Astronomer Royal's Report for the period 1878 May 2 to 1879 May 20 was presented at the Annual Visitation on June 7. Under the head of Astronomical Instruments we notice that to make the instrument easy for use with the long half-prism spectroscope the telescope-tube of the great equatoreal has been shifted longitudinally 30 inches in its cradle, bringing the eye-end nearer to the centre and allowing more room between it and the floor. A new and larger first prism has been made for the half-prism spectroscope, a third prism has been added, and some other important improvements have been made by Mr. Hilger, and the instrument is now used for all observations except in cases where small dispersion is desired. The horizontal flexure of the transit-circle telescope has changed from $-0''{\cdot}13$ to $+0''{\cdot}16$. With the transit-circle, 4284 transits and 3704 zenith-distances have been observed, and with the altazimuth 733 azimuths of Moon and Stars and 368 zenith-distances of the Moon. "After a fine autumn, the weather in the past spring has been remarkably bad. More than an entire lunation was lost with the transit-circle, no observations of the Moon on the meridian having been possible between January 8 and March 1, a period of more than seven weeks. Neither Sun nor Stars were visible for eleven days, during which period the clock-times were carried on entirely by the preceding rate of the clock. The accumulated error at the end of this time did not exceed 0^s·3."

The reductions are in their usual advanced state, the current transit and circle observations being completely reduced to May 15 and May 1 respectively, and the altazimuth observations to April 21 (the end of the preceding lunation). The whole of the planetary results for last year exhibiting geocentric and heliocentric errors are complete. Before issuing the Nine-year Catalogue, some discussions were made of its N.P.D.'s as compared with the two last preceding Greenwich Catalogues, the Cape Catalogue (1860), and the Melbourne Catalogue (1870); and from these, as well as from the results of the ecliptic investigations, it appears that Stone's refractions used since 1868 do not give satisfactory results, whilst Bessel's refractions of the 'Tabulæ Regiomontanæ' harmonize all the observations. The latter have therefore been adopted for the future, and the results for 1877 and 1878 have been corrected as far as practicable.

Two determinations of colatitude with the two constants of refraction respectively have been made for these two years, the difference depending on the change of refraction being 0''·3. From the ecliptic investigation it appears that the mean of the observed N.P.D.'s of the Sun throughout the year, or the distance from the pole to the equator, is too small by +0''·31 for 1878 (using Bessel's refractions), whilst in 1877, with Stone's refractions, it was too small by 1''·13. In connexion with the question of refraction, thermometers mounted to determine the curves of equal temperature in the transit-circle room seem to show that these curves are circular.

The spectroscopic observations have been almost entirely suspended, in order that the reduction of accumulated photographic observations might proceed more rapidly. These latter have now been completely reduced, exhibiting areas and heliographic longitudes and latitudes of spots and faculæ from 1874 up to the present time. With the half-prism spectroscope the motions in the line of sight have been determined for 7 stars (3 of them not previously examined), and for two of these a dispersive power equivalent to 15 prisms of 60° was employed, though the stars are only of the second magnitude. The spectrum of Brorsen's comet and of the eclipsed Moon and the solar spectrum in the neighbourhood of G were also examined, the last named with reference to the existence of bright lines in this region. Photographs of the Sun have been taken on 150 days, and on 121 of these days there is a complete absence of spots. On comparing them with those of the preceding year, when there was an absence of spots on 66 days out of 156, it appears that we have not yet passed the minimum.

"The Magnetic Reductions for the years 1865 to 1876 are nearly completed. The results are exhibited in the form of annual and monthly mean curves of diurnal inequality, as in preceding investigations; the abscissæ showing the variation of magnetic declination, and the ordinates that of horizontal force, throughout the 24 hours. The annual curves of diurnal inequality are now complete for the period of 36 years, from 1841 to 1876; and from the great length of this series of observations, all made on the same system and with similar instruments, most important inferences may be drawn, both as to the laws of diurnal inequality in general and its changes in different years and seasons, and as to the connexion between magnetic phenomena and sun-spots. These annual curves show a well-marked change in close correspondence with the number of sun-spots. About the epoch of maximum of sun-spots they are large and nearly circular, having the same character as the curves for the summer months; whilst about the time of sun-spot minimum they are small and lemniscate-shaped, with a striking resemblance to the curves for the winter months. It may be worthy the consideration of the Board whether the whole of these results, with any modifications that experience suggests, should not be printed and circulated as a separate volume.

"The monthly curves, 1865–1876, have been formed for three periods of four years each, corresponding roughly to periods of minimum, maximum, and mean of sun-spots, and the whole series stands thus (the general character of the curves being added):—1841–1847, (curves) mean; 1848–1857, small; 1858–1863, large in summer, small in winter; 1865–1868, small; 1869–1872, very large; 1873–1876, mean. The maxima of sun-spots occurred in 1848, 1860, and 1870, and the minima in 1844, 1856, and probably in 1879.

"The connexion between changes of terrestrial magnetism and sun-spots is shown in a still more striking manner by a comparison which Mr. Ellis has made between the monthly means of the diurnal range of declination and horizontal force, and Dr. R. Wolf's 'relative numbers' for frequency of sun-spots. It appears from this that not only is there a general correspondence in the two sets of phenomena, but that the minor irregularities of the sun-spot curve are reproduced in the curves of diurnal magnetic range; and further that the well-marked annual inequality in the latter is itself variable, being greatest at the time of maximum of sun-spots and least at that of minimum.

"It will be an interesting inquiry, when sun-spots become more numerous, to determine whether the present paucity of earth-currents is connected with the fewness of spots."

In the competition trial of chronometers, a falling off is shown in the first six chronometers of last year as compared with any of the preceding 16 years. The Greenwich and Deal time-balls have been dropped with very few failures, and the report on the Westminster clock shows its error to have exceeded 1ˢ on only 77 days out of 383.

Under the head of Extraneous work, the Astronomer Royal reports that Major Tupman, who has voluntarily devoted his whole time to the Transit of Venus reductions, has now prepared the copy for press of the observations and reductions for the three stations in the Hawaiian Islands, and nearly finished those for Egypt (Mokattam), Rodriguez, and New Zealand. The observations at Kerguelen have been finally examined, but the copy for press is not prepared.

"We are naturally looking to the Transit of 1882; and the general impression appears to be that it will be best to confine our observations to simple telescopic observations or micrometer observations at Ingress and Egress, if possible, at places whose longitudes are known. For the first phenomenon (accelerated ingress) the choice of stations is not good; but for the other phenomena (retarded ingress, accelerated egress, retarded egress), there appears to be no difficulty. The adoption of a south-polar station seems to be practically abandoned."

In regard to the Numerical Lunar Theory, the Astronomer Royal has now discussed the theory completely, and in going into details of secular changes he is at this time engaged on that which is the foundation of all, namely, the change of excentricity of the solar orbit, and its results in producing lunar acceleration.

As to the practicability of reducing the magnitude of the volume of Greenwich observations, the opinion is that no curtailment ought to be made. A strong appeal has been made for extension of spectroscopic observations of stars in reference to their motion in the line of sight, to which the Astronomer Royal has given a tacit response by the modification of the Great Equatoreal so as to facilitate that extension. He adds, "The tendency of external scientific movement is to give great attention to the phenomena of the solar disk (in which this Observatory ought undoubtedly to bear its part). And I personally am most unwilling to recede from the existing course of magnetical and meteorological observations."

"The general tendency of these considerations is, to increase the annual expenses of the Observatory. And so it has been, almost continuously, for the last 42 years. The annual ordinary expenses are now between 2½ and 3 times as great as in my first years at the Royal Observatory. I would fain flatter myself that the value of its results has increased in a greater degree."

SUN-SPOTS AND PROMINENCES*.—Herr Schmidt's observations of the Sun at Athens during 1878 show that out of 360 days of observation there were only 62 on which spots were visible. On 5 days only during the year were the observations prevented by cloud; and Herr Schmidt mentions it as a remarkable thing at Athens that the Sun was not seen for 3 consecutive days. The photographs taken at Greenwich during the twelve months ending 1879, May 1, show that out of 142 days of observation, there were only 27 on which spots were seen, whilst in the preceding twelve months, ending 1878, April 30, spots were noted on 98 days out of 163. There has thus been a marked decrease in Sun-spots up to the present time, and there is no sign that we have yet reached the minimum.

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* Astron. Nachr. No. 2241; Comptes Rendus, Vol. lxxxvii. Nos. 6 and 26; Memorie Spettrosc. Ital. 1878 Nov., Dec., 1879 March.

Prof. Tacchini's observations point to the same conclusion. The frequency of spots has fallen from 1·14 in 1877 to 0·31 in 1878, and of groups of spots and pores from 0·97 to 0·33, whilst that of days without spots has risen from 0·49 to 0·76, the change being distinctly progressive throughout 1878. The frequency of spots in the last quarter of 1878 was only 0·03. In the first quarter of 1879 it was 0·10, and of days without spots 0·92.

The faculæ and the prominences also have been very few and faint, the average for the latter being between 2 and 3 a day. Prof. Tacchini points out that the faculæ occur most frequently within 30° of the Sun's equator, and also near the north pole, whilst the prominences are for the most part confined to the zones from 30° to 50° of heliographic latitude north and south. Thus where the prominences are most frequent, the faculæ are at their minimum. The distribution of prominences is very different in 1878 from what it was near the time of sun-spot maximum in 1871. Prominences were then pretty uniformly distributed over a large zone, extending from 50° N. to 50° S., with two minima between 50° and 70°, and two maxima near the poles. Now we have two maxima between 30° and 40°, and two very decided minima between 60° and the pole, in each hemisphere.

Prof. Tacchini has further compared the frequency of prominences in July and August, at the two epochs 1871 and 1878. In 1871 the average daily number was 14·0, whilst in 1878 it was 2·7, so that the number is now reduced to less than one fifth of what it was soon after the sun-spot maximum. At the same time the outbursts of metallic vapours have become extremely rare, and the *b* lines of magnesium are but seldom seen reversed in the chromosphere. On the other hand the 1474 K line, supposed by Mr. Lockyer to indicate a low temperature form of hydrogen, is commonly present.

THE COMPENSATION OF CLOCKS, WATCHES, AND CHRONOMETERS.—Some of our readers may be glad to have their attention directed to a paper on the above subject by Mr. Edward Rigg (Assayer in the Royal Mint) which appears in a recent number of the Journal of the Society of Arts, and which has also been circulated separately. The object of the paper is apparently not to bring forward any thing new, but to collect together much information, especially in regard to modern devices, to be found only by consulting a variety of works. Mr. Rigg confines his attention principally to the subject of the compensation for variations of heat and atmospheric pressure, although, incidentally, something is said on other points. It would be difficult to give here, in the short space at our disposal, any detailed abstract of the contents of the paper: it must suffice to say that, in regard to clock pendulums, Mr. Rigg describes a number of contrivances that have from time to time been proposed, having for object the production of as perfect a method of compensation as possible, and discusses their various advantages and disadvantages; he then speaks of the influence of variations of atmospheric pressure, gives a brief historical sketch of the subject, and remarks that only two plans for neutralizing the effect appear to be in actual use—those proposed respectively by the Astronomer Royal and by M. Redier.

He then proceeds to speak of the compensation of watches and chronometers. This is a very wide subject. He refers briefly to the attempts at first made to attain a sufficient compensation by action on the balance-spring; but this method has long been quite given up, the simple compensation balance, as originally made by Earnshaw and Arnold, giving much more satisfactory results. Very many contrivances for the correction of what is commonly known as the secondary error are referred to or described. In regard to these auxiliaries Mr. Rigg remarks that it is worthy of notice, and is illustrated in a marked manner by the Greenwich chronometer trials, that no one form of balance has succeeded in establishing itself as invariably better than others, whilst balances known to be among the best will figure low in the Greenwich list.

He thinks that circumstances which are beyond the control of any workman influence the rate, and that when the performance of a chronometer is exceptionally good, the path of the compensating weights happens to accord with the several influences by which the movement of the balance is modified. The composition of the brass, the fineness of steel, degree of hammering, smoothness of acting surfaces, play of pivots, constrained molecular state of metals employed, and, above all, the nature of the oil—all these may be satisfactory to a certain limit, beyond which each may have an infinitesimal effect which cannot be isolated or neutralized. Mr. Rigg considers that the general uniformity observed in the errors of chronometers with balances of the ordinary construction tends to prove that the influences above mentioned are not of primary importance, although standing in the way of absolute perfection; and he asks whether a recognized though comparatively large error that can be determined and allowed for, is not to be preferred to a so-called perfect compensation that may never be attained, except by accident, whilst being also more liable to derangement. He remarks also that much skill now spent in the designing of auxiliaries might be more profitably expended in devising more expeditious means of adjusting the primary compensation, and refers to the attempt of the Astronomer Royal to solve the problem as being the best method hitherto invented for the purpose. Mr. Rigg lays much stress on the necessity of solidity of construction, which is so sadly wanting in a vast number of the improvements that have been proposed, and is of opinion that, until a balance is invented that will satisfy both theory and practice, it will be preferable to resort to the method of tabulating the errors of any balance of simple construction that can be depended upon to behave in the same manner when exposed to similar conditions.

The whole paper may be commended to those who are interested in the matters on which it treats. The copy which came into our hands had one great defect. Descriptions are given referring to diagrams; but no copies of the diagrams by which the paper was so profusely illustrated when read before the Society of Arts are given with the printed account. W. E.

NEW COMET.—A telegram from Washington announces the discovery of a comet by Mr. L. Swift, on June 15, at Rochester, N.Y. The following observations have been made:—

	G.M.T.	R.A.	Dec.	Obs.
	h m	h m s	° ′	
June 21	11 8	2 47 31	+64 29·1	Winnecke.
25	11 58	2 49 37	68 41·6	Tupman.
26	20 8±	2 50 18	70 7	Washington.

The comet is about 3′ in diameter, with bright central nucleus of about the 9th magnitude.

A NEW minor planet of the eleventh magnitude was discovered by M. Borrelly at Marseilles on June 13.

PROF. YOUNG suggests that the discrepancy as to the relative brightness of the blue and yellow bands in the spectrum of Brorsen's comet, as estimated by himself and by the Greenwich observers, may have been caused by the absorption of the dense yellow glass used in the Greenwich spectroscope. Prof. Young used very white glass inclining to greenish blue, and he estimated the blue band as brighter than the yellow, whereas with the Greenwich spectroscope it appeared much fainter.

MR. ELLERY's latest measures of α Centauri are:—

1879·252, Pos.-angle 174°·4, Dist. 3″·41. No. of obs. 13.

PLANET 196 (*correction to 'Observatory,'* No. 26, p. 93).—Major Tupman writes:—"The object I observed on May 25 proved to be a small star. My last observation of the planet was on May 21, $10^{h}\ 52^{m}$ G.M.T., in appt. R.A. $12^{h}\ 15^{m}\ 56^{s}$, appt. Dec. +6° 34′ 32″. Daily motion 2′ 50″ south. On May 29 I discovered my mistake, and searched unsuccessfully for the planet, which was no doubt overpowered by the moonlight." EDITOR.

Astronomical Memoranda, 1879, *August.*

Sun. Aug. 1, sets $7^{h}\ 47^{m}$, rises $16^{h}\ 26^{m}$; Aug. 31, sets $6^{h}\ 48^{m}$, rises $17^{h}\ 14^{m}$.

Equation of time:—Sun *after* clock, Aug. 1, $6^{m}\ 7^{s}$; Aug. 11, $5^{m}\ 2^{s}$; Aug. 21, $3^{m}\ 3^{s}$; Aug. 31, $0^{m}\ 16^{s}$.

Sidereal Time at Mean Noon:—Aug. 1, $8^{h}\ 38^{m}{\cdot}9$; Aug. 11, $9^{h}\ 18^{m}{\cdot}3$; Aug. 21, $9^{h}\ 57^{m}{\cdot}7$; Aug. 31, $10^{h}\ 37^{m}{\cdot}2$.

Moon. Full, Aug. 1, $19^{h}\ 12^{m}$; Last Quarter, Aug. 9, $14^{h}\ 9^{m}$; New, Aug. 17, $8^{h}\ 11^{m}$; First Quarter, Aug. 24, $3^{h}\ 12^{m}$; Full, Aug. 31, $6^{h}\ 58^{m}$.

Occultations:—Aug. 1, $13^{h}\ 6^{m}$ υ Capric. Oc. D. 109°; Aug. 3, $13^{h}\ 17^{m}$ ρ Aquarii Oc. R. 314°; Aug. 9, $13^{h}\ 33^{m}$ ϵ Arietis Oc. R. 290°; Aug. 24; $7^{h}\ 2^{m}$ B. A. C. 5347 Oc. D. 358°; Aug. 25, $8^{h}\ 5^{m}$ B. A. C. 5800 Oc. D. 107°; Aug. 27, $7^{h}\ 17^{m}$ B. A. C.

6576 Oc. D. 81°. The angles at disappearance (D.) and reappearance (R.) are reckoned from the *apparent* N. point towards the right hand round the Moon's circumference as seen in an inverting telescope.

There is a near approach to some of the Pleiades (23, 27, and 28 Tauri) on Aug. 10, $11^h\ 33^m$ to $12^h\ 52^m$.

Mercury is an evening star till Aug. 23, when he is in inferior conjunction.

Venus is an evening star, at greatest brilliancy on Aug. 19.

Aug. 1, R.A. $11^h\ 32^m{\cdot}7$, Dec. 1° 19′ N., tr. $2^h\ 54^m$, sets $9^h\ 4^m$.
Aug. 31, 12 20 ·7, 9 14 S., 1 43 , 7 0

The illuminated portion of the disk is 0·49 on July 15 and 0·29 on Aug. 15, and the corresponding diameters $24''{\cdot}0$ and $36''{\cdot}0$ respectively.

Mars is in Aries. His diameter is $12''{\cdot}6$ on Aug. 1 and $15''{\cdot}5$ on Aug. 31.

Aug. 1, R.A. $2^h\ 15^m{\cdot}3$, Dec. 10° 54′ N., tr. $17^h\ 35^m$, rises $10^h\ 35^m$.
Aug. 31, 3 14 ·8, 15 34 N., 16 36 9 10

Jupiter is in opposition on Aug. 31, retrograding through Aquarius. His diameter is $45''{\cdot}2$ on Aug. 1, and $46''{\cdot}4$ on Aug. 31.

Aug. 1, R.A. $22^h\ 53^m{\cdot}0$, Dec. 8° 33′ S., tr. $14^h\ 12^m$, rises $8^h\ 51^m$.
Aug. 31, 22 39 ·8, 9 58 S., 12 0 , 6 48

Jupiter's Satellites:—Aug. 2, $14^h\ 16^m$ iv. Ec. D.; Aug. 4, $14^h\ 2^m$ ii. Sh. I., $15^h\ 21^m$ ii. Tr. I.; Aug. 5, $13^h\ 8^m$ i. Ec. D.; Aug. 6, $10^h\ 22^m$ i. Sh. I., $10^h\ 59^m$ i. Tr. I., $11^h\ 40^m$ iii. Oc. R., $12^h\ 41^m$ i. Sh. E., $12^h\ 51^m$ ii. Oc. R., $13^h\ 18^m$ i. Tr. E.; Aug. 7, $10^h\ 30^m$ i. Oc. R.; Aug. 11, $9^h\ 50^m$ iv. Tr. E.; Aug. 12, $15^h\ 3^m$ i. Ec. D.; Aug. 13, $9^h\ 44^m$ iii. Ec. D., $11^h\ 24^m$ ii. Ec. D., $12^h\ 16^m$ i. Sh. I., $12^h\ 43^m$ i. Tr. I., $14^h\ 35^m$ i. Sh. E., $15^h\ 0^m$ iii. Oc. R., $15^h\ 2^m$ i. Tr. E., $15^h\ 6^m$ ii. Oc. R.; Aug. 14, $9^h\ 32^m$ i. Ec. D., $12^h\ 14^m$ i. Oc. R.; Aug. 15, $9^h\ 4^m$ i. Sh. E., $9^h\ 28^m$ i. Tr. E., $9^h\ 39^m$ ii. Tr. E.; Aug. 19, $15^h\ 27^m$ iv. Oc. R.; Aug. 20, $13^h\ 46^m$ iii. Ec. D., $13^h\ 59^m$ ii. Ec. D., $14^h\ 10^m$ i. Sh. I., $14^h\ 27^m$ i. Tr. I.; Aug. 21, $11^h\ 27^m$ i. Ec. D., $13^h\ 58^m$ i. Oc. R.; Aug. 22, $8^h\ 36^m$ ii. Sh. I., $8^h\ 39^m$ i. Sh. I., $8^h\ 53^m$ i. Tr. I., $9^h\ 4^m$ ii. Tr. I., $10^h\ 58^m$ i. Sh. E., $11^h\ 11^m$ i. Tr. E., $11^h\ 29^m$ ii. Sh. E., $11^h\ 55^m$ ii. Tr. E.; Aug. 23, $8^h\ 24^m$ i. Oc. R.; Aug. 27, $16^h\ 4^m$ i. Sh. I., $16^h\ 10^m$ i. Tr. I.; Aug. 28, $13^h\ 22^m$ i. Ec. D., $15^h\ 42^m$ i. Oc. R.; Aug. 29, $10^h\ 33^m$ i. Sh. I., $10^h\ 36^m$ i. Tr. I., $11^h\ 15^m$ ii. Sh. I., $11^h\ 21^m$ ii. Tr. I., $12^h\ 52^m$ i. Sh. E., $12^h\ 54^m$ i. Tr. E., $14^h\ 7^m$ ii. Sh. E., $14^h\ 11^m$ ii. Tr. E.; Aug. 30, $10^h\ 8^m$ i. Oc. R.; Aug. 31, $7^h\ 49^m$ iii. Sh. I., $7^h\ 50^m$ iii. Tr. I., $8^h\ 39^m$ ii. Oc. R., $11^h\ 19^m$ iii. Tr. E. $11^h\ 24^m$ iii. Sh. E.

Saturn is nearly stationary in Pisces.

Aug. 1, R.A. $1^h\ 2^m{\cdot}0$, Dec. 3° 51′ N., tr. $16^h\ 20^m$, rises $9^h\ 57^m$.
Aug. 31, 0 58 ·7, 3 23 14 19 7 59

	Outer Ring.		Inner Ring.		Ball.
	Maj. Axis.	Min. Axis.	Maj. Axis.	Min. Axis.	Diam.
Aug. 8...	$42''{\cdot}15$	$7''{\cdot}59$	$28''{\cdot}03$	$5''{\cdot}04$	$16''{\cdot}9$
Aug. 28...	$43''{\cdot}39$	$7''{\cdot}53$	$28''{\cdot}86$	$5''{\cdot}01$	$17''{\cdot}4$

The south side of the rings is visible, the elevation of the Earth above their plane being 10° 22′ S. and 10° 0′ S. on Aug. 8 and Aug. 28 respectively, and of the Sun 8° 11′ S. and 8° 47′ S.

EDITOR.

Publications received:—Prof. Challis, *Lectures on Practical Astronomy and Astronomical Instruments* (Deighton, Bell, & Co., Cambridge, 1879)—*Annales de l'Observatoire Royale de Bruxelles*, vols. i. & ii. (Bruxelles, F. Hayez)—*Annales de l'Observatoire de Moscou*, vol. v. part 2 (Moscow, 1879)—Leonard Waldo, *Report of Observation of Total Solar Eclipse, July* 29, 1878, *made at Fort Worth, Texas* (Cambridge, U.S., 1879)—G. H. Darwin, *On the Bodily Tides of Viscous and Semi-elastic Spheroids* (Phil. Trans. part i. 1879)—*Memorie degli Spettroscopisti Italiani*, 1879, June—*Popular Science Monthly*, 1879, June (Appleton, New York)—*American Journal of Science*, 1879, June (Newhaven, Conn.)—*Science Observer*, 1879, April (Boston Scientific Society)—*Kansas City Review*, 1879, June—*Sirius* (Leipzig). EDITOR.

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THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

No. 28. AUGUST 1. 1879.

The Meteors of April 12–26, 1879.

Although it was hardly expected that any considerable return of the April Lyraids would be observed this year, still it was important that observers should keep a good look out for any meteors belonging to this stream, especially on the 19th to 20th, the date of its maximum display, as its varying intensity from year to year is a valuable point to note, the cluster having a suspected period of about 27 years. It is also very essential that the radiant-point of this meteor-stream should be more accurately fixed, its hypothetical connection with Comet I., 1861, being a little doubtful.

The centre of radiation appears to be very diffuse; and this may possibly result from the coexistence of one or more showers with closely adjoining radiants.

Its limits of duration also need to be more exactly determined.

In the hope of contributing some results towards the determination of these important elements, and also of helping to confirm, if possible, the suspected existence of many contemporary displays pointed out by Mr. Denning, the observations were mainly undertaken this year. The observations were commenced as early as April 12, and terminated on the 26th, the moonlight and cloudy weather preventing a further continuance of the watching.

I have thought it of sufficient importance that the details of each night's work should be given, as well as a summary of the deduced results.

April 12. A watch of $1\frac{1}{2}$ hour was sustained between $10^h\ 30^m$ and 12^h in a perfectly clear sky, and only 5 meteors were recorded, none being Lyraids. At $11^h\ 36^m$ a nearly stationary meteor of the 1st mag. was seen accurately at 223° −2°. This appears to agree with a position deduced by Mr. Denning from the Italian Met. Assoc. Obs. 1872, as at 228° −5°, March 31 to April 13, and with a stationary meteor recorded by Dr. Weiss, April 20, 1874, at 223°·4 −5°·8. April 13 cloudy. On the 14th a watch of $1\frac{1}{2}$ hour, from 9^h to $10^h\ 30^m$, revealed only 4 meteors, none being

Lyraids. Two stationary meteors of the 2nd and 3rd magnitudes respectively were recorded, the former at 206° −8°, and the latter at 207° +25°, the positions being very accurately noted. The first supports a new and active shower deduced by Mr. Denning, from his own observations, at 206° −8°, April 16 to May 31, and from Italian Met. Assoc. Obs. 1872, at 204° −8°. Mr. Corder places the radiant from his observations at 208° −6°. Mr. Denning also noted the shower in 1874 and 1876 at 209° −3° and 207° −7° April 13–27. The close accordance between the several independent determinations of the radiant-point would seem to fix the exact centre with remarkable precision, the mean being 206°·7 −6°·7. A succession of cloudy and stormy evenings unfortunately prevented any further watching until the 19th, when an observation of 2½ hours, from $10^h\ 30^m$ to 13^h, was obtained (the sky being clear), and only 4 meteors were recorded, there seeming to be a remarkable paucity of shooting-stars. Of the 4 recorded, one was undoubtedly a Lyraid. Two others with foreshortened paths, near α Lyræ, of a decided blue colour, with short streaks and rapid motion (which would most likely be taken by the majority of observers for members of the Lyraid shower, especially if not accurately noted), indicated very strongly a position a few degrees north of the true Lyraids; and from two other foreshortened paths, recorded very accurately on the following evening (20th), a radiant-centre was well deduced at 272° +41°·5. This position is supported by a stationary meteor, recorded by Dr. Weiss, April 21, 1870, at 272°·8 +40°·1. If this should prove to be an independent contemporary display, it would perhaps serve to explain the heretofore recorded discrepancy in the many independent deductions of the true Lyraid radiant-centre, from the fact that its members have been confounded with the true Lyraids. Further observations of this strongly suspected concomitant shower are required. A watch of 3½ hours, from $8^h\ 30^m$ to 12^h, was obtained on the 20th, and meteors were found to be more plentiful, 18 being recorded, 4 of them being very bright, =1st-mag. stars. A careful scrutiny of the recorded paths indicated a fertile return of the Lyraids, 8 meteors being undoubtedly members of this stream. Their paths were rather short, their motions rapid, with streaks, and nearly all were of an orange colour. No foreshortened paths were recorded near the centre of radiation, but the paths mapped gave a very well-determined position at 274° +34° (sky remarkably clear). On the 21st 10 meteors were recorded during a watch of 2½ hours, from $8^h\ 30^m$ to 11^h, only one being a Lyraid. Two nearly stationary meteors were quite accurately fixed at 262° +17° and 238° −7°. During a watch of 2 hours, from $8^h\ 30^m$ to $10^h\ 30^m$, on the 22nd (the sky being clear), only 5 meteors were recorded, 3 of them very faint. No Lyraids seen. On the 23rd an hour's watch was taken, in a clear sky, from 11^h to 12^h, and only 4 were recorded. The 24th and 25th were cloudy. The last observation was obtained on the 26th from $11^h\ 30^m$ to 13^h (1½ hour), and 6 meteors were mapped, none being Lyraids. These observations seem to show the Lyraids to have been a feeble and short-lived shower this year, and also that the contemporary showers (as observed in the east quadrant) were few and unimportant. The following centres of radiation were deduced from the 56 meteors mapped:—

1.	Apr. 12–26	R.A. 206°	Dec. − 8°	5 meteors.
2.	„ 20–26	242	+24	4 or 5 „
3.	„ 20–22	245	+50	3 „
4.	„ 22–26	253	+ 5	5 „
5.	„ 19–20	272	+41·5	4 „
6.	„ 19–21	274	+34	10 „

1. Meteors quite bright, short, and rather slow; position very accurately deduced.

2. Meteors quite faint, short, and rapid. A feeble return of the Coronids.

3. Meteors quite faint, short, and rapid. Draconids, No. 1.

4. Meteors bright, short, and rapid; position very accurately deduced from a 1st-magnitude stationary meteor seen on the 22nd and visible one second. Principally seen on the 23rd. Appears to confirm a position deduced by Mr. Denning from Italian Met. Assoc. Obs. 1872, at 251° +8°, May 3–15. Dr. Weiss recorded a stationary meteor April 22, 1874, at 248°·8 +6°·1; Denning, April 16–19, at 220° +2°.

5. Meteors quite bright, short, and rapid; position well deduced. New?, or possibly a branch of the Lyraids.

6. Meteors generally bright, short, and rapid. Lyraids.

There were other feeble showers suspected from positions at R.A. 219° −8°, 207° +25°, and 237° +5°, 3 meteors each.

On April 19 there was an auroral display in the N., confined to a steady glow with a few faint streamers. Observations made at Cambridgeport, Mass., U.S.A. EDWIN F. SAWYER.

Spectroscopic Observations on Brorsen's Comet, 1879.

I SAW this comet for the first time on March 20, but was prevented by clouds from making any observations on that night. I saw its spectrum for a very short time with a small star-spectroscope, and noted three bright bands in it and a very faint continuous spectrum.

I observed the comet a second time on March 25, when it was so bright that I could see it in the finder. With power 240 it was very granulated and showed a large condensation in the middle.

The spectrum contained three bright lines, very wide and misty, on both sides; and it was not possible to reduce them to a line with any width of slit. There was also a faint continuous spectrum in the field of the spectroscope between 5732 and 4550 tenth-metres of wave-length. I measured on this day the three bands five times with a micrometer.

I was prevented by clouds from again observing the comet till April 14. The comet was then so bright that I could see it in the twilight with the 6½-inch Merz refractor; it showed a large condensation in the centre, where there was a little nucleus like a star of the tenth magnitude. I observed certainly polarized light on the comet with a Vogel's polariscope (Nicol prism and oscillating quartz). Of the three bands, that in the middle was the brightest, that in the yellow was next in brightness, and that in the green was the faintest.

The continuous spectrum was measured as follows:—from 5695 to 4640 tenth-metres of wave-length. Ten measures were made of the bright lines, 5 being made at the beginning of the observations, after this a comparison of the cometary spectrum with hydrocarbon gases, and finally 5 more measures of the bright lines.

The continuous spectrum was not at all emitted from the nucleus of the comet, as was the case with the comet Coggia in 1874. In this comet the continuous spectrum is as extended as the bright lines, whilst with the comet Coggia the continuous spectrum was only a narrow line, but much brighter than in this comet.

May 15. I made only five micrometric measures of the bright lines of the comet-spectrum. The comet was very large, but not so bright as it had been. The state of the atmosphere was very bad this evening.

The mean of 20 measures gives the following values for the wave-lengths:—

Band	I. 5605	tenth-metres.
Band	II. 5146	„
Band	III. 4823	„

I compared the spectrum of the comet with the following hydrocarbon spectra, and have also measured their lines for comparison with those of the comet:—1, the blue flame of a Bunsen burner (the gas was from benzine); 2, an original tube by Dr. Geissler (Bonn), marked C_2H_3; 3, a similar tube marked "Alcohol"; and 4, a similar tube marked "Leucht-gas" (coal-gas); and I give the results for all these, together with the measures of lines in a hydro-carbon spectrum by M. Lecoq de Boisbaudran.

	Comet Brorsen.	Bunsen burner.	C_2H_3.	"Leucht-gas"*.	"Alcohol."	C_2H_3, Lecoq de Boisbaudran.
I....	5605	5638	5580	5580	5580	5629
II....	5146	5148	5155	5148	5148	5161
III....	4823	4825	4860	4848	4825	4738

---

* "Leucht-gas" is probably illuminating-gas from the Bonn gasometer and probably made from Ruhrort coals.

The Geissler tubes were made incandescent with a Ruhmkorff induction-coil of moderate dimensions and two pint Bunsen cells.

I think from the preceding little table we can see that the spectrum of Brorsen's comet is analogous with those of hydrocarbon; the little differences may result from the different pressures in the tubes; and it seems very desirable that anybody who makes vacuum tubes should indicate also the pressure in them.

On this occasion I must also add a few words as to the definition of my large reflector, constructed after the "Lindsay model" by Mr. John Browning of London. In the beginning of this month I deduced the value of a revolution of the screw of a Browning's double-image micrometer, and observed for this purpose *α* Ursæ Minoris in the afternoon. In the evening I was curious to see its companion. I looked for it before sunset, and I saw it for the first time, with power 208 (achromatic), *five minutes after sunset.* I called my astronomer M. Regler to see it, and this gentleman saw the companion very well 10 minutes after sunset. I think this is the best recommendation for Mr. Browning's reflector. My reflector has a clear aperture of 10¼ inches and 6 feet 5 inches focus. NICOLAS DE KONKOLY.

O Gyalla Observatory, 1879, June 30.

Conjunction of Mars and Saturn, 1879, *June* 30, *observed at Cadiz.*

I RETURNED from the country on the 27th, exclusively for the purpose of observing this phenomenon. Some time ago I received a reflecting equatoreal telescope by Browning of 8½ inches; through my absence the instrument was dismounted and I erected it for the first time on the evening of the 27th.

I had never seen an instrument of this class before, and I experienced some difficulties in arranging the stand, circles, speculum, flat, &c.; notwithstanding this, in two hours all was put in order, and the equatoreal erected on the terrace of my house.

On the 30th the night was superb and not a single cloud was visible, but the air was unsteady; the wind was light, blowing from N.E., and warm. I obtained, however, some beautiful views of Jupiter with a power of 208 diameters.

At 12^{h} 23^{m} Cadiz mean time, I perceived the two planets in conjunction with an eyepiece magnifying 60 times; but, on account of their low altitude, I could not see their details. Mars was gibbous, and red, as in a non-achromatic instrument. Saturn partially red, and with motions like expansions and contractions; it was impossible to see the ring.

At 12^{h} 40^{m} the two planets were easily divided by the naked eye; one was red and the other white, very brilliant: through the telescope Mars was red and Saturn yellowish like a topaz; the *ansa* near Mars was red and the more distant one very light green.

At $12^h\ 50^m$, with power 125, I saw the images rainbow-tinted (*irisadas* in Spanish) and diffused; I perceived two very small stars at 4 and 8 minutes approximately from Mars forming a right angle. The two planets were seen at the same time in the field of the telescope.

At $13^h\ 0^m$ I saw Saturn with many details, but Mars continued rainbow-tinted.

At $13^h\ 5^m$ I put in the eyepiece of 208 diameters, and I could perceive very distinctly the belts or bands of Saturn, but the definition of Mars was not improved.

At $13^h\ 20^m$ I inserted a low eyepiece, and pushing it out of focus I formed two tangent disks; that of Mars was reddish and yellowish and that of Saturn pale green; focussing properly with this eyepiece I saw the two small stars near Mars, and two satellites of Saturn. Mars, one of the stars, and the more conspicuous satellite were exactly in a straight line; the colour of Mars was red, and that of Saturn greenish, and nearly white, but not livid or gloomy.

At $13^h\ 30^m$ I saw with this eyepiece four satellites of Saturn.

These are all my observations on the conjunction; I could not employ the micrometer to take any measures, as I had not had an opportunity of trying it. AUGUSTO T. ARCIMIS.

Cadiz, 1879, July 6.

*The University Observatory, Oxford**.

In accordance with the Statute "Of the University Observatory," I have now the duty of laying before the Board of Visitors the third Report of the various kinds of work which have been carried on at the University Observatory during the last twelve months, and of the general condition of the instruments therein. The Lecture Room is completed in every particular, and I have found the interior arrangements very convenient for the purposes originally designed.

Lectures.—During the Michaelmas Term, fourteen students attended lectures. In the Hilary Term, two, and during the Easter and Trinity Terms, ten. Independently of these lectures, four graduates, connected with tuition in the University, have availed themselves of the use of the instruments to a very considerable extent, in order to complete their acquaintance with practical Astronomy. Two of these latter gentlemen studied also and applied themselves to the use of the Sextants, which have been provided out of the funds of the Observatory. The two public lectures on the Astronomy and Astronomical Instruments of Ptolemy and Hipparchus were attended, respectively, by fifty-eight and ninety-one members of the University and their friends, and I was happy to observe a considerable proportion of undergraduates among them. Life and health being given, I feel encouraged to continue this form of lectures in future Terms, until I shall have detailed and illustrated the history of Astronomy to the present epoch. Lectures also were offered on the Physical Libration of the Moon and on Solar Physics. But until some material advantages are offered to students, in some degree corresponding to those valuable and numerous rewards which are at once reaped from the other studies of the University, I can hardly expect that justice can be done to the difficult subject of Astronomy, or to what I believe to be the unusually great opportunities provided by the University for the students at the Observatory. In plain language, the study of Astronomy is supposed not to pay sufficiently well in the public examinations to encourage students to take it up.

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* Report of the Savilian Professor to the Board of Visitors, read 1879, June 4.

Instruments, &c.—The large Equatoreal Refractor and the De La Rue Reflector are in good working condition; but it is the experience and the dictum of one of the ablest of modern astronomers, that a thorough examination of important optical instruments about every four years is essential to their successful preservation. I suggest, therefore, that the large Equatoreal be overhauled by its constructor in the course of the year. This instrument has been in constant use for the formation of an accurate chart of about forty of the stars in the Pleiades, partly (as I have already stated in my last Report) with the view of ascertaining the proper motions of the members of this remarkable group by comparing their present co-ordinates with those obtained by Bessel, half a century ago; and partly with the hope of ascertaining whether the new duplex Micrometer, with which the measures have been taken, bears a fair comparison with the merits of the far more costly Heliometer. The results, now in course of reduction, I propose to publish in Part II. of the Oxford Observations. Nearly 300 photographs of the Moon have been taken by means of the De La Rue Reflector, and the measures of those already secured for the computation of the Moon's physical libration are advancing favourably. Considerable labour has been bestowed upon the determination of the scales of the De La Rue measuring engine; and it is satisfactory to find from various intercomparisons that the scales of this instrument, and of the duplex Micrometer, and of the ordinary filar Micrometer, are in good accord. With the view of ascertaining how far the lunar photographs can be relied on for minute and delicate measurements, micrometrical measures of the shadows of several prominent lunar mountains were secured in the Refracting Telescope, simultaneously with the taking of photographs with the Reflector; the latter were then measured in the De La Rue. engine, and it was satisfactory to find that the results of the telescopic and of the photographic measures were in close accordance, within the limits of the unavoidable errors of observation. Having acquired this confidence, I devised an improvement on Olbers's method of computing the heights of lunar formations, and have applied the method to the practical determination of the heights of several mountains. I may here mention that a still further proof of the reliability of celestial photography (in at least one of its applications) may be gathered from the following facts. From measurements and computations founded on sixteen of the Oxford photographs, a provisional determination of the Moon's semi-diameter has been obtained. The Oxford photographic result differs from the 'Nautical Almanack' value only by 0''·118. It may also be remarked that the *probable error* of this result is less than one half of the *probable error* of the result obtained by Wichmann, who adopted methods of reduction similar to ours, but who, instead of a photographic process, observed with Bessel's Königsberg Heliometer. The results will be embodied in the forthcoming fasciculus of Observations.

The periodical comets of Tempel and of Brorsen have been well observed, and it is my intention to adopt these measures in the determination of the elliptic elements of the present orbit of the latter.

In the first fasciculus of the Oxford Observations, the computation of the elements of the elliptic orbits of a few binary stars were published; these stars have been re-observed, and it is satisfactory to find the observed and computed relative positions of the binary components in remarkably fair accordance. These results, also, it is proposed to publish at the end of the present year.

I propose during the next few months to apply the three telescopes of different apertures, mounted on the large Refracting Equatoreal, in the measurements of the diameters of five of the planets, applying to each of the three telescopes a filar and an Airy's divided eyepiece micrometer. The comparison of the results may be hoped to be both interesting and instructive. These also, it is hoped, will find a place in the next fasciculus of our Observations.

Dr. De La Rue, with his usual generosity, has been at the expense of procuring the refiguring of one of the silver-on-glass mirrors of 13 inches aperture, by Mr. With. This most skilful artist speaks of it as his final and best performance.

I trust that the preparation and the delivery of the lectures referred to, and the completion of the astronomical researches here detailed, may be accepted as evidences of the activity of the Observatory, and of the realization of the objects for which it has been established.

The Library.—Several important presents of books have come to us from various sources, notably from the Observatories of Pulkova and Washington. The German government also has presented the Observatory with Schmidt's large Map of the Moon, which is now mounted and placed on the wall of the Lecture Room. Lady Herschel has also presented the Observatory with a valuable photographic portrait of Caroline Herschel.

The room provided for the Library is now ready to receive its book-cases. There is a balance reserved in the hands of the Curators of the University Chest voted by Convocation for this purpose; I would suggest that the Board of Visitors should apply for the sum that is necessary for the proper reception of the books already existing, and for those expected in the future. There will then be ample room for the ancient and valuable books contained in the Savilian Library, which are at present very inadequately and inconveniently (not to say dangerously) housed in a very small room contiguous to the Bodleian Library. If placed in the University Observatory they would be far more accessible and probably more useful. For this transfer I understand that I have the consent of the Savilian Professor of Geometry, who with me is a joint keeper of Sir Henry Savile's Library. If such an arrangement is to be effected, it would be proper to supply the Savilian Professor of Geometry with a key of the University Observatory and of the Library therein.

During the last three months I have, from the resources of the Observatory, supplied two of the city journals with the weekly duration of sunshine and the amount of the weekly rainfall. The former is, I believe, at present unique in its announcement.

The grant of £400 per annum for four years, and the generous allowance of £100 per annum paid by Dr. De La Rue for the stipend of the second assistant during the same period, both expire on December 31 of the present year. By dint of a most scrupulous economy, these sums have hitherto sufficed to ensure the efficient progress of the Observatory business. But they have left no margin whatever for the purchase of necessary books, and the stipend of the first assistant (now £260 and increasing by £20 annually until it reaches £300 per annum) would absorb by far the greater part of the present University allowance, leaving no provision whatever for the junior assistant, nor for the other moderate but unavoidable expenses of the establishment.

It may be desirable here to remind the Board of Visitors, that in June 1877 a committee of their Board inquired into the question of what annual expenditure was necessary for the efficient but economical conduct and maintenance of the Observatory. The result of their deliberations was that the sum required amounted to £710 per annum*, and the necessity for providing this sum was

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* The items of this expenditure, as transmitted by the Board of Visitors to the University Commission, are as follows:—

First Assistant	£300
Second Assistant and Photographer	150
Attendant	80
Library and Building	30
Maintenance of Instruments	30
Photography	35
Coals, Water, and Gas	35
Sundries (say)	50
	£710

communicated by the Vice-Chancellor, as chairman of the Board, to the University Commissioners, now in session. For the present, however, and until the arrangements of the Commissioners are completed, it may be possible to postpone the realization of the whole of this recommendation of the Board, and for the next few years the necessary expenses might be covered by a sum of £600 per annum, now that Dr. De La Rue's subsidy has properly come to an end. Of course all this arrangement depends upon the judgment of Convocation.

Very loyal and able co-operation has been invariably accorded to me by the two intelligent assistants attached to the Observatory.

It was with no slight satisfaction that I found our home resources sufficient for the reconstruction of working models of Ptolemy's ancient astronomical instruments, and of other accessories required for the illustration of my lectures. C. PRITCHARD.

Magnetism and Solar Spots.

AMONG the various terrestrial phenomena the changes in which it has been sought to connect with the changes observed in solar spots, there is perhaps only one in which such correspondence can be said to be any thing like well made out—that of magnetism. Not but that if the solar-spot variation is felt in one way, it must also be felt in others; more than probably this is the case, but the connexion can scarcely be accepted as satisfactorily established, except as regards the element mentioned, the accumulated evidence in respect to which seems to prove that the magnetic diurnal ranges undoubtedly increase and decrease in magnitude with the increase and decrease of solar spots. The connexion was many years ago first suggested by General Sir E. Sabine. Dr. Wolf, of Zürich, has since been one of the most diligent workers at the question: his investigations on the subject are to be found in his 'Astronomische Mittheilungen,' results taken from which we have at different times laid before our readers. He lately communicated to the memoirs of the Royal Astronomical Society (Vol. xliii.) a very complete Table of monthly sun-spot values extending from the year 1749 to the year 1876. This Table attracted the attention of Mr. Ellis, of the Royal Observatory, Greenwich, who was induced to apply the long series of magnetic observations made at the Royal Observatory, and reaching back to the year 1841, as an independent test of the accuracy of the generally received relation between solar spots and magnetism. His results are embodied in a paper read before the Royal Society on 1879, May 8, "On the Relation between the Diurnal Range of Magnetic Declination and Horizontal Force, as observed at the Royal Observatory, Greenwich, during the years 1841 to 1877, and the Period of Solar Spot Frequency."

We believe that the comparison of magnetic diurnal ranges with sun-spot changes had hitherto been made only in the case of the element of Declination. But Mr. Ellis wished to institute a more complete comparison, and desired to include, not only the variations of Declination, but also those of Horizontal Force and Vertical Force, representing, respectively, variations towards the west, towards the north, and directly downwards—a much more thorough manner of dealing with the question. He found, however, that the results for vertical force were not very manageable; the vertical-force instrument had been more than once changed, and the results present some anomalies, which are possibly in part instrumental. The inequalities of Declination and of Horizontal Force are, however, both completely discussed: in regard to these elements, the observations have, throughout, been made on the same general plan and with the same instruments; the results are therefore well adapted for an inquiry of the kind. The observations are treated from 1841 to 1877, a period of 37 years. The mean diurnal range of declination in each individual month (omitting days of unusual magnetic disturbance) is taken to represent the magnetic energy of the month. Similarly for horizontal force. Two series of numbers are thus formed. Each series is then smoothed by taking the mean of each twelve consecutive monthly numbers, and again the mean of each two consecutive numbers. The numbers so obtained are strictly comparable with those given by Dr. Wolf in the Table already referred to, in preparing which he has used a precisely similar process. From these various series of numbers curves are drawn which show great similarity. Epochs of minima occur in the years 1843, 1856, and 1867, and epochs of maxima in the years 1848, 1860, and 1870. On tabulating the several epochs it is found that the mean magnetic epoch, on the whole, follows the sun-spot epoch by 0·27 of a year: by another method of treatment this becomes reduced to 0·10 of a year. It was no part of Mr. Ellis's plan to determine the average length of the period; he simply wished to show that the magnetic and sun-spot oscillations are in close accord, and this appears to be completely done.

The general circumstance that the diurnal ranges of magnetic elements are subject to an eleven-year period, similar to that of sun-spots, being considered to be satisfactorily established, it seemed desirable to ascertain whether the more fitful changes of the phenomena in any way also correspond. In order to make this comparison, the magnetic numbers, instead of being smoothed, as before described, were now cleared only of the *average* annual inequality, and compared with the *actual* monthly sun-spot numbers (not the smoothed numbers as before). Curves are given, founded on these numbers, and they show some very remarkable correspondences between the more rapid sun-spot and magnetic variations, especially between the years 1869 and 1873. It is remarked that the magnetic results appear to be sensibly improved since the instruments were placed (in the year 1864) in an underground apartment in which the diurnal range of temperature is less than 1°.

Further inquiry seemed to point to a variation in the annual inequality of magnetic diurnal range. The mean annual inequality is determined for groups of three years, at each epoch of minimum and maximum, both for declination and horizontal force. Then, allowing for the possible influence of the accidental sun-spot variation, it appears, on the mean of the whole, that the annual inequality is sensibly increased at the time of a sun-spot maximum, and diminished at the time of a sun-spot minimum. It is difficult to enter further here on this interesting point.

The following are the general conclusions supposed to be derived from the whole inquiry:—

1. That the diurnal ranges of the magnetic elements of declination and horizontal force are subject to a periodical variation, the duration of which is equal to that of the known eleven-year sun-spot period.

2. That the epochs of minimum and maximum of magnetic and sun-spot effect are nearly coincident; the magnetic epochs, on the whole, occurring somewhat later than the corresponding sun-spot epochs. The variations of duration in different periods appear to be similar for both phenomena.

3. That the occasional more sudden outbursts of magnetic and sun-spot energy, extending sometimes over periods of several months, appear to occur nearly simultaneously, and progress collaterally.

4. That it seems probable that the annual inequalities of magnetic diurnal range are subject also to periodical variation, being increased at the time of a sun-spot maximum, when the mean diurnal range is increased, and diminished at the time of a sun-spot minimum, when the mean diurnal range is diminished.

Conclusions Nos. 1, 2, and 3 appear to be sufficiently certain, but the evidence in favour of No. 4 is not so decisive.

We remark with satisfaction one point in this inquiry. The magnetic results have been published from time to time in the Greenwich Observations, as also have Dr. Wolf's numbers in his 'Astronomische Mittheilungen.' The thorough independence of the data is thereby assured.

Double Stars for August.

THE principal pairs between 16^h and 21^h are as follows:—

ν Scorpii. R.A. 16^h 5^m, Dec. $-19°\ 9'$.

Mag. A 4, B 5, C 7, D 8.

A B	1878·2	0°·8	0″·8	Burnham.

A was discovered to be a close double by Mr. Burnham in 1874. Most probably a binary system.

A C	1877·5	336°	40″·7	Flammarion.
C D	1877·4	47·4	1·86	Dembowski.

CD also most probably form a physical pair.

Σ 2021 = 49 Serpentis. R.A. $16^h\ 7^m{\cdot}7$, Dec. +13° 51′.

Mag. 6·7, 6·9.

1876·5	327°	3″·56	Wilson.

Binary. Common proper motion $+0^s{\cdot}152$ in R.A. and $-0''{\cdot}369$ in N.P.D.

Σ 2032 = σ Cor. Bor. R.A. $16^h\ 10^m{\cdot}2$, Dec. +34° 10′.

Mag. A 5, B 6, C 13, D 10.

A B	1877·4	202°·3	3″·68	Wilson.
A C	1876·4	221·7	15·92	Hall.
A D	1877·4	87·6	54·15	Flammarion.

AB form a well-known binary system. Doberck's elements are:—$T = 1826{\cdot}93$, $☊ = 16°\ 27'$, $\lambda = 73°\ 51'$, $\gamma = 31°\ 56'$, $e = 0{\cdot}7515$, $a = 5''{\cdot}885$, $P = 845{\cdot}86$ years.

Σ 2055 = λ Ophiuchi. R.A. $16^h\ 24^m{\cdot}9$, Dec. +2° 15′.

Mag. 4, 6.

1877·4	32°·3	1″·56	Doberck.

Dr. Doberck's elements are:—$☊ = 157°\ 21'$, $\lambda = 94°\ 16'$, $\gamma = 44°\ 44'$, $e = 0{\cdot}4930$, $P = 233{\cdot}89$ years, $T = 1803{\cdot}91$.

Σ 2084 = ζ Herculis. R.A. $16^h\ 36^m{\cdot}8$, Dec. +31° 49′.

Mag. 3, 6·5.

1877·5	134°	1″·24	Hall.

Flammarion's elements are:—$☊ = 26°\ 13'$, $\lambda = 51°\ 11'$, $\pi = 287°\ 10'$, $T = 1864{\cdot}90$, $e = 0{\cdot}405$, $a = 1''{\cdot}36$, $P = 34{\cdot}58$ years.

Σ 2130 = μ Draconis. R.A. $17^h\ 2^m{\cdot}8$, Dec. +54° 38′.

Mag. 5, 5.

1877·6	169°·9	2″·49	Doberck.
78·8	171·4	·56	Goldney.

An interesting object. Probably binary. Variable.

Σ 2262 = τ Ophiuchi. R.A. $17^h\ 56^m{\cdot}5$, Dec. −8° 11′.

Mag. 5, 6.

1877·6	250°·5	1″·90	Stone.

Doberck's elements are:—$☊ = 65°\ 26'$, $\lambda = 41°\ 24'$, $\gamma = 58°\ 42'$, $e = 0{\cdot}6055$, $P = 217{\cdot}87$ years, $T = 1821{\cdot}91$.

Σ 2272 = 70 Ophiuchi. R.A. $17^h\ 59^m{\cdot}4$, Dec. +2° 32′.

Mag. 4·5, 6.

1877·7	78°·5	3″·12	Stone.
78·7	71·9	·17	Goldney.

Σ 2382 = 4 Lyræ. R.A. $18^{h}\ 40^{m}{\cdot}4$, Dec. +39° 33′.

Mag. 4·6, 6·3.

1877·4	15°·3	3″·24	Doberck.
78·7	18 ·6	·28	Goldney.

Σ 2383 = 5 Lyræ. Mag. 4·9, 5·2.

1877·4	137°·6	2″·45	Hall.
78·7	139 ·5	·43	Goldney.

Σ 2579 = δ Cygni. R.A. $19^{h}\ 41^{m}{\cdot}2$, Dec. +44° 50′.

Mag. 3, 8.

1877·1	330°·1	1″·63	Dembowski.

Probably binary. Period 178 years (Hind), 415 years (Behrmann).

Σ 2737 = ε Equulei. R.A. $20^{h}\ 53^{m}{\cdot}1$, Dec. +3° 50′.

Mag. A 5·5, B 7, C 7·5.

AB	1877·7	289°·1	0″·83	Doberck.
$\frac{AB}{2}$ and C	1877·7	73 ·4	10 ·82	Flammarion.

A remarkable ternary system.

Joseph Gledhill.

Mr. Edward Crossley's Observatory,
Bermerside, Halifax.

Meteor Notes for August.

Evening observations may be made between the 11th and 25th, and in the mornings between the 15th and 29th. The *Perseids* should be watched for on every fine night during the first half of the month. They are not, however, visible in any numbers until about the 6th, and later than the 12th not many are seen. The morning hours when the radiant is high is the best time to observe them, and they may be expected to appear, as in 1871–74–77, at the rate of one per minute at least. This remarkable shower deserves more than ordinary notice at its return this year, from the fact that an 8-year period * of intensity is to be inferred from existing observations as follows :—

1839, Aug. 10. Heis observed 160 meteors per hour.

1847, Aug. 10. Heis at Aachen recorded 501 meteors between $9^{h}\ 10^{m}$ and $13^{h}\ 30^{m}$ ($4^{h}\ 20$), and in 1848 the horary number at Paris was 110.

1863, Aug. 10. An unusually rich display of Perseids. Prof.

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* See Phipson's 'Meteors, Aerolites, and Falling Stars,' p. 159.

Herschel referred to it as "the brightest star-shower that had been witnessed in England since the Leonids of Nov. 12th–13th, 1832."

1871, Aug. 10–11. An exceptionally brilliant return of the Perseids was observed on these nights.

The lapse of a similar interval brings us to the present year. There appear to be no records for 1855, at which time these Perseids should have been more abundant than usual, if the suspected periodicity held good, and it would be interesting to consult any observations made at that time.

If the weather is clear on the nights of August 9th, 10th, and 11th this year, the sky should be watched throughout, and the horary number of Perseids ascertained, so that the figures may be compared with the numbers similarly derived in preceding years, though it is unfortunate that moonlight will slightly interfere with observation, for on the 10th the moon (at her last quarter, and rising at $10^h\ 6^m$) is visible nearly all night, and will overpower many of the fainter class of meteors. There is more than one radiant-point, for, from the numerous records of stationary and much foreshortened meteors seen in Perseus at this epoch, no less than three centres are defined, namely at B Camelopardi, χ Persei, and γ Persei. Thus we require not only observations of the intensity of the display, but also of the several distinct points of departure; and it seems very likely that the shower at its ensuing return may attract much attention from the number and brightness of the meteors.

The list of known meteor-streams in activity this month has accumulated to such an extent of recent years, that it is difficult to select those of chief interest as affording the best displays; but the positions of many of these will be found in our previous references to August meteor-showers (see 'Observatory,' vol. i. p. 125, and vol. ii. pp. 131, 132). The observer cannot do better than watch during every available period as long as possible; then, by projecting the paths, deduce his own radiants and compare them with those of former observers. This is preferable to looking out for certain showers, some of which recur with such feebleness as to give little clue to their existence.

Fireballs should be looked for on the 3rd, 5th, 7th–13th, 15th, 19th–20th, and 22nd.

Recent Observations.—A bright meteor was seen by several persons at Bristol on the night of June 18; but the descriptions of its apparent path are too vague and insufficient to be serviceable. One observer writes:—"At 3 minutes to 11 this evening, June 18, while returning homewards, I became suddenly aware of a bright light illuminating the ground, and on looking upwards I saw a beautiful meteor. It appeared of immense size, nearly six inches in diameter, and was travelling very slowly from south to north in almost a straight line, leaving very little tail behind it."

W. F. DENNING.

CORRESPONDENCE.

To the Editor of 'The Observatory.'

Bright Lines of Oxygen in the Solar Spectrum.

SIR,—

I was not a little disappointed that, in the discussion on Dr. Draper's paper at the last meeting of the Royal Astronomical Society, no one took up the point which seems to me to be the one really at issue, viz. whether certain intervals between the absorption lines of the solar spectrum have or have not the right to be considered *bright lines.* I do not know that any one has yet ventured to assert that these intervals are one whit brighter than the average spectrum in their neighbourhood; but be this as it may, it is beyond all question that if there be any superiority of brilliance at these points, it is of the slightest.

The number and exactness of the coincidences between the intervals and the bright lines of oxygen have no direct bearing on the question. It is obvious that if an oxygen line happen to fall exactly in the centre between two solar absorption lines, due, say, to iron, it is no more proof that the bright space between them is a bright line of oxygen, than is the dark space between the two corresponding bright lines of iron, when obtained in a terrestrial laboratory, a proof of absorption exercised by the same gas. It would be interesting to learn that the wave-lengths of the three lines were in arithmetical progression; but this is all the observation would tell us.

The same argument holds good with regard to the dark lines of the other elements. The space between a line of titanium and another of manganese would have no more right to be considered a bright line, just because a bright line of oxygen happened to coincide with its centre, than the space between two iron lines. It may be objected that we cannot as yet "show cause" for the majority of the dark solar lines. They are not, on that account, less certainly the effect of absorption. If not absorption lines, they must be the effect of contrast. But it is impossible to conceive that bright lines so little exceeding the average spectrum in brilliance, that most observers fail to detect the difference, should yet by their contrast with that spectrum give rise to a fictitious appearance of dark lines. The utmost that can be conceded is that *possibly* one or two of the very feeblest spectral lines may originate in this way, an admission that does not affect the matter, for the spaces in question are bounded by dark and well-defined lines.

The dark lines, or, at all events, all but the very feeblest, being due to absorption, the spaces between have no significance whatever apart from them. They *may*, indeed, contain bright lines; but until these bright lines are exhibited as such—until, that is, certain portions of the spectrum are shown to be definitely brighter than the average—the bright spaces have no more significance than the dark spaces which separate star from star in the midnight sky. The stars are the important features in the sky, the rest is dark of necessity. The absorption lines are the important features in the spectrum, the rest is bright of necessity.

This leads us to a further consideration. The slight inexactness of a coincidence would be in itself no disproof of the connexion between certain bright lines in the solar spectrum and oxygen. We can hardly imagine that the lines of that gas are so neatly disposed with regard to those of all other spectra whatsoever, that in no single case does a line of any other element overlap a line of oxygen, still less that the other solar elements give pairs of lines so judiciously arranged that the oxygen lines invariably take up their position in the exact centre of a couple. Supposing, then, that oxygen does evidence itself in the solar spectrum by faint bright lines, we should expect the coincidences between these lines and those of oxygen to be by no means always perfect. Sometimes a dark absorption line would overlap the bright line, and make it appear not only displaced, but altered in character, or sometimes the absorption line would divide the bright line or even obliterate it. Such an exactness of coincidence as that of which Mr. Ranyard spoke, but which neither Dr. Draper claims, nor his photographs show, so far from proving the theory by "millions to one," would rather throw doubt on the accuracy with which the investigation had been conducted.

The existence of the bright lines remains yet to be proved; they may be in the spectrum, or they may not; and till this matter is decided it is useless to compare this or that spectrum, not with bright lines, but with "eligible sites" for them, if only they would consent to reveal themselves. The first point, therefore, is to examine the spectrum with the greatest possible dispersion, care being taken to employ long collimators and the "irreducible minimum" of slit opening, so that the spectrum may not only be extended, but pure. The dark lines will then occupy only a fraction of its length, and the intervening spaces can be carefully studied for local and minute differences of brilliance. Photographs should also be taken with a wide range of exposure, particularly with small exposures, by which the differences in brightness in the spaces will be exaggerated. When in these or in other ways Dr. Draper has succeeded in displaying bright lines in the solar spectrum, a comparatively rough approximation to a series of coincidences will suffice to trace their connection with oxygen, nitrogen, or carbon as the case may be.

There is no reason why we should hesitate to accept Dr. Draper's theory, if once the supposed bright lines could be indubitably exhibited as such. We can hardly believe that oxygen does not exist in the sun; and we have the case of the D_3 line to remind us that the vapours of the solar atmosphere do not invariably show their presence by dark lines. It could only be cause for unmixed satisfaction, if, after so many years of such hard and careful labour, Dr. Draper completed his work by proving that there are such differences of brilliance in the interlinear spaces, that some portions of the spectrum, and these coincident with the lines of oxygen, could be legitimately considered as consisting of bright lines.

I have throughout assumed that the groundwork of the solar spectrum is continuous. Some speakers at the Meeting seemed to question this; but the reasons for believing it are too obvious and too well known to justify me in further occupying your space, on which I have already trespassed too far, by their recapitulation.

I am, Sir, yours very truly,
E. W. Maunder.

Blackheath, S.E., 1879, June 14.

The Radiant-point of Comet I., 1870.

Sir,—

My attention was only a few days ago turned to the remarks of Mr. Denning in the 'Observatory,' Vol. ii. p. 165, on the radiant-point of Comet I. 1870, for which he, in accordance with the Reports of the British Association, gave the position $\alpha = 43\frac{1}{2}^\circ$, $\delta = +53^\circ$. This position is not correct, the true radiant being $27^\circ\cdot9$ $+48^\circ\cdot4$, as I showed in the 'Astr. Nachr.' Vol. lxxxii. No. 1963 (Nov. 1873). There are several radiants of shooting-stars near this place, especially one by Julius Schmidt, 31° +55° (Aug. 3–12), which probably is identical with Mr. Denning's shower from 32° +53°.

I am Sir, yours faithfully,
J. L. E. Dreyer.

The Observatory, Dunsink,
Co. Dublin, 1879, July 5.

The Variable Star R *Scuti.*

Sir,—

Seeing, in the 'Annuaire' for 1879, that the minimum of this star was on June 25th, I looked at it on that night, and was considerably surprised to see that it was about 5–6 mag. of Heis's scale, and plainly visible to the naked eye, in spite of the bright twilight and a somewhat hazy sky. This observation I confirmed on one or two nights following. The minimum given in the 'Annuaire' is 7·3 mag. It is noteworthy that the magnitude of this star at minimum seems to be increasing. Heis, in 1868, gave its minimum 9 mag. 'L'Annuaire' for the present year gives its minimum 7·3 mag.; and, unless it occurred before June 25th, its minimum was really about 5½ mag. In Arago's 'Astronomy,' I see that its minimum is given as "9 to 6 mag."

I am, yours truly,
Vaughan Cornish.

Debenham, Suffolk.

NOTES.

Instruments for Travellers*.—In a lecture to the French Geographical Society, M. D'Abbadie has discussed the relative merits of the sextant and theodolite as applied to the determination of geographical positions in unexplored countries. He can speak on this subject from much practical experience derived from extensive travels in the wild districts of Ethiopia. M. D'Abbadie insists strongly on the disadvantages of the sextant, the use of which in expeditions on land by the English he regards as a mistake, though it was only natural for them to take with them an instrument with which their voyages by sea had rendered them familiar. The circumstance that an angle of more than 140° cannot be measured with it, makes it often impossible to observe the meridian altitude of the Sun in the tropics, whilst observations by night require artificial light, which a traveller in wild regions often finds it difficult to procure. Further, in constructing a map, azimuths are required; and these are so difficult to obtain with a sextant, that they are rarely taken, especially as they involve troublesome calculations. In fact M. D'Abbadie considers that the sextant is only useful for determining the latitudes and longitudes of isolated points; and for this purpose he recommends the pocket sextant, 3 inches in diameter, with which the latitude can be found within 1500 yards by a single altitude and the time within 2 seconds, whilst distances of 3 to 4 miles may be determined by means of a short base 50 to 100 yards long. In M. D'Abbadie's opinion, however, the pocket sextant has, even in this use of the instrument, no real advantage over the theodolite, except in requiring less preparation. It is accordingly the theodolite which he recommends as the most useful instrument for travellers; and he describes a pattern which he has found very serviceable. In this instrument a prism is placed in front of the object-glass (which is more powerful than in most theodolites of its size), and thus the telescope is always horizontal, a convenient form of construction. The whole weighs 7¾ lbs., without its case. The tripod stand is formed of three rattan canes, which are screwed on to a wooden triangle, the whole being made as roughly as may be, so as not to excite the curiosity of the natives, a point on which M. D'Abbadie lays great stress in all arrangements for exploring expeditions. In determining azimuths with this instrument, M. D'Abbadie recommends the well-known method of equal altitudes.

Photographs of the Spectrum in Colours.—Capt. Abney has succeeded in obtaining pictures of the solar spectrum in its natural colours, both on silver plates and on compounds of silver held *in situ* by collodion. The photographs are produced by oxidation of silver compounds when placed in the spectrum, the colouring matter being due apparently to a mixture of two different sizes of molecules of the same chemical composition, one of which absorbs at the blue end and the other at the red end of the spectrum; and the sizes of these molecules are unalterable whilst exposed to the same wave-lengths as those by which they were produced. Capt. Abney believes that the colours may be preserved unchanged when exposed to ordinary daylight. He has thus succeeded in explaining the action by which Becquerel some years ago obtained photographs in colours, as produced by oxidation and not due to interference. Becquerel's photographs were fugitive, but Capt. Abney would appear to have succeeded in "fixing" his. An exposure of two minutes with a wide slit was amply sufficient to produce the pictures of the spectrum.

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* "Instruments à employer en voyage," par M. Antoine D'Abbadie. Bulletin de la Société de Géographie, Paris.

THE PULKOWA OBSERVATIONS.—As numerous inquiries have been addressed to M. Struve relative to the appearance of vol. ix. of the 'Observations de Poulkova' before vol. viii., he wishes us to explain how this has occurred. Vol. viii. is designed for the Catalogue to be deduced from the Meridian Observations inserted in vols. vi. and vii. The reductions for this Catalogue, though considerably advanced, have been much delayed by the lamented death of Dr. Von Asten, to whom the work had been intrusted, so that it is not likely that the volume will be distributed before 1881. In a similar way vol. x. of the 'Observations' is designed to contain the continuation of the micrometrical measures of double stars; but very little progress has as yet been made in the work, so that this volume will not be published for a couple of years; whilst vol. xi., containing the continuation of the meridian work, is nearly ready and will, M. Struve hopes, be distributed very shortly.

SPECTROSCOPIC OBSERVATIONS OF THE SOLAR ECLIPSE*.—Prof. G. F. Barker, who formed one of Dr. H. Draper's party at Rawlins, Wyoming, has published the results of his observations on the spectrum of the corona. He used a Merz spectroscope provided with two direct-vision compound prisms, and with this he saw a continuous spectrum with Fraunhofer lines, but no bright lines whatever, though he looked for them very carefully.

These results agree with those given by Dr. H. Draper's spectroscope. With regard to the Fraunhofer lines, Prof. Barker remarked that they corresponded in length to the image of the corona on the slit, being strong and sharp at the base of the corona and fading gradually out in the upper part. Prof. Barker concludes that the absence of bright lines proves the absence of any considerable mass of incandescent gas, whilst the presence of Fraunhofer lines shows the presence of reflected sunlight, and goes to establish the theory that meteoric matter raining down on the solar surface is the essential cause of the coronal phenomena. The brightness of the continuous spectrum as compared with the intensity of the Fraunhofer lines indicates that part of the light is due to the incandescence of these meteoric masses. These results agree with those deduced by other observers of this eclipse.

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* Amer. Journ. 1879, Feb. and April.

Mr. W. H. Pulsifer, who used in the eclipse a ten-prism Browning spectroscope mounted on a four-inch refractor, observed the reversal of the Fraunhofer lines at commencement of totality with a tangential slit, and has thence deduced the thickness of Young's reversing layer, which he finds to be 524 miles. The reversed or bright lines extended over one third of the breadth of the spectrum; and the length of the tangential slit being 0·08 in., whilst the diameter of the Sun's image was 0·54 in., it was easy to find the thickness of the stratum.

Observatory on Mt. Etna*.—Prof. Tacchini proposes to commence this year the new physical observatory on the site of the "Casa Inglese," at a height of 9600 feet above the sea. It will have an equatoreal of 35 centimetres (14 inches) aperture, and the building will be constructed chiefly of lava, with a conical rotary roof of iron for the equatoreal. Prof. Langley has been for some time at a station on Etna 4500 feet above the sea, engaged in obtaining quantitative data as to the degree in which the conditions of vision are improved at elevated stations; and his conclusion is that while, as regards observations of precision, and even work on double stars, the gain is less than might have been expected, too much can hardly be said of the immense advantage of an elevated station for almost every kind of research connected with solar physics. This is especially the case as regards the chromosphere and the corona. Our only hope of materially extending our knowledge of the latter appears to be in the prospect that we may yet be able to see it without an eclipse, if the observer be in an exceptionally transparent atmosphere.

The Zodiacal Light†.—In sending to Prof. Tacchini a beautiful drawing of the zodiacal light as seen at Groningen on 1875, Feb. 22, by Prof. Groneman, P. Serpieri calls attention to the *central light* forming an internal cone with well-marked and distinct outline on the fainter background of diffused light. The same phenomenon was seen by Prof. Tacchini at Muddapur (Bengal) in 1874 on the morning of Dec. 15 and in the evening of Dec. 29. On the latter occasion the zodiacal light was very brilliant and reached almost to the zenith. P. Serpieri remarks that the zodiacal light was generally faint in the second half of 1878, though it had been most splendid in February, and particularly on the 20th of that month, when it was more brilliant than the Milky Way. In the preceding year, 1877, a strange difference was recorded in its appearance at

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* Amer. Journ. 1879, March.

† Mem. Spettrosc. Ital. 1879, March.

Milan and at Urbino. At the former place, Prof. Schiaparelli noted it as "superb with a Gegenschein" on the morning of August 15, whilst at Urbino P. Serpieri registered it at the same instant as "faint and diffused."

Double Stars*.—Mr. Alvan G. Clark discovered at various times, with one or other of the large object-glasses made by him, some interesting and difficult double stars, which have not hitherto been published. There are 14 of these objects, including the companion to Sirius, for the discovery of which Mr. Alvan Clark received the Lalande prize; and Mr. Burnham has now collected the measures of these stars, made chiefly by the Washington observers, by Baron Dembowski, and by himself. One or two of them already appear to be binaries, even from the observations of three or four years.

Swift's Comet.—The following elements of this Comet have been computed by Dr. Küstner under Prof. Winnecke's superintendence, and appear to represent the observations closely:—

Per. Pass. 1879, April 27·336, Berlin M. T.
Long. Per. 48° 38′ 42″·4. Inclination 107°0′7″·5.
Long. Node, 45° 33′ 36″·6. Log. Per. Dist. 9·948935.
Mean Equinox, 1879·0.

Mr. S. C. Chandler, Jun., has also computed a set of elements agreeing closely with the above ('Science Observer,' 1879, July).

It will be seen that the comet has long passed its perihelion, and it is now receding both from the Earth and Sun, so that its brightness has considerably diminished. It may, however, still be observed without difficulty, and may be picked up by help of the following rough ephemeris, which has been formed by Major Tupman by continuing approximately Dr. Küstner's ephemeris:—

Ephemeris for 12ʰ G. M. T.

1879	R.A.	Dec.	1879	R.A.	Dec.
	h m	° ′		h m	° ′
Aug. 2,	15 15·6	+70 57	Aug. 10,	15 21·9	+63 1
4,	17·1	68 57	12,	23·1	61 2
6,	18·6	66 58	14,	24·6	59 3
8,	20·1	65 0	16,	25·1	57 4

This comet is remarkable as having passed, on July 15, almost exactly through the pole of the heavens.

We learn that the large telescopic mirror of 1·20 metre diameter has been received in the Paris Observatory, and will be mounted, although its outer rim, to a depth of 9 centimetres, is faulty. As the slope of that rim is outwards, the defect could not be remedied without polishing the whole surface; and it was thought preferable to use the mirror with its good diameter of one metre.

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* Amer. Journ. 1879, April.

We learn from the 'Madras Times' for 1879, March 22, that Mr. Pogson at Madras kept a close watch on the Sun for the transit of Vulcan, predicted by M. Oppolzer, from soon after daybreak on March 19 (civil reckoning) to 5.30 P.M. Madras time, but without seeing a vestige of any planet.

Mr. George Hunt points out, with reference to Mr. Gledhill's note on the companions to Procyon ('Observatory,' No. 23, Vol. ii. p. 382), that the star discovered to be double by Bird in 1864 was not G but F.

He also suggests that a Dawes eyepiece should be used in observing occultations, to diminish the glare of the Moon.

Three Minor Planets, Nos. 199, 200, and 201, have been discovered by Prof. Peters at Clinton, New York—on July 10, on July 17, and on July 28 respectively. Editor.

Astronomical Memoranda, 1879, *September.*

Sun. Sept. 1, sets $6^h 46^m$, rises $17^h 15^m$; Sept. 30, sets $5^h 40^m$, rises $18^h 2^m$.

Equation of time:—Sun *before* clock, Sept. 1, $0^m 2^s$; Sept. 11, $3^m 22^s$; Sept. 21, $6^m 52^s$; Oct. 1, $10^m 16^s$.

Sidereal Time at Mean Noon:—Sept. 1, $10^h 41^m{\cdot}1$; Sept. 11, $11^h 20^m{\cdot}5$; Sept. 21, $12^h 0^m{\cdot}0$; Oct. 1, $12^h 39^m{\cdot}4$.

Moon. Last Quarter, Sept. 8, $8^h 4^m$; New, Sept. 15, $17^h 57^m$; First Quarter, Sept. 22, $9^h 19^m$; Full, Sept. 29, $21^h 17^m$.

Occultations:—Sept. 1, $12^h 44^m$ 15 Piscium Oc. R. 325°; Sept. 2, $16^h 37^m$ 45 Piscium Oc. R. 289°; Sept. 5, $13^h 8^m$ μ Arietis Oc. R. 341°; Sept. 6, $11^h 20^m$ 66 Arietis Oc. R. 268°, $16^h 9^m$ 9 Tauri Oc. R. 332°; Sept. 12, $16^h 19^m$ o' Cancri Oc. R. 225°; Sept. 20, $7^h 20^m$ A^2 Scorpii Oc. D. 86°; Sept. 25, $11^h 35^m$ B.A.C. 7263 Oc. D. 155°; Sept. 26, $10^h 40^m$ λ Capric. Oc. D. 117°. The angles at disappearance (D.) and reappearance (R.) are reckoned from the *apparent* N. point towards the right hand round the Moon's circumference as seen in an inverting telescope.

Mercury is a morning star, at greatest elongation W. (17° 57′) on Sept. 9, when he rises in the E.N.E. $1^h 43^m$ before the Sun.

Venus is an evening star, setting soon after the Sun, till Sept. 23, when she is in inferior conjunction.

Mars is in Taurus. His diameter is $15''{\cdot}6$ on Sept. 1 and $19''{\cdot}5$ on Sept. 30.

Sept. 1, R.A. $3^h 16^m{\cdot}5$, Dec. 15° 41′ N., tr. $16^h 34^m$, rises $9^h 10^m$.
Sept. 30, 3 46 ·8, 18 3 N., 15 9 7 27

Jupiter is an evening star retrograding through Aquarius. His diameter is $46''{\cdot}4$ on Sept. 1, and $45''{\cdot}0$ on Sept. 30.

Sept. 1, R.A. $22^h 39^m{\cdot}3$, Dec. 10° 1′ S., tr. $11^h 56^m$, sets $17^h 8^m$.
Sept. 30, 22 26 ·4, 11 16 S., 9 49, 14 54

Jupiter's Satellites:—Sept. 4, $15^h 7^m$ i. Oc. D.; Sept. 5, $6^h 55^m$

iv. Ec. R., $12^h\ 19^m$ i. Tr. I., $13^h\ 36^m$ ii. Tr. I., $14^h\ 38^m$ i. Tr. E.; Sept. 6, $9^h\ 34^m$ i. Oc. D., $12^h\ 0^m$ i. Ec. R.; Sept. 7, $8^h\ 2^m$ ii. Oc. D.; $9^h\ 4^m$ i. Tr. E., $11^h\ 6^m$ iii. Tr. I., $11^h\ 13^m$ ii. Ec. R., $14^h\ 36^m$ iii. Tr. E.; Sept. 12, $14^h\ 3^m$ i. Tr. I.; Sept. 13, $10^h\ 18^m$ iv. Tr. I., $11^h\ 18^m$ i. Oc. D., $13^h\ 56^m$ i. Ec. R., $14^h\ 28^m$ iv. Tr. E.; Sept. 14, $8^h\ 29^m$ i. Tr. I., $10^h\ 15^m$ ii. Oc. D., $10^h\ 47^m$ i. Tr. E., $13^h\ 48^m$ ii. Ec. R., $14^h\ 22^m$ iii. Tr. I.; Sept. 15, $8^h\ 24^m$ i. Ec. R.; Sept. 16, $7^h\ 53^m$ ii. Tr. E.; Sept. 18, $9^h\ 14^m$ iii. Ec. R.; Sept. 20, $13^h\ 3^m$ i. Oc. D.; Sept. 21, $10^h\ 13^m$ i. Tr. I., $12^h\ 30^m$ ii. Oc. D., $12^h\ 32^m$ i. Tr. E.; Sept. 22, $7^h\ 29^m$ i. Oc. D., $10^h\ 20^m$ i. Ec. R.; Sept. 23, $6^h\ 58^m$ i. Tr. E., $7^h\ 19^m$ ii. Tr. I., $10^h\ 11^m$ ii. Tr. E.; Sept. 25, $7^h\ 15^m$ iii. Oc. D., $13^h\ 15^m$ iii. Ec. R.; Sept. 28, $11^h\ 57^m$ i. Tr. I.; Sept. 29, $9^h\ 15^m$ i. Oc. D., $12^h\ 15^m$ i. Ec. R.; Sept. 30, $7^h\ 40^m$ iv. Sh. I., $8^h\ 44^m$ i. Tr. E., $9^h\ 39^m$ ii. Tr. I., $12^h\ 1^m$ iv. Sh. E., $12^h\ 31^m$ ii. Tr. E.

Saturn is in Pisces, retrograding slightly.

Sept. 1,	R.A. $0^h\ 58^m{\cdot}5$,	Dec. 3° 21′ N.,	tr. $14^h\ 15^m$,	rises $7^h\ 55^m$.
Sept. 30,	0 51 ·2,	2 31 N.,	12 14	5 57

	Outer Ring.		Inner Ring.		Ball.
	Maj. Axis.	Min. Axis.	Maj. Axis.	Min. Axis.	Diam.
Aug. 28...	43″·39	7″·53	28″·86	5″·01	17″·4
Sept. 17...	44″·24	7″·24	29″·42	4″·81	17″·8

The south side of the rings is visible, the elevation of the Earth above their plane being 10° 0′ S. and 9° 25′ S. on Aug. 28 and Sept. 17 respectively, and of the Sun 8° 29′ S. and 8° 47′ S.

EDITOR.

POSTSCRIPT.

Eclipse of the Sun, 1879, *July* 18.

We have received the following from Señor Arcimis:—

Sir,—

I observed this morning the partial eclipse of the Sun announced in the Nautical Almanac.

I made use of my reflecting equatoreal telescope of 8½ inches, but I reduced its aperture, by means of the stops, to 7 inches; the eyepiece was a prismatic one by Browning.

The sky was clear, and the first contact took place at $18^h\ 18^m\ 15^s{\cdot}5$ Cadiz mean time; the images were very steady.

The last contact took place at $19^h\ 57^m\ 48^s{\cdot}2$.

The error of the chronometer was accurately known, and the above times are very exact.

The Nautical Almanac of San Fernando gives for the first contact $18^h\ 16^m\ 51^s{\cdot}7$, and for the last $19^h\ 59^m\ 22^s{\cdot}8$.

In making due allowance for the difference of longitude between San Fernando and my observatory, which is $21^s{\cdot}6$, there still remains a considerable difference between the computed and the observed time, that is not to be attributed solely to parallax.

Notwithstanding the slight value of this observation, I send it to you exclusively for the purpose of establishing the fact.

I am, Sir, yours very faithfully,

Cadiz, 1879, July 19. AUGUSTO T. ARCIMIS.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

No. 29. SEPTEMBER 1. 1879.

Fireball Epochs.

The tabulation of many of the recorded instances of fireballs and large meteors proves that certain dates of the year give an unusual abundance of such phenomena. Arranging them into a list of dates in which is exhibited the number seen on every night of the year, I have endeavoured to find the most remarkable fireball epochs; and it is evident that such an investigation is now quite feasible, considering the vast number of observations that have accumulated in recent years. Mr. R. P. Greg has already discussed many such records, and classified them according to character and date. In the British Association Report for 1860 he gives an extensive catalogue of aerolites and bolides (A.D. 2 to A.D. 1860), and in the same Report, and that for 1867, he adds supplementary catalogues. On page 114 of the former volume, a list is given of the number observed on each day of the month during the period from 1492 to 1860. Including these exhaustive compilations with the large additions recently made to this department, so far as I have been able to collect them, it is found that the number of such records amounts to more than 3600, distributed over the year as follows:—

January	239	May	163	September	273
February	174	June	172	October	292
March	186	July	287	November	551
April	234	August	775	December	289

These numbers are derived both from systematic observation by scientific observers and from numerous isolated notices, and refer to meteors equal to or exceeding the lustre of Jupiter. The recent observations are mostly of the former class; and being made chiefly during the periodical shower dates, the effect has been to greatly increase the number of large meteors seen at these special epochs. The older records consist almost wholly of irregular (or accidental) observations, and have the effect of scattering the figures more evenly over the year. The excessive figures for August and

November, as shown in the above list, are due in a great measure to the *Perseids* (Aug. 7–12) and *Leonids* (Nov. 11–15), which have supplied many fine meteors at their annual returns. During the first half of the year the aggregate of such observations is far less than during the last half. The minimum is from January 16 to July 19. The other six months include the maximum, which has fully two-thirds of the total number of the observations. Of short periods, the ones of greatest scarcity are from May 5 to 30, and from March 11 to April 9, and the times of greatest abundance are from July 15 to August 16, and from November 1 to 17. The fact that more fireballs have been recorded (in the proportion of 2 to 1) during the last half of the year is partly explained by the observers having paid more attention to this period than to the first half of the year, which has been much neglected.

In drawing out the list, special care was taken to avoid repetition, for in many instances the same fireballs are to be found registered in several different catalogues.

The principal dates of the year when large meteors have been observed in the greatest abundance are as below:—

January 2, 21, 31.
February 3, 7, 10.
March 1, 2, 4.
April 11–12, 19–20.
May 2, 4, 15, 31.
June 6–7, 12, 29–30.
July 11, 20–21, 25–30.
August 3, 5, 7–13, 15, 19–22.
September 1–2, 6–7, 11–13, 25.
October 13, 15, 17–18, 22, 24, 29.
November 1–2, 4, 6–9, 11–15, 19, 27.
December 8–9, 11–12, 21.

The most remarkable of these epochs are underlined. A similar list to this, based on all the observations collected by Mr. Greg up to 1863, was printed in the British Association Report for that year as follows:—Jan. 28, Feb. 10, April 10, July 18, 20, 29, Aug. 4, 12, October 13, 18, 24, Nov. 13, 15, 21, and December 11 and 21, which confirms the dates now ascertained as above in the most satisfactory manner. Prof. Herschel had as early as 1866 pointed out the "epochs of the 9th–11th of February and 19th–21st of November as dates deserving special attention, with a view of determining the directions of detonating meteors, which, by their frequent return within very narrow limits of time about those dates, show that aërolitic meteors, like the star-showers of Aug. and Nov., revolve in fixed orbits round the Sun" *.

I have endeavoured in several cases to obtain the chief radiant-points for some of these periods, but the observations are not sufficiently exact and plentiful to enable this end to be effectively attained. The path-directions have not been definitely recorded, except amongst the recent observations; and it is utterly impossible to get any thing like accurate radiants from the descriptions generally given, as from S.W. to E. or N.E. to W. Shower-centres determined from such materials are always open to considerable error; and even in cases where the directions and positions of fireballs are clearly stated, some doubt must naturally be attached to the radiants deduced from the intersection of the tracks, which may result from pure accident, in cases where we have very few such observations to rely upon. Apart from this, the large number of meteor-systems visible every night, each possibly giving occasional fireballs, is the source of another difficulty. In fact, though many fine meteors coincide as to date, they show considerable differences in their directions and appearances. Doubly observed bright meteors, of which the number is rapidly increasing, are, however, of extreme value, as giving radiants generally within a few degrees of the truth; and it is to be hoped that observers will continue to give their attention to this special work. During the last ten nights of July, and during the first halves of Aug., Sept., Nov., and Dec., large meteors are abundant, and should always be looked for, with a view of obtaining duplicate observations.

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* Monthly Notices R. A. S. Vol. xxvi. p. 212. "Path of a Detonating Meteor," by A. S. Herschel, Esq.

A brief monthly summary of results, derived from the tabulation of the large meteors already recorded, may here have some features of interest.

The special shower in Quadrans, with a maximum activity on January 2, has supplied most of the fireballs visible at that epoch.

In February there are three dates during the first half of the month on which they should be looked for. The chief radiants have not been determined. The 10th was pointed out by Mr. Greg as a notable date, and is fully confirmed by more recent observations. The great meteor of February 13, 1871, had a radiant near α Hydræ.

During the first four days of March, fireballs have been very numerous, especially on the 1st, 2nd, and 4th; and the latter date is remarkable, from the fact that they were nearly all recorded in very recent years, and that no indication of its intensity is found by the earlier records. A good radiant-point cannot be derived from the projection of the apparent paths of the bright meteors which have been carefully observed at this epoch, for a singular variety is shown in their direction, and this proves them to have belonged to many different showers.

April gives a remarkable period on 11th–12th. The number recorded on each of these nights, as compared with those of ordinary nights of the month, is in the proportion of 7 to 2; for the average number in the table is 5 per night, whereas the figures for the 11th and 12th are 17 and 18 respectively. The two preceding nights had also been suspected to give large meteors; for Von Niessl determined a radiant of two fireballs at δ Cassiopeiæ (April 10, 1874, and April 9, 1876). With the object of finding the major showers for this special period of April 9–12 (though the former might appear to be one of decided scarcity as compared with the 11th–12th), I recently projected the registered tracks of more than 700 shooting-stars in the catalogues of Italian observers; and the chief positions deduced were at 249° +51°, 212° +65°, and 184° +59°, from about 45 meteors each *. The paths of the brilliant meteors recorded at this epoch indicate several distinct radiants, two of them in the Lynx at 106° +46° and 121° +41°, and several others in the fore part of Ursa Major. There is another well-defined period on the 19th–20th, when many bright *Lyrids* have been recorded during the numerous watches which are annually made for their appearance. The last two nights of this month are also rather favourable; and it should be remembered that Heis, on April 29, 1847, and April 27, 1848, found meteors exceptionally abundant.

May apparently gives a smaller number of fireballs than any other month. There are, however, four dates worthy of note, the first of which, the 2nd, coincides with that of a shower of brilliant meteors observed by Major Tupman in 1870. On May 2, 1848, Heis observed many falling stars with bright streaks.

In June, the 6th–7th, 12th, and 29th–30th are the most conspicuous dates. A radiant near Antares is indicated for the former period at 247° −25°; and the fireballs of June 17, 1873, and July 25, 1876, had radiants at 248° −20° and 258° —24°, supporting this position and indicating a long-continued shower near the bright star referred to.

July appears to be a very favourable month for such observations, especially during the last 7 nights. The very decided frequency of fireballs at this period, as shown by existing records, arises partly from the fact that a large number of watches have been sustained just at this period, when the weather is often clear and observers are attracted to the work by the closely ensuing return of the *Perseids*. Apart from this, however, there is no doubt that this epoch is exceptionally one of large meteors; and they are not early *Perseids*, as may be inferred, for that prominent shower certainly does not come into active play until August 5 or 6, and very few of its members are seen in July. They belong to a variety of different radiants, of which the chief is in Aquila at 303° +12°. This is the same as Mr. Greg's *Aquilids*, at 301°·5 +8°·5, and No. 79 of his 1876 catalogue. Other radiants are fairly well though less actively defined at 324° −12°, 338° −11°, 8° +34°, 35° +52°, and 322° +11°. The three former were well seen by Major Tupman in 1870, July 27–28, and severally confirmed by the writer at the end of that month in 1878. The shower near χ Persei (35° +52°) was also seen on the latter occasion as a very conspicuous display of swift streak-leaving meteors, with max. on July 30–Aug. 1 †. An early display of this shower may have been observed by Heis on July 27–28, 1848, for he records that on that night there was a remarkably great number of falling stars, the most with streaks. Heis also records, on July 29, 1854, that there were many meteors, and in 1856–7 they were also numerous on July 27–30.

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* Month. Not. R.A.S., May 1879, p. 402. "The Radiant-points of April 9–12."

† 'Observatory,' Sept. 1878, pp. 163–165. "Meteor Notes."

During the first half of August (max. 10th) a vast number of observations have been made, and many large *Perseids* seen between the 7th–12th. On the 3rd and 5th an unusual proportion of fireballs have made their appearance. The shower of *Perseids* apparently subsides into inactivity on the 4th; but on the ensuing night there is a revival, and on the 19th–22nd another period of frequency, but this is too late for the *Perseids*. The paths of such fireballs as have been accurately placed on record give a double shower in Cepheus to Draco, from the points 339° +66°, 299° +66°; and the positions may really represent one shower only (its diffusion being caused by errors of observation), with a centre at about 319° +67°. Corder saw 11 meteors from a point at 307° +65° on August 17, 1877.

September 1–2 is a very well-defined epoch of fireballs. The major shower is at 306° +54° *, and close to the radiant of the large meteor of Sept. 3, 1875, at 311° +52°. The 6th–7th is also equally decided; and from those dates until the 13th the number of such observations is excessive. The chief radiants appear to be at 332° +31° and 63° +35°. The former is Heis's μ 2 at 328° +35°, Sept. The latter was seen and described by the writer as a rich shower on Sept. 7–16, 1877, at 61° +36° (15 meteors). Two large fireballs seen in September 1875 gave radiants at 344° +14° (7th) and 348° ±0° (14th). On the 25th there is another period of frequency, and the projected tracks give two radiants at 351° +44° and 335° +37°, the latter being clearly a continuation of that at 332° +31° for the 6th–13th. The great meteor of Sept. 24, 1876, had a radiant at 285° +35°, and Heis observed a stationary fireball at 292° +68° (δ Draconis) in 1851, Sept. 26.

October gives several dates of more than ordinary interest. On the 15th, 17th–18th, and 22nd the *Orionids* have probably supplied the majority of the large meteors that have been visible; but this shower is by no means remarkable for the brilliancy of its members.

November is a month of special importance to the observer of fireballs. On the first two nights many have been seen, and again on the 4th, 6th–9th, 11th–15th (*Leonids*), 19th and 27th (*Andromedes*). The former period gives indication of a periodical shower of unusual intensity. Heis saw many meteors on Nov. 2, 3, and 5, 1844, and Nov. 1, 1847. On Nov. 1, 1849, there was a fine display of shooting-stars (Silliman's 'Journal'). On Nov. 2, 1859, between 7 and 8 P.M. there were many large meteors, chiefly in N.E. (B. A. Report, 1860, p. 5). On Nov. 3, 1872, a shower of exceedingly brilliant meteors were observed falling (B. A. Report, 1873, p. 352) †. I have carefully compared the directions of the large meteors recorded on the 1st–4th, and find a very sharply defined and certain shower of *Arietids* from a point at 43° +22°. This stream appears to be as rich as that of the *Taurids* I., and gives meteors of remarkable splendour. It was detected by the writer on the nights of Oct. 29–Nov. 13, 1877, when 31 of its meteors were seen, and the max. determined for Oct. 31–Nov. 1. At about this epoch, and again during the 6th–9th, the *Taurids* I. form a notable shower. A large fireball seen on Nov. 8, 1876, in the evening twilight, had a radiant at 53° +20° (near η Tauri); and on the very same night the writer at Bristol recorded 8 shooting-stars, including several brilliant ones, from a radiant at 58° +16°. Mr. Backhouse at Sunderland saw 12 meteors from a well-defined centre at 54° +16°, on Nov. 6, 1869, and Major Tupman traced a shower at 56° +24° on the nights of Nov. 1–3, 1872. Thus in the early part of November these *Taurids* and *Arietids* form important displays. The *Leonids* (max. 13th) have furnished nearly all the brilliant meteors recorded during the 11th–15th. The 19th is also a very conspicuous date for fireballs, and there is no doubt that the *Taurids* are again the chief contributors. A star-shower of more than usual intensity was seen by the writer on the night of Nov. 19, 1876, with a radiant at 62° +22°; and the large meteors of Nov. 19, 1861, Nov. 27, 1862, Nov. 21, 1865, Nov. 23, 1877, probably belonged to the same system of *Taurids*, which in the evenings of this month often gives a bright and numerous display of shooting-stars. Another special date is the 27th, when the *Andromedes* (max. 1872) are occasionally visible in great abundance.

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* See 'Observatory,' Dec. 1878, p. 271.

† On Nov. 1, 1870, Mr. Wood saw several meteors, one of them as bright as Sirius, from Mr. Greg's radiant R_3 in Musca, and on Nov. 3, 1872, some bright meteors were observed having a radiant near β Persei (45° +35°). These observations all refer, no doubt, to one and the same shower in Musca.

December 8th–9th, 11th–12th, and 21st are the most prominent dates of that month. The two former epochs agree with the Geminids; and there is no doubt that the many fine meteors observed at this season have belonged to that active meteor-shower. On Dec. 9, 1877, Mr. Corder saw a shower of bright meteors from a point at 108° +28°, which, however, he regards as distinct to the true *Geminids*. The 21st of this month is (like April 11–12 and Nov. 19) a very decided date of fireballs, for more than three times as many have been seen on this date as on the other ordinary nights of the month. One of the chief radiants is probably that of the Taurids II. (80° +23°), which in 1878 supplied several fine meteors on that night, though it is also in striking activity earlier in the month. We require many more observations on this and many other nights of the year; for they are likely to yield important materials from which may be derived the radiant-points of these bright meteor displays; and it is hardly necessary to add that in all cases the directions and positions of the apparent paths should be recorded with the utmost accuracy possible. W. F. Denning.

The Visibility of Meteors.

In the spring of 1875 I undertook a series of meteoric observations with the intention of continuing the same for a period of ten years. My object at the time was merely to determine the average number of meteors visible during a certain hour throughout the year; and I simply recorded the number observed each clear evening in the absence of the moonlight, during the hour selected, their velocities (divided into three classes), and their magnitudes compared with the stars or planets. My time being somewhat limited at the commencement of the series, I was only enabled to devote half an hour to the work each evening; but becoming more interested in the subject as the work progressed, and particularly in the more important work of mapping the meteor-tracks, I was able at the expiration of 20 months to give more time to the subject, and for the past two years my watches have been of much longer duration. Wishing to select a convenient hour during which observations could be taken the year round, I found that an earlier hour than 9 P.M. was impracticable, so that I selected the hour between 9 and 10 P.M. for the series. The observations were invariably taken in the east, the centre of observation being at an altitude of about 40°, and the view was limited to about $\frac{1}{8}$ of the visible heavens, this being considered about as large a space as could be conveniently covered. During the first 20 months, the observations were taken from 9 to 9.30 P.M., and the averages multiplied by 2 give the horary number, and during the past two years, from 9 to 10 P.M., in half-hourly series, for the purpose of seeing if the results would differ materially from those deduced during the preceding 20 months; but no appreciable difference was shown, both determinations giving almost exactly the same results. I have summarized the results in the Table given, p. 134, with the calculated horary rate of frequency for the several months.

The averages for the first and last six months of the year are 3·9 and 5·9 respectively.

This corroborates the results deduced by other observers. Meteors were notably scarce during the greater part of the year 1877. Mr. Denning remarked this ('Obs.' Vol. i. No. 4) for the months of Jan. and Feb. 1877. Of the 923 meteors recorded, the following were their estimated brightness:—

>1st mag. *.	=1st mag. *.	=2nd mag. *.	=3rd mag. *.	=4th mag. * and fainter.
19	101	171	225	407

The velocities were divided into three classes—slow, medium, and rapid; and every meteor observed was assigned to one of the three classes. The results, when deduced, exhibit the following curious rule:—That, as the magnitude increases the velocity decreases and *vice versâ*; or, in other words, the fainter meteors, as a rule, move with greater velocity than the brighter meteors. This

Month.	1875.			1876.			1877.			1878.			Summary.		
	Duration of watch.	No. of Meteors seen.	Horary No.	Duration of watch.	No. of Meteors seen.	Horary No.	Duration of watch.	No. of Meteors seen.	Horary No.	Duration of watch.	No. of Meteors seen.	Horary No.	Duration of watch.	No. of Meteors seen.	Horary No.
	Hours.			Hours.			Hours.			Hours.			Hours.		
January	....	....	....	3·0	15	5·0	6	16	2·7	8	19	2·4	17·0	50	3·0
February	....	....	....	2·5	11	4·4	2	7	3·5	3	12	4·0	7·5	30	4·0
March........	....	....	....	2·5	8	3·2	7	14	2·0	4	11	2·8	13·5	33	2·5
April	....	....	....	2·5	12	4·8	6	25	4·2	1	4	4·0	9·5	41	4·3
May	2·0	10	5·0	4·5	27	6·0	7	31	4·4	2	4	2·0	15·5	72	4·7
June	4·5	22	4·9	3·0	18	6·0	4	10	2·5	3	16	5·3	14·5	66	4·6
July	6·5	41	6·3	5·0	33	6·6	3	8	2·3	6	35	5·8	20·5	117	5·7
August	6·5	38	5·8	3·0	19	6·3	4	28	7·0	8	54	6·8	21·5	139	6·5
September	6·0	29	4·8	2·5	16	6·4	7	43	6·1	3	21	7·0	18·5	109	5·9
October	3·0	22	7·3	4·0	26	6·5	5	29	6·0	4	32	8·0	16·0	109	6·8
November	4·0	25	6·3	2·5	17	6·8	5	25	5·0	3	22	7·3	14·5	89	6·1
December . ..	3·0	16	5·3	2·5	14	5·6	9	29	3·2	2	9	4·5	16·5	68	4·1
Total	35·5	203	5·7	37·5	216	5·7	65	265	4·1	47	239	5·0	185·0	923	5·0

is shown in the following Table, which exhibits the percentage of meteors belonging to each of the three classes, slow, medium, and rapid, and represented by the figures 1, 2, and 3 respectively:—

	>1st mag. *.			=1st mag. *.			=2nd mag. *.		
Velocities......	1.	2.	3.	1.	2.	3.	1.	2.	3.
Per cent...	42·2	36·8	21·0	18·8	38·7	42·5	11·7	25·2	63·1

	= 3rd mag. *.			=4th mag. * and fainter.		
Velocities......	1.	2.	3.	1.	2.	3.
Per cent...	6·7	27·1	66·2	1·9	14·1	84·0

The average length of path of 540 meteors mapped was 9°·8, which appears to be somewhat less than the average of other observers. The most frequent lengths were between 5° and 10°, and the longest path recorded was 72°. EDWIN F. SAWYER.

Cambridgeport, Mass., U.S.A.,
1879, June 4.

Observations or supposed Observations of the Transits of Intra-Mercurial Planets or other Bodies across the Sun's Disk.

IT may not be uninteresting to readers of the 'Observatory' to have recorded in these pages the following list of observations of the above class quoted from a Lecture recently delivered at Gresham College. I think it may be well to give as complete a list as I can of all such observations as have been recorded since 1761. It is a list first brought together by Wolf in the year 1859*. It was shortly afterwards enlarged by Mr. Carrington†, so well known for his elaborate series of sun-spot observations. It has since been increased by some additional observations which are mentioned by Le Verrier‡, and some of which are also brought together in Webb's 'Celestial Objects.'

I omit from the original lists two or three records of a very doubtful character, but I include those numbered 3, 4, and 18, as well authenticated observations of apparent transits, either of comets or of bodies near to the earth, although certainly not of intra-Mercurial planets.

(1) 1761, June 6. Scheuten, at Crefeld, saw during the Transit of Venus for three hours a spot as round, black, and distinct as Venus, but about one fourth of its size, traversing the Sun. He imagined it to be a satellite of Venus.

(2) 1762, end of Feb. Staudacher, at Nüremburg, saw a black round spot on the Sun, which he missed the next day, and of which he said, "Perhaps this may be a new planet."

(3) 1762, Nov. 19. Lichtenberg saw, with the naked eye, a

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* Mittheilungen über die Sonnenflecken, part 10.
† R. A. S. Monthly Notices, Vol. xx. pp. 101 and 191.
‡ Comptes Rendus, Vol. lxxxiii.

great round spot of about one twelfth the diameter of the Sun, traverse a chord of 70° in 3 hours.

(4) 1764, between 1st and 5th of May. Hoffmann, near to Gotha, saw, with the naked eye, a large round spot, of about one fifteenth the diameter of the Sun, traverse it slowly from north to south.

(5) 1777, June 17. Messier saw a number of little bodies very rapidly cross the Sun in parallel directions. (These, Messier says, may have been hailstones or seeds in the air; but were more probably small meteorites.)

(6) 1798, Jan. 18. D'Angos, at Tarbes, saw a slightly elliptical, sharply defined spot, about halfway between the centre and edge of the Sun, which passed off about 25 minutes afterwards. A very careful account of the observation may be found in the 'Jahrbuch' for Berlin, 1804, p. 185, or in 'Comptes Rendus,' Vol. lxxxiii. p. 586.

(7) 1802, Oct. 10. Fritsch, at Magdeburg, saw a spot moving 2 minutes of arc in 3 minutes of time, and not seen after a cloudy interval of 4 hours.

(On 1800, March 20, and 1802, Feb. 7, he also states that he saw spots having rapid motions of their own.)

(8) 1818, Jan. 6. Mr. Capel Lofft and Mr. Acton, at Ipswich, saw a small, uniformly opaque, subelliptical spot, moving for some hours more rapidly over the Sun than Venus in transit. Before sunset it disappeared and certainly seemed to be either of a cometary or planetary character.

This is a very well-attested instance. Mr. Lofft's letter, dated 1818, Jan. 10, is to be found in the 'Monthly Magazine' (not, as generally stated, of that date, but) of 1818, March 1. It is quoted in 'R. A. S. Monthly Notices,' Vol. xx. p. 192.

(9) 1819, Oct. 9. Stark, Canon of Augsburg, saw a well-defined round spot, about the size of Mercury, not to be seen the same evening.

(10) 1820, Feb. 12. A circular well-defined spot, with a circular atmosphere and orange-gold tint, not seen again the same evening, is recorded to have been seen by *two* independent observers, viz., Stark (already referred to) and Steinhübel. It crossed the Sun in about 5 hours (see the 'Meteorologisches Jahrbuch' and the correspondence of Olbers with Bessel, Vol. ii. p. 213).

(11) 1826, July 31. Stark ('Met. Jahrbuch') at 4.45 P.M. saw a round black spot on the north-east border of the Sun, not visible the preceding or following day.

In 1834, 1836, 1837, Pastorff is said several times to have seen little bodies pass over the Sun's disk in comparatively short spaces of time. He in general thought he saw a somewhat larger one accompanied by a much smaller one, but I find no dates given by him.

(12) 1837, July 12. A very small, perfectly round spot without

trace of penumbra seen, in the short space of 6 hours, to cross a considerable portion of the Sun's disk, by De Vico at Rome*.

(13) 1839, Oct. 2. Decuppis, astronomical pupil at the Roman College, saw a perfectly round and defined spot, moving at such a rate that it would cross the Sun in about 6 hours. ('Comptes Rendus,' 1839, p. 809.)

(14) 1847, the latter part of June or early in July. Scott and Wray saw a spot like Mercury in transit.

(15) 1847, Oct. 11. Schmidt at Bonn (Schmidt's 'Resultate,' p. 26), saw a small black spot pass rapidly over the Sun, which was neither a bird nor an insect crossing before the telescope.

(16) 1849, Oct. 14. Schmidt ('Resultate,' p. 31; see also p. 32). A similar observation.

(17) 1849, March 12. Lowe and Sidebotham watched for half an hour a small round black spot traversing the Sun.

(18) 1855, June 11. Ritter and Schmidt, near Naples, watched, just before sunset, with the naked eye, a black body crossing the Sun's disk. ('Comptes Rendus,' Vol. lxxxiii. p. 622.)

(19) 1857, Sept. 12. Ohrt, at Wandsbeck ('Astr. Nachrichten,' No. 1269), saw at 1 P.M. a remarkable round spot near the north border of the Sun, which had disappeared when the Sun was next seen on the 14th. It was a little smaller than Mercury.

(20) 1858, Aug. 1. Circular opaque body moving from east to west, watched from 4^h to 5^h 30^m by Wilson at Manchester. (See Webb's 'Celestial Objects.')

(21) 1859, March 26. Lescarbault's observation.

(22) 1862, March 20. A sharply defined round spot (previously referred to) was watched in progress for 20 minutes by Lummis, of Manchester, and a friend. It was of about half the diameter of Mercury. (See 'Monthly Notices,' Nov. 22, p. 232.)

(23) 1864, Feb. 12. A spot of 8 seconds diameter crossed the Sun at a rate between those of Venus and Mercury. Beswick, New York (doubtful). (See Webb's 'Celestial Objects,' p. 42.)

(24) 1865, May 8. A round black point moving across the Sun was watched for 48 minutes, till its exit, by Coumbary at Constantinople. (See Webb and 'Comptes Rendus,' 1865, part i. p. 1114.)

To the above observations may be added one of an entirely different description referred to in 'Nature,' Vol. xviii. p. 663, which is of special interest in connection with the observations of Messrs. Watson and Swift during the eclipse of July 1878. During the total Eclipse of 1869 four observers at St. Paul's Junction in Iowa (one a lady) saw *with the naked eye* what they termed a little brilliant at a distance about equal to the Moon's diameter from the Sun's limb; and a Mr. Vincent with a small telescope saw a small crescent-shaped object about 3 times as far from the Sun. Dr. Gould, of Cordoba, saw what he supposed to have been the star

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* See the journal 'L'Album' of July 7, 1838, quoted in 'Nature,' Vol. xix. p. 437.

π^2 Cancri, but which, from its brightness, may have been the object seen by the four above-mentioned observers.

Some of the observations in the above list are doubtless much more important and better authenticated than others. Some of them were probably only instances of circularly shaped Sun-spots which lasted, as is sometimes the case, for a very short time. The exact circular shape of a spot on the Sun, or the absence of all penumbra, is no proof that it is a planet, for such spots are seen from time to time which are undoubtedly ordinary Sun-spots. Nor is the disappearance of a spot, after the lapse of a few hours, such a proof. This also happens from time to time with ordinary spots.

E. LEDGER.

On a new Form of Spectrometer, and on the Distribution of the Intensity of Light in the Spectrum.*

I HAVE invented a spectrometer which I think will open a new and interesting field to those who are engaged in spectrum analysis.

The ordinary spectroscope is occupied with the frequency of ether-vibrations or wave-lengths. This which I am about to describe has a different function. It deals with the intensity or brilliancy of light.

It depends on the well-known optical principle that a light becomes invisible when it is in presence of another light about sixty-four times more brilliant.

In some researches, published by me in 1847, on the production of light by heat or the incandescence of bodies, I used this method as a photometer and became sensible of its value. The memoir in which those experiments are related may be found in my recently published 'Scientific Memoirs,' page 23.

Having also published in 1872 a memoir on the distribution of heat in the prismatic spectrum, and shown that the cause of its increasing intensity from the more to the less refrangible regions is due to the compression of the coloured spaces that correspondingly takes place, owing to the action of the prism itself, but having failed to obtain satisfactory measures in the case of the diffraction spectrum, in which such compression or condensation does not occur, I was led to reflect whether better success might not be secured by attempting to measure the relative intensity or distribution of the light.

Admitting what is commonly received as true, that the yellow is the brightest of the coloured spectrum spaces, and that the luminous intensity diminishes from that in both directions, above and below, I supposed that if such a spectrum was brought in presence of an extraneous light, the illuminating power of which could be varied at pleasure, that after the red and the orange on one side and the green, blue, indigo, and violet on the other had been extinguished, the yellow would still remain in the midst of the surrounding illumination. On making the experiment it turned out differently.

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* By J. W. Draper, M.D., President of the Faculty of Science in the University of New York. From the 'American Journal of Science,' 1879, July.

For the sake of clearness of description, I will call this extraneous light, from the function it has to discharge, *the extinguishing light.*

There are many different plans by which the principle above indicated may be carried into practical effect. Several of these I have tried and have found the following a convenient one.

Remove from the common three-tubed spectroscope its scale-tube and place against the aperture into which it was screwed a piece of glass ground on both sides. In front of this arrange an ordinary gaslight, attached to a flexible tube, so that its distance from the ground glass may be varied at pleasure. On looking through the telescope-tube the field of view will be uniformly illuminated, this being the use of the ground glass. The brilliancy of the field depends on the distance of the gaslight, according to the ordinary photometric law.

1st. *Case of the Prismatic or Dispersion Spectrum.*—If the extinguishing light be for the moment put out and in the proper place before the slit tube the *luminous* flame of the Bunsen burner that accompanies the apparatus be arranged, on looking through the telescope a spectrum of that luminous flame will of course be seen. The slit itself should be very narrow so that the spectrum may not be too bright.

Now let the extinguishing flame be placed before the ground glass and a spectrum is seen in the midst of a field of light, the brilliancy of which can be varied at pleasure. If the extinguishing flame be at a suitable distance the whole spectrum may be discerned. As that distance is shortened, first the violet, and then the other more refrangible colours in their descending order disappear, and at length in the steadily increasing effulgence the red alone remains. The yellow never stands out conspicuously as might have been expected.

This is scarcely consistent with the assertion that the yellow is the brightest of the rays. The red is plainly perceptible long after the yellow has gone. There is a greenish tint emitted by gaslight that disappears a little previously to the extinction of the red.

From these observations I think that the luminous intensity of the coloured spaces has a relation to the compression or condensation that the prism is impressing upon them. It may be that, properly considered, the intrinsic intensity of the light is the same for all. In this we must always bear in mind the physiological peculiarities of the eye.

The foregoing statement is perhaps sufficiently explicit to enable any one to verify the facts. I may, however, mention some improvements in the apparatus which experience has led me to adopt.

The intensity of the extinguishing light may be insufficient to obliterate the spectrum, even though the slit be closely narrowed. How then may the intensity of the spectrum be diminished and that of the extinguishing light be simultaneously increased? I accomplished this by depositing on that face of the prism which acts as a reflector an *excessively* thin film of silver. This, though it was transparent to the transmitted rays, increased very greatly by its metallic reflection the extinguishing ones. I could not see any difference between the spectrum of the light that had come through this film and that before the face was silvered, but the reflected light was incomparably more brilliant. The complete obliteration of the entire spectrum presented now no difficulty.

Nothing need be said about collateral contrivances, which would suggest themselves to any one: A strip of wood a metre long and bearing divisions served to keep the extinguishing-lamp in the proper direction as regards the ground glass, and indicated its distance. I may add, however, that satisfactory observations can be made very conveniently by keeping the extinguishing-flame at a constant distance and varying its intensity by opening or closing its stop-cock. This avoids the trouble arising from moving the flame. In one instrument I caused an index attached to the head of the stop-cock to move over a graduated scale, and so ascertained how much it was opened. This, though permitting of pleasant working, had not the exactness of the method of distances.

Such are the results obtained from the prismatic dispersion of gaslight. I completed this part of the investigation by an examination of sunlight. For this purpose I resorted to the foregoing principle, introducing a beam of sunlight reflected from a heliostat through a slit. The spectrum of this was thrown upon a paper screen so placed that by opening or closing an adjacent window-shutter the light of the sky in greater or less quantity could fall upon the paper, and act as an extinguisher. When the shutter was fully opened the spectrum was quite obliterated; and on gradually closing it so as to diminish the extinguishing light, the red region first came into view, the other colours following in the order of their refrangibility, the extreme violet appearing last. On reversing the movement of the shutter the colours disappeared in the reverse order, the red disappearing last.

At the moment when the red was approaching extinction there always existed on its more refrangible side a gleam of greyish-green light. It was in the position of that greenish gleam which appeared as I have described when gaslight was examined. Its colour recalled to my mind the faint greenish-grey light I had seen when a strip of platinum is ignited by a feeble electric current, as described in my memoir of 1847, above referred to.

Subsequently I constructed a camera having two apertures in its front. Through one of them, by a suitable arrangement of a heliostat, slit, direct-vision prism, and convex lens, a solar spectrum was formed on the ground glass. Through the second aperture, which was about an inch square covered with a glass ground on both faces, an extinguishing beam of sunlight passed. This ground glass served to disseminate the extinguishing light uniformly over the spectrum. I could regulate the power of this light by varying the size of the aperture through which it came by means of a slide.

It is needless to give details of the results obtained by this instrument. They were identical with those described in the foregoing paragraphs.

It might be supposed that the irrationality of dispersion of different prisms would influence the results perceptibly. Accordingly I tried prisms of different kinds of glass and other transparent substances, but could not find that this was the case. In all the extinction began in the violet and ended in the red.

Nor did there seem to be any difference when the effect was viewed by different eyes. To persons irrespective of age or the condition of their sight, the extinction took place in the same manner. I had not an opportunity of examination in a case of colour-blindness.

2nd. *Case of the Grating or Diffraction Spectrum.*—If the cause of the increasing intensity of light in the prismatic spectrum from the more to the less refrangible region be the compression exercised by the prism on the coloured spaces, increasing as the refrangibility is less, we ought not to find any such peculiarity in the diffraction spectrum. In this the coloured spaces are arranged uniformly and equably in the order of their wave-lengths. An extinguishing light ought to obliterate them all at the same moment.

Having modified the common spectroscope by taking away its dark box so that the slit-tube and telescope-tube could be set in any required angular position, I put in the place of its prism a glass grating inclined at forty-five degrees to rays coming in through the slit. The ruled side of the grating was presented to the slit. Now when the extinguishing flame was properly placed before the ground glass, the plane side of the grating reflected its light down the telescope-tube. In this, as in the former case, the spectrum was seen in the midst of a field of light, the intensity of which could be varied by varying the distance of the extinguishing flame, or by varying the opening of its stop-cock. This light needs no reinforcement by increasing the reflecting power of the back face of the grating, these spectra being much more feeble than that given by a prism, and the unassisted light being quite able to extinguish them.

As the glass grating I was using gave its two series of spectra of unequal brightness, I selected the most brilliant, and in it used the spectrum of the first order. I saw, not without pleasure, that as the force of the extinguishing illumination increased, all the coloured spaces yielded apparently in an equal manner and disappeared at the same moment. Sometimes, however, there seemed to be a very slight difference in favour of the red. On diminishing the illumination all the colours came into view apparently at the same time. This spectrum gives a better opportunity than the prismatic for observations on the yellow space, which, by being uncompressed, exposes a wider surface to view. This yellow space showed no superiority in resisting extinction over the other colours.

But as gaslight, compared with sunlight, is deficient in the more refrangible rays, I repeated the examination of the latter, as I had previously done for the prismatic spectrum, modifying the apparatus so as to use a grating in the place of the prism. The observations in this case of sunlight were quite as satisfactory as those in which gaslight had been used.

General Conclusions.—1st. In the prismatic spectrum the luminous intensity increases from the more to the less refrangible spaces, its maximum being not in the yellow but in the red. This is due to the action of the prism, which narrows and, as it were, condenses the coloured spaces more and more as we pass toward the red, increasing the intensity of the light as it does that of the heat.

2nd. In the grating, or diffraction spectrum, the luminous intensity is equal in all the visible regions, all the colours being simultaneously obliterated by an extinguishing light.

It must, however, be borne in mind that these conclusions should be taken in connection with the physiological action of the eye. Owing in part to the imperfect transparency of its media, and partly to the inability of its nervous mechanism to transmit waves of certain frequency to the brain, the spectrum does not begin and end sharply, as to a perfect eye a perfect spectrum ought to do.

There are, hence, two causes which must not be overlooked in these observations. 1st, the physiological peculiarity of the eye, which gives to each end of the spectrum the aspect of gradually fading away. 2nd, in the case of solar light the absorptive action of the atmosphere, which is chiefly exerted on the more refrangible rays.

I think, bearing in mind the correlation of light and heat, both being corresponding manifestations of the same vibratory movement in the ether, that these results substantiate those I published in 1872, on the distribution of heat in the spectrum; and that as the different coloured spaces are equally luminous, so they are equally warm.

I have made some attempts to compare with each other the luminous intensity of the bright lines in various spectra, especially those emitted by a strontium flame; but not being able to continue these researches at present, I have postponed them to a more favourable opportunity. J. W. Draper.

University of New York, May 5th, 1879.

Double Stars for September.

The most interesting Struvian Double Stars in the remaining hours of R.A. are here given. The discoveries of the elder Herschel, Σ., and OΣ. being now disposed of, a selection from the stars recently discovered by Burnham and others will be given in October and the following months; of these, many are excessively difficult, and many are southern stars. Many are certainly physical pairs, and some are already known to possess an interest scarcely inferior to that of the most famous of the older binaries. Very good service will be done to this department of Astronomy by amateurs who succeed in obtaining good measures of these objects.

Σ 2749. R.A. 20^{h} 58^{m}·7, Dec. +3° 3′. Mag. A 7·7, B 8·9, C 9.

A and $\frac{BC}{2}$	1876·3	153°·0	3″·65	Dembowski.
B C	77·8	148 ·9	1 ·12	Burnham.

A ternary system?

Σ 2758=61 Cygni. R.A. 21^{h} 1^{m}·3, Dec +38° 8′.
Mag. 5·3, 5·9.

1878	116°·7	20″·02	Goldney.

A famous binary.

OΣ 527. R.A. 21^{h} 2^{m}·4, Dec. +4° 40′. Mag. 7, 8.

1877·7	99°·4	0″·64	Burnham.

Binary. In rapid movement.

Σ 2777=δ Equulei. R.A. 21^{h} 8^{m}·5, Dec. +9° 32′. Mag. A 4, B 5, C 10.

A B (OΣ 535)	1877	156°·2	0″·2	Burnham.
A C	77	24 ·0	37 ·6	Flammarion.

The changes in A B are orbital: period 7 or 14 years. C is probably fixed.

τ Cygni. R.A. 21^{h} 10^{m}, Dec. +37° 32′. Mag. 4·8, 7·6.
Colours, yellow, blue.

1878·4	150°·0	1″·06	Burnham.

Discovered by Clark in 1874. Holden has detected a more distant companion.

1876·9	260°·3	15″·68	

Σ 2799. R.A. 21^{h} 23^{m}, Dec. +10° 33′. Mag. 6·6, 6·6.

1877·3	312°·3	1″·53	Doberck.

Orbital motion.

Σ 2822=μ Cygni. R.A. 21^{h} 38^{m}·8, Dec. +28° 12′. Mag. A 4, B 5, C 7·5.

A B	1877.7	117°·8	3″·76	Doberck.
A C	·6	57 ·1	209 ·9	Flammarion.

A B binary:

Σ 2900. R.A. $22^h\ 17^m{\cdot}8$, Dec. +20° 15′. Mag. A 6, B 9·2, C 8.

A B	1876·8	178°·9	1″·78	Doberck.
A C	77·8	330 ·2	63 ·5	Flammarion.

A B form a physical pair.

Σ 2909 = ζ Aquarii. R.A. $22^h\ 22^m{\cdot}6$, Dec. −0° 38′. Mag. 4, 4·1.

1878·8	332°·9	3″·35	Goldney.

A remarkable binary. Period 1578·3 years (Doberck).

Σ 3001. R.A. $23^h\ 13^m{\cdot}7$, Dec. +67° 27′. Mag. 5·2, 7·8.

1877·7	191°·6	2″·46	Doberck.

Binary. Colours, A very yellow, B very blue.

85 Pegasi. R.A. $23^h\ 55^m{\cdot}8$, Dec. +26° 27′.

A C	1878·4	33°·9	14″·16	Burnham.

In 1878 Mr. Burnham detected the duplicity of A. His most recent measures are:—

A B	1879·4	287°·2	0″·73	Mag. 6, 12.
A C	·3	29 ·9	15 ·0	„ 6, 9.
A D	78·9	277 ·1	61 ·73	„ 6, 13.

A B form a physical pair.

Σ 3062. R.A. $23^h\ 59^m{\cdot}9$, Dec. +57° 46′. Mag. 6·9, 8.

1877·7	297°·7	1″·43	Doberck.

Binary. Period 112·6 years (Schur), 104·4 years (Doberck).

Correction. 'Observatory,' vol. ii. p. 382. Read:—"F was discovered to be double by Bird," &c. Mr. G. Hunt has kindly pointed out this error.

JOSEPH GLEDHILL.

Mr. Edward Crossley's Observatory,
Bermerside, Halifax.

Meteor Notes for September.

EVENING observations may be made between the 9th and 23rd, and in the mornings between the 14th and 28th. The number of radiant-points visible this month is very large, and for the most part they are situated in Cassiopeia, Lacerta, Auriga, Perseus, Lynx, Pegasus, and Cepheus. It is for the observer to single out their accurate positions, and disentangle them in cases where several lie near together.

A rich shower near ε Persei, at 60° +37°, should be looked for between the 9th–15th. Two radiants from 154° +41° and 130° +46° also require further observation in the mornings during the last half of the month. On the 27th–28th there is a well-marked radiant N. of α Aurigæ (84° +52°), seen by Zezioli, A. S. Herschel, and others, and it was observed by the writer as early as the 5th in 1877. On the latter date a shower should be expected from the vicinity of α and β Cephei; for Zezioli records on Sept. 5, 1867, $9\frac{1}{2}^h$, that nine small shooting-stars diverged from that region in various directions. On Sept. 21, 1876, Corder noticed a good centre of radiation of 10 extremely small meteors at 352°·5 +16°·5, and on the 17th, 21st, and 23rd there were radiants in Lacerta, on the 20th–21st at δ Cygni, and at the end of the month near Polaris. Sawyer observed an exact radiant of swift, short meteors at 3° +21° on Sept. 27th–30th, 1878, which appears to be a new shower.

There is a conspicuous radiant in September, which almost coincides with the position of the August Perseids; Heis gives the centre at 43½° +62½°, Sept. 1–31 (51 meteors); Tupman saw it at 42° +55° on Sept. 5, 1870; and the writer has determined a radiant at 45° +57°, Aug. 24–Sept. 14 (23 meteors).

Further details of September meteor-showers are given in the 'Observatory,' Vol. i. p. 157 and Vol. ii. pp. 163–165.

Fireballs are decidedly numerous this month, and may be looked for with more than ordinary chance of success on the 1st–2nd, 6th–7th, 10th–13th, and 25th.

Recent Observations.—At Bristol the following observations were recently made by the writer:—

Date.	Time.	Duration.	Meteors seen.	State of sky.
1879.	h m h m	hrs.		
July 28......	11 45 to 13 45	2	45	Slight fog; stars dim.
29......	10 30 to 13 0	2½	26	Sky clear; moonlight.
Aug. 9	9 30 to 12 0	2½	33	Much cirrus cloud.
11......	9 40 to 10 10	½	20	Sky clear; cloudy after 10^h 10^m.
12......	11 15 to 12 0	¾	19	Sky clear; clouds at 12^h.

Thus 143 shooting-stars were seen in 8¼ hours of watching during the period from July 28 to Aug. 12. The night of August 10 was overcast throughout. On July 28 the Aquariads were the chief shower, with a radiant at 338° −14° (14 meteors). There were six bright Perseids II. and several meteors from Delphinus (307° +10°), and the region of η Pegasi and Lacerta. On July 29 the Moon was up until 12^h 30^m, and there were not many shooting-stars. The best shower was from 30° +37°, which supplied 6 swift, streak-leaving meteors. On Aug. 9, 33 meteors were counted, and less than half of these were Perseids with a centre at 46° +58°. A bright stationary meteor =Sirius was recorded at 10^h 43^m at the point 342° −16°, and evidently belonging to the same radiant as the July 27–30 Aquariads, which certainly endures through the first half of August, for in 1877 the writer observed 10 meteors from a centre at 342° −12°, Aug. 3–16. On Aug. 11 the sky was clear for ½ hour; and of the 20 meteors seen, 15 were Perseids. They were increasing remarkably fast when clouds came up at $10^h 10^m$, for during the last ten minutes of the watch there were 12 meteors. On Aug. 12 the sky was clear at times. Of the 19 meteors seen in the ¾ hour from $11^h 15^m$ to 12^h, 8 were Perseids; but the display on this night was much inferior to that of the preceding night. In all about 38 Perseids were observed on Aug. 9–12, and they gave a pretty well-defined radiant at 46° +58°. The paths of the brightest meteors seen were as below:—

Date.	Time.	Mag.	Path from	Path to	Notes.	Radiant.
1879.	h m		° °	° °		
July 26...	13 36	♂	180 +75	152 +73	Slow; yellow; train.	η Draconis.
July 29...	12 16	♃	140 +66	158 +54	Rapid; streak.	Perseids II.
July 29...	12 35	>1	347 +33½	337 +20	Rapid; no streak.	? Perseids II.
Aug. 9 ...	10 4	>1	309 −11	313 −24	Slow; inexact.	Aquilids or Draconids II.
Aug. 9 ...	10 43	>1	342 −16	stationary.	Transient.	Aquariads.
Aug. 9 ...	11 53	♃	34 +78	254 +85½	Rapid; streak.	Perseids I.
Aug. 12...	11 30	♃	6 +47	351 +37	Rapid; streak.	Perseids I.
Aug. 12...	11 55	♂	30 +64	50 +54	Slowish; bright train of sparks.	Draconids II.

During the writer's observations of the last few annual returns of the Perseids, a radiant-point in Musca has become conspicuous, supplying very swift, long-pathed meteors with slender streaks of 15° or 20°. From 19 paths (including several corroborative ones registered by the Italian observers in 1872), the shower-centre is exactly derived at 44° +25°. The position is not new, for Weiss found a radiant at 44° +24°, Aug. 11–12, 1869, and it may be also identical with 54° +28°, Aug. 3–12 (Schmidt), 48° +19°, Aug. 10 (Denza), and 39° +28°, Aug. 22–28 (Tupman). There was also a radiant-point this year of some bright slow meteors, devoid of streaks or trains, from near β Ursæ Minoris, at about 215° +76°.

Mr. V. Cornish at Debenham observed 26 meteors between July 23rd–28th. On the 28th two radiants were suspected in positions at 263° +66° and 288° +67½°. A stationary meteor (3rd mag.) was noted at 280° −16½° on the 24th.

Major Tupman at Blackheath, S.E., secured the tracks of 18 meteors between 10.28 and 11.40, on Aug. 11. Of these, 14 or 15 were Perseids, giving a normal radiant at 45° +56°; but there was evidence of two centres separated 6° or 7° in declination. Four bright Perseids were recorded as below:—

			From	to	
	h m		° °	° °	
Aug. 11......	10 50	= ♀	165 +73	186 +53	Swift; bright streak.
"	10 50½	= ♀	165 +59	183 +34	Swift; bright streak.
"	10 55	= ♃	235 +39	235 +19	Very swift; streak.
"	11 18	= ♃	0 +68	310 +58	Streak.

Mr. J. L. McCance at Putney Hill, S.W., saw a bright white meteor on July 23, 12.9½, =1st mag. star, passing rapidly from about 16¼° +33° to 18° +28°. It left a brightish train, which, however, quickly died out.

The Rev. G. T. Ryves writes from Stoke-on-Trent that on Aug. 9 he saw 13 meteors (of which 12 were Perseids), during an hour's watch of a cloudless sky, from 9 to 10 P.M. The 10th was overcast. On the 11th observations were commenced at 10.40; and during the first 20 minutes 12 meteors were seen, of which three were large shooting-stars, about equal to Sirius in brightness and accompanied by streaks. From 11^h until 13^h, 48 others were counted, including 40 Perseids, but afterwards the sky became cloudy.

The night of August 10, when the chief display was expected, appears unfortunately to have been overcast at nearly all stations. It is therefore impossible to say at present whether this year's return of the Perseids has been of exceptional character. Certainly, so far as may be inferred from observations on the 9th and 11th, the shower was not very remarkable, differing little from those of the few preceding years; but we may have more favourable reports from foreign observatories. W. F. DENNING.

CORRESPONDENCE.

To the Editor of 'The Observatory.'

The Inner Satellite of Mars.

SIR,—

Mr. G. H. Darwin's tidal theory of the evolution of satellites, an abstract of which was given in the 'Observatory' for July, will doubtless attract the general attention of astronomers, not only because of its mathematical basis, but also from the fact of its obvious inconsistency with the cosmogony of Laplace. The closing paragraph, however, of the article referred to can hardly be accepted without further explanation. Having shown that the mutual influence between a satellite and the tide which it produces in its primary must retard the rotation of the planet, and at the same time lengthen the period of the satellite, Mr. Darwin concludes:—

"The anomalous satellite of Mars, which revolves faster than its primary rotates, appears to present a case strongly confirmatory of these views; for it seems probable that its extreme minuteness has preserved it as a standing memorial of the primitive time of rotation of Mars round its axis."

Now the mass of this inner satellite is to that of our Moon in the (approximate) ratio of 1 to 30,000,000. It can therefore produce no sensible tides in the primary. It is, in fact, from this absence of tides that Mr. Darwin infers the *constant* distance of the satellite. What, then, has lengthened the rotation period of Mars from less than 8 to nearly 25 hours?

I am, Sir, yours faithfully,
DANIEL KIRKWOOD.

Bloomington, Indiana, U. S. A.,
July, 1879.

The Occultation of Antares, 1879, *July* 28.

SIR,—

I observed the occultation of Antares by the Moon on the evening of the 28th; the night was clear, but the atmosphere presented some opacity peculiar to east wind in this locality; the wind blew with violence—that is to say, it reached the number 5 of the scale.

I made use of my Browning equatoreal reflector of 8½ inches. The chronometer was compared with the astronomical clock before and after the observation.

At 8ʰ 30ᵐ the star was perfectly visible to the naked eye; I observed it through the telescope, using different powers, but I could not find the *comes*.

At 8ʰ 56ᵐ 10ˢ·9, Cadiz M.T., the immersion took place without presenting any particular phenomenon of any kind.

At 10ʰ 0ᵐ 49ˢ·9 the emersion took place somewhat to the north of Mare Crisium; three or more minutes before this moment I saw on the very edge of the Moon one or two very small stars, which appeared and disappeared four or five times; the undulations of the air were very considerable, and I was obliged to use the lowest of the eyepieces.

When Antares appeared it was alone, and I could not perceive the small attendant.

I am, Sir, yours very faithfully,
AUGUSTO T. ARCIMIS.

Cadiz, 1879, July 31.

SIR,—

The occultation of Antares was seen here on the night of Monday the 28th of June. The evening was fairly fine, but the air was tremulous, and the Moon and star were in "a boil." At 9.15 we took three dry plates, two of Wratten and Wainwright's instantaneous, with 10 seconds exposure. Both these showed the Moon and star (as we considered), but a Kennett dry plate with longer exposure showed the Moon only without the star.

With the 6-in. Cooke equatoreal Mr. Dowlen and myself made the time of disappearance to be

$$9^h\ 38^m\ 35^s \text{ G.M.T.}$$

Two friends at the 3½-in. Cooke made the time of disappearance,

$$9^h\ 37^m\ 55^s \text{ G.M.T.}$$

When the star reappeared, the Moon was too low for the equatoreal; and using the $3\frac{1}{4}$-in. I saw the reappearance (through a considerable mist) at

$10^h\ 8^m\ 16^s$ G.M.T.

The emergence was, however, well seen, and the star seemed to hang momentarily on the Moon's limb. The false rays were extinguished by the mist, and a disk, like a satellite of Jupiter, was visible. Antares disappeared absolutely suddenly, and without a trace of gradual extinction. One of our party saw the phenomenon plainly with the naked eye. The *comes* could not at any time be seen. Definition of the star-spectrum was most imperfect, and a few dark thick lines in the red only could be traced. Power on equatoreal, Browning's achromatic B; on $3\frac{1}{4}$-in. Cooke, 50 Huyghenian.

I am, Sir, yours faithfully,
J. Rand Capron.

Guildown, Guildford,
1879, Aug. 4.

[The occultation of Antares was observed by several observers at Greenwich. The *comes* was very distinctly seen with the Great Equatoreal of 13 inches, and less clearly with a 4-inch Simms refractor. With the former the disappearance of the small star took place about 10 seconds before that of Antares, its blue colour being very striking by contrast with the fiery red of the large star. At reappearance the star was very low and the *comes* was not seen.—Ed.]

Sir,—

This phenomenon was observed under very favourable conditions with the $3\frac{3}{8}$-in. achromatic telescope of 46 inches focus.

Just before disappearance the image became very quiet, and the companion star was distinctly seen with power 120. I carefully kept the star as near the centre of the field as possible, and watched the approach of the Moon. I could not see the dark limb, but was soon made aware of its existence by seeing the small star vanish first, and immediately afterwards the great star was snuffed out with a suddenness that was quite startling. Both stars were beautifully defined right up to the Moon's limb, and showed no signs of any clinging or distortion whatever. At the reappearance the star was so low that the companion could not be seen.

This small star was discovered by Prof. Mitchell with a 12-inch object-glass in 1844; but I think the difficulty of seeing it is greatly overrated, for about half an hour before the occultation I could see it very well with power 180, but not with 120, this latter power not separating it far enough from the blaze of the great star; but just before disappearance, when the brightness of the Moon greatly diminished this blaze, and gave the great star a neater disk, 120 showed the companion perfectly, and its colour (greenish blue) was in striking contrast to the deep orange of the great star.

It is evident that in those latitudes where Antares reaches a considerable meridian altitude the companion would be quite an easy object; it is of the 7th magnitude. Distance 3″·5. Position-angle 273°. In this country it is merely a test of the condition of the atmosphere and not of the goodness or power of the telescope. I am, Sir, yours faithfully,

CHARLES GROVER.

SIR,—

I send you a few remarks on the late occultation of Antares, July 28, 1879, as there appeared to be something unusual in the disappearance of the bright star.

Time recorded $9^h\ 36^m\ 34^s$ G.M.T., but the brightness, and especially the distinctness, of the star altered very much 3½ seconds earlier.

I was using an Airy's compensating eyepiece, which, in my 5¼-inch refractor, magnified about 250 times and gave a very fairly defined image.

I had seen the companion very shortly before the disappearance, but failed to notice its occultation; but the bright star suddenly became blurred and expanded so as to resemble in its hazy character the planet Uranus, and greyer than before, and after maintaining this appearance for more than two seconds it faded rapidly, but not instantaneously, out of sight.

Another observer, who was using a 2¼-inch telescope with magnifying-power 45, did not notice this phenomenon. I did not see the companion after the reappearance, which was timed $10^h\ 6^m\ 23^s$, but the star was then very low.

Is this change and gradual extinction to be accounted for by lunar atmosphere, or could it be, as the appulse was very oblique, that the star was for a few seconds only partially screened from us by a lunar peak or sierra?

Colebyfield, Wimbledon, I am, Sir, your obedient servant,
1879, Aug. 8. F. C. PENROSE.

Changes in a Group of Solar Spots.

SIR,—

I beg to send the following record of changes in a group of solar spots as seen at Guildown, Guildford, with a 6-in. Cooke equatoreal and low-power Kellner eyepiece.

June 27, 10 A.M. Clouds passing over Sun; air unsteady. Two small spots seen; upper one with simple nucleus, lower one nucleus compound.

June 28, 1.10 P.M. Group now seen to consist of 4 large spots and many smaller ones; air unsteady and clouds passing at times. The Rev. T. Wiltshire assisted me in sketching, and we thought some of the small spots changed in form and development while making our drawings.

June 30, 3.30 P.M. Air unsteady, with wind and light clouds.

Nucleus of upper spot compound; lower spot incomplete round margin, with appearance as if breaking up on one side. Group surrounded by not well-defined faculæ.

July 1, 5.25 P.M. High wind; many faculæ round the group. "Rice grains" very distinct, especially so near group of spots. Central spot faintly seen, and has a very pale margin. Lower spot composed of two masses of granular-looking matter without nuclei.

July 2, 4.5 P.M. High wind and clouds. Central spot of yesterday shown only by two minute dots. Upper spot but little changed. Lower granular matter now changed into two spots with well-defined nuclei and margins.

July 4, 7.45 A.M. But one spot left with brilliant ring of faculæ round it. When the air became steadied, the spot was found bridged across. 11 A.M. no change.

July 5, 7.30 A.M. Spot still visible, but reduced in size; ring of faculæ very apparent, but more irregular. Spot bridged unequally (yesterday it was nearly equally bisected). 5 P.M. same day: spot gone; place occupied by a few faculæ.

Sun-spots appear to be becoming more frequent; but it is not often one has the opportunity to witness the rise and fall of a group as above described.

I am, Sir, yours faithfully,
J. Rand Capron.

Guildown, Guildford,
1879, August 4.

The Diameter of the Spurious Disk of a Star.

Sir,—

In regard to the above-named subject, I am much puzzled by a difficulty which I hope to be allowed to mention here. Sir G. Airy gives the formula for the first bright ring, $s=\frac{3\cdot70}{e}$, in which "*s* is the angular semidiameter of the ring as viewed from the centre of the object-glass," and "*e* the radius of the object-glass expressed in inches." From this it follows that the diameter of the ring varies inversely as the aperture (which is conformable to experience), and also that with the *same* aperture the diameter of the first ring *is the same for all stars.*

This seems to be the basis of Mr. Knott's very interesting and ingenious paper in No. 4 of the 'Observatory.'

Now, Sir J. Herschel, in his article "Light" in the 'Encyclopædia Metropolitana,' has the following passage, which I hope to be allowed to quote in its entirety, both on account of its intrinsic interest, and also because the nature of the work in which it is contained renders it not generally accessible. Speaking of the spurious disks of stars, he says:—"These disks were first noticed by Sir William Herschel, who first applied sufficiently high magnifying-powers to telescopes to render them visible. They are not the real bodies of the stars, which are infinitely too remote to be ever visible with any magnifiers we can apply, but *spurious* or unreal images, resulting from optical causes which are still to a certain degree obscure. It is evident, indeed, to any one who has entered into what we have said of the law of interferences, and from the explanation given in Art. 590 and 591 of the formation of *foci* on the undulatory system, that (supposing the mirror or object-glass rigorously aplanatic) the focal point in the axis will be agitated with the united undulations, in complete accordance, from every part of the surface, and must, of course, appear intensely luminous; but that as we recede from the focus in any direction in a plane at right angles to the axis, this accordance will no longer take place, but the rays from one side of the object-glass will begin to interfere with and destroy those from the other, so that at a certain distance the opposition will be total, and a dark ring will arise, which, for the same reason, will be succeeded by a bright one, and so on. Thus the origin both of the central disk and the rings is obvious, though to calculate their magnitude from the data may be difficult. But this gives no account of one of the most remarkable peculiarities in this phenomenon, *viz.* that the apparent size of the disk is different for different stars, being uniformly larger the brighter the star.

"This cannot be a mere illusion of judgment; because when two unequally bright stars are seen at once, as in the case of a close double star, so as to be directly compared, the inequality of their spurious diameters is striking; nor can it be owing to any real difference in the stars, as the intervention of a cloud, which reduces their brightness, reduces also their apparent disks till they become mere points." ("Light," paragraph 767.)

Rigel and its companion, seen with a first-rate object-glass, is a striking illustration of the above fact.

Now I wish to speak with great deference and submission on so recondite a subject; but it certainly does seem to me that the above theoretical formula does not here represent the *facts* of the case, as in different stars the diameter of the spurious disk and ring seems to be a function, not only of the aperture, but also, in some unexplained manner, of the relative light of those stars, conventionally termed their magnitudes. I should feel particularly obliged by any remarks which your able correspondents may be inclined to make on the subject.

I am, Sir, yours faithfully,
George Hunt.

Hopefield, West Dulwich, S.E.,
1879, August 5.

[We think that the variation in the size of the spurious disk according to the brightness of the star may be explained by the circumstance that, according to the Undulatory Theory, the light fades away *gradually* from the central point outwards to the first dark ring, and that with the fainter stars it is only the central portion which is sufficiently bright to produce a sensible impression. Sir

G. Airy has not given the diameter of the spurious disk, but that of the first dark ring, which is its *extreme* limit. With regard to the first bright ring, its diameter is theoretically the same for stars of all degrees of brightness, and we believe that this conclusion is in accordance with such observations as we have. In the case of very bright stars the imperfection of the telescope and the tremors of the air frequently confuse the central disk and the first and second bright rings in one mass of light; but there is nothing in this to invalidate the theoretical conclusion.—Ed.]

NOTES.

Fall of a Meteorite on the 10th of May, in Iowa*.—Professor S. F. Peckham writes from Minneapolis, May 29, 1879:—I have the pleasure of informing you that on the 10th of May a meteor exploded, and fell in full daylight, at 5 p.m., at Esterville, Emmet County, Iowa. One of the fragments, weighing about 500 pounds, fell on railroad land and was dug up from a depth of fourteen feet, in a stiff clay soil. Another smaller portion, weighing about 170 pounds, fell on the farm of A. A. Pingrey, at a distance of two miles from the first. Many smaller pieces, of a few ounces or pounds weight, were scattered in the vicinity. The smaller mass fell upon a dry knoll, and penetrated the earth vertically to a depth of 4½ feet. The fall was accompanied by a noise, described as a continuous roll of thunder, accompanied by a crackling sound.

Through the efforts of Professor E. J. Thompson of our Faculty, the smaller mass has been obtained for the University cabinet. It is irregularly square in form, about 15 × 18 inches, and of an average thickness of six inches.

A preliminary chemical examination shows the metallic portion to consist of an alloy of iron, nickel, and tin. Full half the mass consists of stony matter, which appears in dark-green crystalline masses, embedded in a light-grey matrix. When the whole is powdered, a violent reaction ensues on the addition of hydrochloric acid, which is increased on boiling. The boiling acid appeared to dissolve all but the grey matrix, abundance of iron passing into solution. Some of the crystalline masses are two inches in thickness, and exhibit distinct monoclinic cleavage. Under the microscope in thin sections, olivine and a triclinic felspar appear to be embedded in a matrix of pyroxene. This work is in the hands of Professor C. W. Hall, of the University, who intends to make a very thorough investigation of the optical properties of the minerals and matrix.

The chemical examination was first attempted upon a very small quantity of material, but, now that we have an ample quantity, a complete analysis of the several minerals and the alloy will be made. A small piece of the metal, polished and etched, exhibited the Widmanstättian figures very finely.

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* Amer. Journ. 1879, July.

The larger mass is still in the hands of those who dug it from the ground, although their ownership is contested by one who claims to have contracted for the land on which it fell. Their idea regarding its value enlarge daily, the latest announcement being, that they should feel insulted at an offer of $5000. We trust their feelings may be spared.

THE STRASSBURG OBSERVATORY.—Dr. Winnecke's Report for 1878 has been published in the 'Vierteljahrsschrift der Astron. Gesellsch.' With the 6-inch orbit-sweeper 118 observations of nebulæ were made, and a very marked periodic variation of brightness was established in the case of one nebula, *h* 882, and also probably in that of another, *h* 1061. The observations of Nova Cygni were concluded on Dec. 28, when the star was of about 12 mag. Some measures of double stars were made, and in particular a relative parallax 0″·2 was found for the brighter component of the optical double Σ 1516. This star is of 7–8 mag., and has a proper motion of 0″·4. Maxima and minima of a number of double stars were observed, and the minima of Algol were found to differ considerably from the ephemeris. Tempel's periodical comet 1873 was observed several times with the orbit-sweeper, and some meridian observations of the Moon and planets were made.

The series of observations of the Sun's diameter with the heliometer commenced in the previous year was continued, and a number of miscellaneous phenomena were observed. The building of the new Observatory is proceeding; and the new transit-circle, altazimuth, and orbit-sweeper will be erected there in the present year, 1879.

SIR THOMAS MACLEAR, formerly her Majesty's Astronomer at the Cape, died at Mowbray, near Cape Town, on July 14, in his eighty-fifth year. He was born in the county of Tyrone, Ireland, and educated at Winchester. Following the medical profession, he settled at Biggleswade, where he erected a small observatory, furnished with the Wollaston telescope of the R. A. S.; here he took a considerable interest in the observation of occultations and occasional astronomical phenomena. On the return of Mr. Henderson from the Cape in 1834, Mr. Maclear was appointed Her Majesty's Astronomer in that Colony, an office which he held till his resignation in 1870. During his direction of the observatory a large number of observations were made, though only a portion of them has been reduced and published. The most important undertaking carried out by Sir Thomas Maclear was the verification of Lacaille's arc of meridian, the details of which were published in 1866 in two quarto volumes. For this work Sir Thomas was awarded the Lalande Medal of the Academy of Sciences of Paris, of which he was, since 1863, a corresponding member.

THE death of Professor LAMONT, at the age of seventy-four, has also been announced. This distinguished astronomer and physicist was Director of the Munich Observatory, to which he was appointed in 1835. He was one of the greatest authorities in modern terrestrial magnetism; and soon after his appointment to the Observatory he inaugurated a regular system of magnetic observations, the success of which led to the establishment of magnetic observatories in different parts of the globe. In astronomy, Prof. Lamont has done most excellent service by the publication of zone-catalogues of small stars between 15° north and 15° south of the equator, supplementing the more extensive zone observations of Argelander and Bessel. From a comparison of the reduced places with those of corresponding stars in other catalogues, they have been found to be very accordant. Prof. Lamont was a Scotchman by birth.

THE object detected by Dr. Peters on July 17, at first supposed to be a new planet, has turned out to be Frigga, No. 77, which for many years had escaped observation. The minor planet discovered by Dr. Peters on July 27 therefore becomes 200, and not 201 as recorded in the last N°.

A NEW minor planet, No. 201, was discovered by Herr Palisa at Pola on the evening of August 7.

A TELESCOPIC comet was discovered on August 21 by Herr Palisa, in R.A. $10^h\ 2^m$ and Decl. 49° 6′ N., its daily motion being $+6^m$ in R.A. and $-3'$ in Decl. The comet is round and tolerably bright.

ON August 24, Dr. Hartwig discovered, at Strassburg, a faint comet in R.A. $12^h\ 19^m$ and Decl. N. 61° 2′. Motion towards the south-east.

EDITOR.

Astronomical Memoranda, 1879, October.

Sun. Oct. 1, sets $5^h\ 38^m$, rises $18^h\ 4^m$; Oct. 31, sets $4^h\ 34^m$, rises $18^h\ 55^m$.

Equation of time:—Sun *before* clock, Oct. 1, $10^m\ 16^s$; Oct. 11, $13^m\ 10^s$; Oct. 21, $15^m\ 15^s$; Oct. 31, $16^m\ 16^s$.

Sidereal Time at Mean Noon:—Oct. 1, $12^h\ 39^m\cdot4$; Oct. 11, $13^h\ 18^m\cdot8$; Oct. 21, $13^h\ 58^m\cdot2$; Oct. 31, $14^h\ 37^m\cdot7$.

Moon. Last Quarter, Oct. 8, $1^h\ 43^m$; New, Oct. 15, $3^h\ 9^m$; First Quarter, Oct. 21, $18^h\ 18^m$; Full, Oct. 29, $14^h\ 10^m$.

Occultations:—Oct. 1, $11^h\ 17^m$ 101 Piscium Oc. R. 300°; Oct. 4, $11^h\ 39^m$ 36 Tauri Oc. R. 273°; Oct. 5, $12^h\ 19^m$ *k* Tauri Oc. R. 304°; Oct. 7, $16^h\ 29^m$ B.A.C. 2238 Oc. R. 220°, Oct. 11, $17^h\ 35^m$ B.A.C. 3529 Oc. R. 198°; Oct. 20, $7^h\ 46^m$ B.A.C. 6369 Oc. D. 53°; Oct. 24, $7^h\ 12^m$ θ Aquarii Oc. D. 141°, $8^h\ 27^m$ θ Aquarii Oc. R. 287°; Oct. 30, $11^h\ 13^m$ ϵ Arietis Oc. D. 180°, $11^h\ 43^m$ ϵ Arietis

Oc. R. 224°. The angles at disappearance (D.) and reappearance (R.) are reckoned from the *apparent* N. point towards the right hand round the Moon's circumference as seen in an inverting telescope.

Mercury is in superior conjunction on Oct. 5, after which he is an evening star setting shortly after the Sun.

Venus is a morning star, stationary on Oct. 12 and at greatest brilliancy on Oct. 30. The illuminated portion of the disk is 0·026 on Sept. 15 and 0·130 on Oct. 15, and the corresponding diameters 56″·7 and 48″·9 respectively.

Oct. 1, R.A. $11^h\ 34^m\cdot2$ Dec. 5° 34′ S., tr. $22^h\ 50^m$, rises $17^h\ 13^m$.
Oct. 31, 11 47 ·4 0 15 S., 21 8 15 7

Mars is visible all night, nearly stationary in Taurus. His diameter is 19″·6 on Oct. 1 and 23″·0 on Oct. 31.

Oct. 1, R.A. $3^h\ 47^m\cdot1$, Dec. 18° 6′ N., tr. $15^h\ 5^m$.
Oct. 31, 3 29 ·0, 18 23 N., 12 48

Jupiter is an evening star nearly stationary in Aquarius. His diameter is 44″·9 on Oct. 1 and 41″·3 on Oct. 31.

Oct. 1, R.A. $22^h\ 26^m\cdot1$, Dec. 11° 18′ S., tr. $9^h\ 45^m$, sets $14^h\ 50^m$.
Oct. 31, 22 21 ·2, 11 41 S., 7 42 , 12 46

Jupiter's Satellites:—Oct. 1, $6^h\ 44^m$ i. Ec. R.; Oct. 2, $8^h\ 17^m$ ii. Ec. R., $10^h\ 39^m$ iii. Oc. D.; Oct. 7, $8^h\ 11^m$ i. Tr. I., $10^h\ 30^m$ i. Tr. E., $12^h\ 0^m$ ii. Tr. I.; Oct. 8, $6^h\ 36^m$ iv. Oc. D., $8^h\ 40^m$ i. Ec. R., $10^h\ 56^m$ iv. Oc. R.; Oct. 9, $6^h\ 15^m$ ii. Oc. D., $10^h\ 53^m$ ii. Ec. R.; Oct. 13, $7^h\ 31^m$ iii. Tr. E., $12^h\ 50^m$ i. Oc. D.; Oct. 14, $9^h\ 59^m$ i. Tr. I.; Oct. 15, $7^h\ 17^m$ i. Oc. D.; Oct. 16, $6^h\ 45^m$ i. Tr. E., $8^h\ 37^m$ ii. Oc. D.; Oct. 17, $6^h\ 13^m$ iv. Sh. E.; Oct. 18, $6^h\ 29^m$ ii. Tr. E.; Oct. 20, $7^h\ 32^m$ iii. Tr. I., $11^h\ 7^m$ iii. Tr. E.; Oct. 21, $11^h\ 48^m$ i. Tr. I.; Oct. 22, $9^h\ 6^m$ i. Oc. D.; Oct. 23, $6^h\ 15^m$ i. Tr. I., $8^h\ 34^m$ i. Tr. E., $11^h\ 0^m$ ii. Oc. D., Oct. 24, $7^h\ 0^m$ i. Ec. R.; Oct. 25, $6^h\ 3^m$ ii. Tr. I., $8^h\ 55^m$ ii. Tr. E., $9^h\ 34^m$ iv. Ec. D.; Oct. 27, $11^h\ 12^m$ iii. Tr. I.; Oct. 29, $10^h\ 57^m$ i. Oc. D.; Oct. 30, $8^h\ 5^m$ i. Tr. I., $10^h\ 25^m$ i. Tr. E.; Oct. 31, $5^h\ 25^m$ i. Oc. D., $6^h\ 4^m$ iii. Ec. D., $8^h\ 55^m$ i. Ec. R., $9^h\ 22^m$ iii. Ec. R.

Saturn is in Pisces, in opposition on Oct. 5.

Oct. 1, R.A. $0^h\ 50^m\cdot9$, Dec. 2° 30′ N., tr. $12^h\ 9^m$.
Oct. 31, 0 42 ·6, 1 39 N., 10 3

	Outer Ring.		Inner Ring.		Ball.
	Maj. Axis.	Min. Axis.	Maj. Axis.	Min. Axis.	Diam.
Oct. 7...	44″·51	6″·76	29″·60	4″·50	17″·8
Oct. 27...	44″·14	6″·22	29″·35	4″·13	17″·6

The south side of the rings is visible, the elevation of the Earth above their plane being 8° 44′ S. on Oct. 7 and 8° 6′ S. on Oct. 27, and of the Sun 9° 4′ S. and 9° 22′ S.

Neptune is in Aries, approaching opposition.

Oct. 1, R.A. $2^h\ 38^m\cdot7$, Dec. 13° 33′ N., tr. $13^h\ 57^m$.
Oct. 31, 2 35 ·4, 13 17 N., 11 56

EDITOR.

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THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

No. 30. OCTOBER 1. 1879.

Spectroscopic Observations of Meteors.

I OBSERVED with the spectroscope on July 26 and 28 a number of meteors which have given me the following results:—The nuclei nearly always exhibit a continuous spectrum, and the trains a discontinuous; but there are also exceptions, the trains giving occasionally a continuous spectrum. In the majority the brightest line of sodium can be distinguished; but there are many of the trains in which nothing but a continuous spectrum can be seen.

I think this may arise from a great difference in the height of the different meteors, and that the spectrum does not show the sodium-line when the meteor becomes incandescent at a great height, whilst this line is always found in the spectrum of a meteor which moves at a moderate height. In fact I should attribute the sodium not to the meteor itself, but rather to our atmosphere. It is certain that in the high regions the atmosphere contains little or no sodium, whilst in the lower regions it contains enough to be visible in the spectroscope; so that the presence of the sodium line in the spectrum of meteors would be due to the incandescence of the air itself compressed by the meteor.

I have also found in the spectrum of some of the July meteors a red line; and although 1 have not been able to perceive any blue line, I can attribute this red line to nothing but potassium, the blue line being invisible on account of its faintness.

On August 12, 13, and 14, I also observed a number of meteors with the spectroscope, amongst others on the 12th a yellow fireball with a fine train, which came directly from the Perseid radiant. In the head of this meteor the lines of lithium were clearly seen by the side of the sodium line. Three seconds later I observed another meteor of the second magnitude; but, notwithstanding its brightness, I could not find any trace of lines.

On August 13, at $10^h 46^m 10^s$, I observed in the north-east a magnificent fireball of emerald-green colour, as bright as Jupiter, with a very slow motion. The nucleus at the first moment only showed a very bright continuous spectrum with the sodium line; but a second after I perceived the magnesium line, and I think I am not mistaken in saying those of copper also. Besides that, the spectrum showed two very faint red lines.

On August 14 also I observed several meteors with the spectroscope; but I only found one in which I could see a faint red line by the side of the sodium line.

Besides these spectroscopic observations, Captain Charles de Reviczky observed on July 26, at $11^h\ 48^m$ O'-Gyalla M.T., a stationary meteor of the third magnitude in R.A. $2^h\ 0^m$, Dec. $+29°\ 0'$.

On August 11, $9^h\ 47^m\ 1^s$ O'-Gyalla M.T., my assistant, M. J. Rosenzweig, also observed a stationary meteor of the third magnitude in R.A. $2^h\ 14^m$, Dec. $+55°\ 18'$.

We have registered the positions of meteors of the July and August showers on the following dates:—July 25, 72 meteors; July 26, 87 meteors; July 28, 26 meteors; August 11, 110 meteors; August 12, 50 meteors; August 13, 50 meteors; August 14, 35 meteors.

I defer publishing the positions of these meteors till the radiants have been calculated. N. DE KONKOLY.

O'-Gyalla Observatory, Hungary.

On Meteoric Fireballs seen in the United States during the year ending March 31, 1879*.

THE following paper does not claim to present a complete list of the fireballs which have appeared in our entire country during the last twelve months. It includes, however, all that have been brought to the writer's notice. Of those described, only three can be certainly classed as detonating meteors, and in no case has an explosion been followed by a fall of meteoric stones; at least, no aërolites have been actually discovered.

(1) 1878, June 3, $2^h\ 59^m$, A.M.—This meteor, observed at Chicago by Prof. E. Colbert, was about equal in apparent magnitude to the moon at four days old. Its course was from near the zenith to a point about 4° above β Cassiopeiæ. Near α Cassiopeiæ it exploded into seven or eight fragments†.

(2) 1878, June 6, $9^h\ 25^m$ (local time).—On the evening of June 6 Mr. George H. E. Trouvelot at Cambridge, Mass., saw a very large meteor which passed directly over o Ursæ Majoris, and disappeared just below η in the same constellation. It was pear-

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* Read before the American Philosophical Society, May 2, 1879.

† Letter from Prof. E. Colbert, dated June 3, 1878. See also the 'Science Observer' for July 1878, p. 3.

shaped, the greatest and least diameters being in the ratio of 4 to 3; and it left behind it a long bright train. About three or four seconds after its appearance it burst into five or six fragments, each of which assumed the same form as the original meteor. "The preceding portion was of a crimson-red hue, quite brilliant, and not dissimilar to the strontian flame. The following portion was of a bluish-violet colour, which merged into that of the trail. This latter was composed of globules, each succeeding following one being of a more and more subdued violet, and finally not distinguishable from the colour of the sky"*.

(3) 1878, August 11, 10^{h} 10^{m} (Indianapolis time).—A few minutes after 10 o'clock on Sunday evening, Aug. 11, the Rev. John A. Bower, of Bloomington, Indiana, saw a large meteor near the eastern horizon. Mr. Bower had just taken a position facing an eastern window. The meteor when first seen was almost exactly east of Bloomington, perhaps two or three degrees south of east, and 10° above the horizon. Its motion was from south to north, and the length of its apparent track was 20° or 25°. The first half of its course was but slightly inclined to the earth's surface; the inclination, however, became sensibly greater towards the point of disappearance, which was N. about 70° E., and very near the horizon. The apparent diameter of the meteor was about one third that of the Moon. The motion was extremely rapid, the time of flight not exceeding two seconds. No detonation was heard at Bloomington, nor was the meteor seen to separate into fragments at the time of disappearance.

The observations of Mr. Bower were given me verbally. To verify their accuracy I placed myself in the position which he occupied, and had him point out the meteor's course as he had seen it. The foregoing statement, I am satisfied, must be very nearly correct, except as to the time of flight, which is admitted by the observer to be very uncertain.

The same meteor is supposed to be described in the following telegrams which appeared in the newspapers of the next Tuesday morning:—"Titusville, Crawford Co., Pa., August 12. A beautiful meteoric display was witnessed from here last evening. The meteor made its appearance in the west at 10.30, moving in a northerly direction. It was of a greenish colour, and shone with great brilliancy, lighting up the entire surrounding country with a light that for the time prevailed over that of the full Moon. Its appearance was only momentary, when it burst and divided into three fragments, two of which assumed a reddish colour. Calculating from the time the explosion was seen until it was heard, the meteor was about 25 miles distant." "Oil City, Venango Co., Pa., August 12. A meteor of unusual brilliancy passed here last evening a few minutes after 10 o'clock. It was nearly twice the size of a cannon-ball. Its course was north."

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* 'Science Observer,' June 1878.

All accounts agree that the meteor's course was northward. It was seen to the west of Titusville; and as the final explosion occurred about 25 miles from that city, we may conclude that the track terminated over Crawford County, Pa. The observations at Bloomington, Indiana, indicate that the body first became visible over West Virginia. The distance directly east from Bloomington to the meridian which bounds Verango County, Pa., on the west, is 348 miles. Hence the meteor's altitude when first seen was about 77 miles. This, it must be admitted, is somewhat uncertain; but we may safely conclude that it was not less than 70 miles nor more than 85. The length of the visible track was between 160 and 175 miles. The estimated time of flight was probably too short; but the great velocity appears to indicate a hyperbolic orbit.

(4) 1878, September 16, $9^h\ 0^m$, P.M.—This meteor was observed by Mr. Benjamin Vail, of Henryville, Clark County, Indiana. It was first seen near γ Ursæ Majoris, and it passed over ζ in the same constellation. Its apparent diameter was about one fourth or one fifth that of the Moon*.

(5) 1878, November 12, $7^h\ 0^m$, P.M. (local time).—Washington, Indiana. Mr. D. Eckley Hunter, Principal of the High School, of Washington, Daviess County, Indiana, was, with several of his students, watching for shooting-stars on the evening of November 12, when, precisely as the town clock was striking seven, a large fireball appeared very close to Vega, passed in a southerly direction through the Milky Way, and disappeared about 20° N.W. of Jupiter. Its motion was very slow, the time of visibility being estimated by Mr. Hunter at 10 seconds. Its apparent diameter was about two thirds that of the Moon. What struck Mr. Hunter as especially remarkable was the sharply defined disk which the meteor presented up almost to the very moment of its disappearance†.

(6) 1878, November 14, $3^h\ 30^m$, P.M.—In the New York semiweekly 'Tribune' of December 10, 1878, Mr. Thomas Whitaker, of Hillside Farm, Mass., reports the appearance of this brilliant meteor as observed by himself. The sky was very clear at the time, and the meteor was seen in bright sunshine. It was due south from the place of observation.

(7) 1878, December 30.—A few minutes before 7 o'clock (Indianapolis time) on the evening of December 30, 1878, a large meteor was seen in Indiana, Ohio, and Pennsylvania. So far, however, as known to the writer, the only observations sufficiently precise to be available in determining the height and direction of its path were made at Anderson, Madison County, Indiana; Washington, Washington County, Pa.; and Wooster, Wayne County, Ohio.

Anderson, Indiana, lat. 40° 5′, long. 8° 28′ W.—The observations at Anderson were made by Mr. Frederick E. Dickinson, a member of the Senior Class in Indiana University‡. Mr. Dickinson was in the street, walking eastward, when the meteor appeared in front of him a few degrees N. of E. at an altitude of not less than 15° nor more than 17°. As the meteor passed behind a building, the point of disappearance could not be determined. The apparent diameter was one fourth that of the Moon, and the time of flight was estimated at two seconds, or probably a fraction less.

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* Letter from Mr. Vail.

† Letter from Mr. D. E. Hunter.

‡ The meteor was seen by others in Indiana; but the descriptions given were nothing more than vague guesses in regard to its size and general direction.

Washington, Pa., lat. 40° 10′, long. 3° 12′ W.—The phenomenon as seen in Washington is described in the following letter from the Professor of Mathematics in Washington and Jefferson College:—

"Washington, Pa., February 1, 1879.

"Dear Sir,—The fact of the appearance of a meteor here some time ago may be of interest to you. I set down the facts as I learned them at the time, and intended to have written you sooner. About 7 P.M., Washington, Pa., time, December 30th, a brilliant meteor was seen here. The account which I have was given me by a young man, Mr. A. M. Gow, Jun., who has given me, I have no doubt, a very fair statement of what was to be seen at that time. He was walking eastward on the south side of the street, so that he was in the shade of the buildings, the moon shining brightly at the time. Suddenly a light shone about him as if an additional lamp had been lighted close behind him. He did not turn immediately to look, but when he did he saw a meteor about the size of the Moon as he thought by comparing his impression with the half-full Moon immediately afterwards. It was of a slightly greenish colour; but just as it disappeared it became reddish. The place in which he saw it, as far as I can judge, was about α Cygni, and the place of its disappearance was about α Lyræ. If so, you see its course was W.N.W., and it was observed through 24° of its path. The meteor had been visible a little time, however, before Mr. Gow turned to look. It was seen by three others here that I know of.

"Yours truly,
"D. J. M'Adam."

"*Prof. D. Kirkwood.*"

Observations at Wooster, lat. 40° 50′ *N., long.* 4° 56′ *W.*—Professor Samuel J. Kirkwood, of Wooster University, had a good view of the meteor, which he describes as the most brilliant he has ever seen. It was greatly elongated, and the apparent diameter at right angles to its path was half that of the Moon. The point of the meteor's first appearance and also the first part of its track were accurately observed through large tree-tops*. Prof. Kirkwood gives the following angles as the result of careful measurement with a surveyor's transit:—

"First appearance, east, alt. 50°. Disappearance, S. 13° E., alt. 13°." Prof. Kirkwood remarks:—"The first appearance is, I am satisfied, quite exact and entirely reliable. I am not so confident of the observation at disappearance. The meteor exploded when about S. 33° E."*

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* A member of the Junior Class in Wooster University was with Prof. Kirkwood.

According to the observations at Wooster and Anderson, the meteor became visible at a height of 72 miles over a point in Columbiana County, Ohio, lat. 40° 50′, long. 3° 40′ W. The Wooster and Washington observations seem incompatible; the latter, however, make no claim to strict accuracy. As the explosion seen at Wooster was not observed either at Washington or Anderson, it is probable that at these greater distances the disappearance was simultaneous with the separation into fragments. Such doubtless has been the case in several other instances. For example, the fragments of the meteor of August 11, 1878, seen at Titusville, Pa., were invisible at Bloomington, Indiana. If we assume, then, that as seen from Washington the point of explosion was also that of disappearance, we find by a tentative process that the observations are approximately satisfied by supposing the separation to have taken place over Tuscarawas County, Ohio, about N. 80° W. from Washington, Pa., at a distance of 70 miles, and at a height of 17 or 18 miles above the earth's surface. After the explosion the parts remained visible at Wooster until within 12 or 13 miles of the earth. The course was nearly S.W.; the true length of the entire visible track as seen at Wooster was about 85 miles; that of its projection on the earth's surface about 60 miles. The inclination of the path to the surface of the earth was about 45°. The velocity, though uncertain, was probably greater than that corresponding to an elliptic orbit. No detonation was heard at any point of observation.

(8) 1879, January 28, 2^h 28^m, A.M., local time.—Observations at Princeton, Green Lake County, Wisconsin. Lat. 43° 50′ N., long. 12° 13′ W.—Rev. William M. Richards states that between 2 and 3 o'clock† on the morning of January 28 he was awakened by a sudden flash of intense light which he at first supposed to be lightning. It continued, however, for some seconds, and by the time he was thoroughly awake and ready to make observations the light had assumed a reddish tinge, somewhat resembling that of a Roman candle. He next supposed it to be a fire, but immediately found that if so, it must be out of the village. After making other conjectures, to be as quickly dismissed, he finally reached the conclusion that the light was meteoric.

"By that time," he says, "the frightful conflagration had settled down into a low pyramid of lurid light, the base extending 60° along the N.E. horizon, and the vertex having an altitude of 30°. . . . . The time of the meteor's flight is very uncertain; perhaps

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* Letter from Prof. S. J. Kirkwood.

† Mr. Richards did not notice the *exact* time. The Michigan observations give 2^h 28^m.

8 to 10 seconds. The brilliant and white light at first would indicate that the movement was from the west"*.

Observations at Traverse City, Michigan, lat. 44° 43′ *N., long.* 8° 40′ *W.*—The 'Grand Traverse Herald' of January 30, 1879, states that the accounts of this meteor by different observers were, in some respects, very conflicting, those who saw it being too much startled to observe it closely. "What *is* known is that it was an immense ball of fire, that the darkness was made light as noonday, and that a terrible explosion followed its disappearance. A night watchman who saw it explode says it flew into minute pieces like star dust. The one thing that all agree upon is the explosion. This was heard with equal clearness at Mayfield, 13 miles south of Traverse City, and at Williamsburg, 12 miles east. The effect was like that of an earthquake. Houses were jarred, windows shook, and dishes rattled upon the shelves. A swaying motion seemed to be given to the buildings as an upheaval and settling back. If the meteor had not been seen it would have been thought an earthquake-shock."

In response to a letter of inquiry, Thomas T. Bates, Esq., editor of the 'Grand Traverse Herald,' has given me the following additional statement:—

"A night watchman in our streets, an intelligent, cool-headed man, gives me in substance this: 'Was on watch; passing from due west to east, saw a great light: turned quickly and saw a ball of fire over my right shoulder; turned to left and watched it until it disappeared: when first seen it appeared about as high as ordinary rain-clouds; was on a down grade close to and apparently over the bay; came from S.W. and passed to N.E.; appeared to me larger than full moon; full moon looks to me to be 18 or 20 inches in diameter. Meteor appeared to pass me, and move out of sight at about the rate of speed a descending rocket has after its explosion. Had a good chance to see it plainly. Just after passing me a singular thing occurred—a ring of fire seemed to peel off the meteor itself, and this followed the ball of fire out of sight, but dropped a little behind it. It was perfectly distinct, and appeared to be hollow, for I could see a dark centre. Every thing was as light as day. I looked at my watch as it disappeared; it was just 28 minutes after 2 o'clock. I passed on my beat, and shortly the terrific explosion came; it shook and jarred every thing around. I immediately looked at my watch, and it was 32 minutes after 2.'

"This is his report as it was given the next day, and as it was repeated to me a few days ago. I have no idea that the meteor fell into Carp Lake†, or that even a portion of it fell there. Every thing points to the correctness of Mr. Smith's report which I send you.

"Truly yours,
"THOS. T. BATES."

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* Letter from Rev. Mr. Richards. See also the Sci. Am. of March 15, 1879.

† It was reported that a hole through the ice on Carp Lake had been discovered the next day, indicating that the aerolite had probably fallen into the lake.

Charlevoix, Charlevoix County, Michigan, lat. 45° 15′ N., long. 8° 12′ W.—Willard A. Smith, Esq., editor of the 'Charlevoix Sentinel,' states that the meteor was seen by several persons at Charlevoix (where it appeared to be at least four times as large as the full moon), that it burst almost exactly over the village, and that parts of it were seen to fall. The ground was covered with deep snow, which was disturbed in several spots by the meteorites, though, strangely enough, no fragments were found. The meteor before striking the earth lighted up surrounding objects with an intensity of brightness surpassing that of sunshine, and its explosion resembled the sound of musketry. Its direction was nearly from S.W. to N.E., and the interval was very brief between the explosion and report *.

Cheboygan, Michigan, lat. 45° 37′ N., long. 7° 31′ W.—Mr. and Mrs. Jacob Walton, of Chebogyan, both saw the meteoric light as it approached from the S.W. It lasted several seconds, and was so bright as to cast a very distinct shadow into the windows from the roof of the porch. From this shadow Mr. Walton estimated the greatest altitude of the meteor at about 45°. The explosion was not heard at Cheboygan †.

The preceding observations indicate that the meteor's course was approximately N.E. by N., and that it first became visible over a point not far from lat. 44° 25′ N., long. 9° 0′ W. The distance from Traverse City to the point at which meteoric matter is said to have fallen is about 42 miles. But the interval of four minutes between the observed explosion and the report corresponds to a distance of about 49 miles. This would make the point of explosion 26 miles above the earth's surface. The height at first appearance, if we can rely upon the rather uncertain estimate of Mr. Walton, at Cheboygan, must have been nearly 100 miles. The true length of the visible track was about 124 miles, and the length of its projection on the earth's surface 66 miles. The time of flight is very uncertain, though the observations indicate a rather slow motion.

(9) 1879, February 3, $11^h\ 30^m$, P.M., Indianapolis time.—This meteor is thus described by a correspondent of the Indianapolis 'Daily News' for February 7:—

"Raysville [Henry County], Indiana, February 4, 1879. Last night between 11 and 12 o'clock was seen, by a few citizens of this place, one of the largest and most brilliant meteors ever observed in this section. It was larger than the one seen in December 1876, but did not last so long, being but a few seconds in view. It apparently rose from the eastern horizon, and advanced rapidly, marking its path by a stream of flame until it had almost reached the zenith, when it exploded with a dull but plainly audible report. The different parts shot earthward in various directions, but the lights of all were extinguished before they had gone very far."

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* Condensed from a letter of Mr. Smith to Mr. Bates.

† Letter from C. S. Ramsay, Esq., to Mr. T. T. Bates.

(10) 1879, February 17, $8^h 0^m$, P.M., Chicago time.—This meteor was observed by C. A. Kenoston, Professor of Mathematics and Astronomy in Ripon College, Wisconsin. It was first seen in the west at an altitude of about 30°. It moved slowly along the line of the two southern stars in the square of Pegasus, and disappeared near the horizon without any audible explosion. It was very bright—increasingly so,—and seemed to have a short tail *.

(11) 1879, February 20, $10^h 45^m$, P.M.—This meteor was seen at Henryville, Clark County, Indiana, by Mr. Benjamin Vail. A brilliant train remained visible nearly a minute after the meteor itself had disappeared †.

(12) 1879, March 15, $3^h 53^m$, A.M.—This meteor was observed by a number of persons at Washington, Daviess County, Indiana. When first seen it was S. 10° W., at an altitude of 25°. It moved westward 20°, and then burst into many fragments. The meteor was of a pale bluish colour; but when it exploded it lighted up every thing almost like daylight.

A luminous cloud of smoke or vapour remained visible several minutes after the explosion. No sound was noticed ‡.

Remarks.—As already stated, no meteorites are known to have fallen from any of the fireballs in the preceding list, although in more than one case the disappearance was followed by loud detonations, and the explosion of the meteor of February 3 took place near the zenith of the principal point of observation. It is also remarkable that the only two whose velocities could be approximately determined were almost certainly moving in hyperbolic orbits. This last-mentioned fact is in harmony with the theory of Professor Von Neissl, who regards aerolitic and detonating meteors as a distinct class of cosmical bodies, differing both from comets and periodic star-showers in the original velocities with which they enter the sphere of the Sun's attraction §. But not only have certain comets moved in hyperbolas, but the computed velocities of at least a few bodies have undoubtedly indicated elliptic motion. This theory, therefore, can hardly be accepted without further confirmation.

Are meteoroids moving in hyperbolas to be regarded in general as fragments of disintegrated comets?—The discovery that the meteors of November 14, November 27, April 20, and August 10 are intimately connected with comets moving in the same orbits, has suggested that *all* shooting-stars and meteoric fireballs may have been produced by the gradual dissolution of comets or cometary clusters. It must be remembered, however, that the comets connected with these meteor-streams are all periodic, and that the dispersion of their matter is due to an indefinite number of returns to perihelion. In cases of non-periodicity, complete dissolution, as the result of a single perihelion passage, would be extremely improbable. We conclude, therefore, that the meteor of December 30, 1878, and others with hyperbolic orbits are *not* cometary fragments dissevered by solar influence.

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* Letter from Prof. Kenoston.

† Thus briefly referred to in a letter from Mr. Vail. No further description was given.

‡ Letter from Prof. D. E. Hunter.

§ See the Report on Luminous Meteors by a Committee of the Brit. Assoc. for the Adv. of Sci. for 1877.

That some fireballs explode noiselessly, while others, apparently no larger, produce loud detonations, is a remarkable fact not hitherto explained. The fact also that explosions very often occur without being followed by the fall of aerolites seems no less mysterious. Professor Newton has suggested that aerolites are probably furnished only by such meteors as penetrate the atmosphere with relatively slow motions, those moving with great velocities being burnt up or dissipated before reaching the earth's surface. Much, however, must evidently depend on the size and constitution of the meteoroids. Small meteors (shooting-stars) are entirely consumed in the atmosphere. The composition and structure of meteoric stones are very widely various. "While some are extremely hard, others are of such a nature as to be easily reducible to powder. It is not impossible that when some of the latter class explode in the atmosphere they may be completely pulverized, so that, reaching the earth in minute particles, they are never discovered"*. The fireballs of August 11 and December 30, 1878, as well as that of February 3, 1879, had very rapid motions; and we can perhaps best account for the non-appearance of aerolites on the theory of their complete disintegration†. The meteors, it is obvious, could not have escaped out of the atmosphere. Events of this kind are doubtless of very rare occurrence. We have, it is believed, no authenticated instance in which a fireball has escaped after approaching within 39 miles of the earth's surface‡. Assuming this as an inferior limit, and taking 100 miles as the greatest height at which such bodies become visible, it is easy to show that but one in thirty-four can continue its orbital motion.

Suggestion to Observers.—In the theory of meteors it is a matter of first importance to determine the form of their orbits. If any move in hyperbolas they must have had a proper motion in space before entering the solar system. Now the nature of a meteor's orbit is determined from its observed velocity. Unfortunately, hawever, the time of flight (on which the velocity depends) is generally a very uncertain element, the estimates of different observers being very discordant. Persons, therefore, who report such phenomena should train themselves to habits of exactness in measuring the time of visibility. DANIEL KIRKWOOD.

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* Met. Astr. p. 65.

† The average height of shooting-stars at extinction is about 55 miles; that of aerolitic fireballs and detonating meteors at the time of explosion about 25 miles.

‡ This was the nearest approach of the great meteor of July 20, 1860. See Prof. Coffin's memoir in the 'Smithsonian Contributions,' vol. xvi.

Description of a Three-feet Telescope.

THE completion of a silver-on-glass reflector of three feet aperture on a novel plan, both as regards mirror, support, and mounting [*], enables me to give a short description of it.

In settling the plan at the commencement, the whole question had to be considered. The want was a powerful telescope; the means as to time and money were limited. The reflector, on the score of cheapness of the raw material, and from the fact that increase of aperture gives increase of light, was at once decided on: what the material should be was not so easy to settle; metal was more certain than glass in manufacture, but not nearly so reflective as silver on glass. Glass, therefore, it must be; and many plans were thought of to get rid of the bugbear of bad annealing. It was suggested to have a back of well-burnt fireclay or earthenware, or even terra cottta, and forming it roughly to the size and shape of the disk, to flux on the face a thin sheet of commercial plate-glass and work that. But the doubt whether the expansion and contraction would be the same for the two materials, and the experimental nature of the idea, determined me to try glass, which, after one failure, answered perfectly.

There was then the very important question to settle of the proper support of this disk to avoid flexure, and to give the necessary edge support, that would allow the mirror to be used with any part of the edge uppermost.

A careful reading of all I could find on these matters was not very encouraging; but it was at last decided to follow the plan I had already used with an 18-inch, which was this:—For the edge support, thin copper bands, of about $1\frac{1}{8}$ by $\frac{3}{16}$ inch, and a little longer than $\frac{2}{3}$ of the circumference, and with the ends made round and screwed for nuts, were used, and the cell was provided with three blocks, cast on the edge at 120° apart, each with two slots. To get a good idea how these bands were used, it is only necessary to imagine the cell and mirror on edge, with one of these blocks at the bottom edge. The other two would be right and left near the top; and if one of the copper bands was then attached to the right-hand block and taken round the bottom edge of mirror to the left-hand one, a very good edge support of the mirror would be given *for the mirror in that position.* Then by putting in the other bands, one from the right-hand block, over the top edge of the mirror down to the bottom block, and the other from the left-hand block over the top of the mirror to the bottom block, there is a similar support to the mirror in other positions. A layer of cloth was placed between the bands and the glass. For the bottom support, the glass was credited with some rigidity and strength, and it was not thought necessary to use more than six points of support. These points were found by supposing the mirror to be divided into six sectors of 60°; the centre of gravity (considering it as of no thickness) of each triangular piece was found, and a circle described round this point, keeping within the sides. This gave the size of each of six round, gun-metal plates, which were faced flat, and turned out in the centre to form broad rings of support. This was done to prevent the plates presenting only one point to the glass. On the under part of these plates a knob was turned, fitting into the ends of three steel levers, long enough to connect the plates in couples, the centre of each lever resting in a proper place provided on the cell. The whole of the mirror by this arrangement rests on three points of the cell, and the bottom of the mirror itself rests on six large, flat plates, each of which takes its share of the total weight.

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[*] The principal points of which are contained in a paper in the 'Monthly Notices' of the R. A. S. for 1879, April.

The cell was made of cast iron, of moderate thickness, as the strength was not of much consequence, it having been arranged that the three points where the levers came down, bearing the weight of the mirror, should be exactly over the large adjusting-screws of the mounting, to insure permanence of adjustment. A gun-metal ring was bolted on to the top edge of the cell, at a point some ¾ of an inch below the surface of the mirror. This was necessary, as the plan for silvering would require the mirror to stand up for this distance from the permanent cell to allow a band to be fastened round, and also for the purpose of working at the mirror—for it was decided to work the mirror in the cell. This gun-metal ring was turned so as to take on tightly a large tin cover. This arrangement does not provide any means for preventing the mirror from falling forward, as this is provided by other means, so that the mirror is not put to any stress beyond the edge-nipping of the copper bands if screwed up too tightly—a fault easily to be avoided.

The mounting, following the conditions laid down in the paper already mentioned, consists of a massive polar axis of cast iron, floating, or partly floating, in mercury to relieve friction, and prolonged beyond the top bearing for some three feet. This supports a strong, square, base plate, on which rests the cell, and from the corners of which spring four steel tubes, which, with steel tension-rods, form the square framework that carries the diagonal mirror and eye end. The back, or other part of this base plate, hinged, as it were, to the top of the polar axis, carries the necessary counterpoise to the steel framework. A gun-metal driving-wheel, of 40 inches diameter, with 1440 teeth, is connected, by means of very short steel spindles, with the driving-clock, of a peculiar construction, and the necessary mechanism for the various motions is provided. The polar axis is attached to a large cast-iron block, which contains the clockwork, and spreads out with a broad flange to a base of about 5 ft. × 4 ft.

The foundation-pier is made hollow and some 8 feet deep, so as to form a well for the driving-clock weight.

The whole of the telescope is covered by an open framed house, running round and clear of the telescope, on an iron railway, the sides and roof being formed of sheet-iron. The roof slides clear off, so that practically the telescope is in the open air. Nothing gives such a good idea of the house as to consider it a slight modification of Herschel's altazimuth mounting to his four-feet, but with this exception, that the house is quite independent of the equatoreal. It has the same sloping side, up which slides a platform, in this case about 12 feet long, with a ladder at the side to reach it. This platform is capable of being raised or lowered, and gives a most delightful means of using the telescope; being counterpoised, and running on rollers with rackwork motion, it can be worked with the greatest ease and nicety.

A. Ainslie Common.

Sept. 1879.

Double Stars for October.

The great binaries and some other double stars have now been gone through. For want of space a good many interesting objects discovered by *h*, Σ, OΣ, and β (Burnham) were omitted, and these will now be taken up in order of R.A. As before, *h*=Sir John Herschel, Σ=Struve, OΣ=Otto Struve, and β=Burnham, De=Dembowski, Se=Secchi, C.O.=Cincinnati observers (Stone, Howe, &c.).

Σ 3063. R.A. $0^{\mathrm{h}}\ 1^{\mathrm{m}}{\cdot}5$, Dec. −5° 12′. Mag. 8·3, 10·2.

1865	224°·4	1″·84	De.
1877	223 ·3	1 ·82	β.

β 391. R.A. $0^{\mathrm{h}}\ 3^{\mathrm{m}}{\cdot}2$, Dec. −28° 39′. Mag. 6, 6.

1876	97°·2	0″·78	C.O.

OΣ 2. R.A. $0^{\mathrm{h}}\ 7^{\mathrm{m}}{\cdot}5$, Dec. +26° 20′. Mag. 7, 8, 10.

A B 1874	43°·8	0″·88	OΣ.

Binary? A C fixed.

Σ 13. R.A. $0^{\mathrm{h}}\ 9^{\mathrm{m}}{\cdot}4$, Dec. +76° 14′. Mag. 6·6, 7·1.

1875	97°·2	0″·58	De.

Orbital motion.

OΣ 4. R.A. $0^{\mathrm{h}}\ 10^{\mathrm{m}}{\cdot}4$, Dec. +35° 49′. Mag. 7·5, 7·8.

1873	173°·0	0″·35	De.

Orbital motion.

Σ 35. R.A. $0^h\,25^m{\cdot}5$, Dec. −2° 43′. Mag. 9·4, 9·6.

1868	267°·5	7″·88	De.
1878	266 ·6	8 ·62	β.

Measures wanted. Angle unchanged. Distance diminished.

Σ 51. R.A. $0^h\,37^m{\cdot}3$, Dec. +16° 42′. Mag. 8, 9·5.

1858	127°·6	4″·05	Se.

Probably binary. Measures wanted.

Σ 118. R.A. $1^h\,20^m{\cdot}6$, Dec. +82° 44′. Mag. 8·5, 9·4.

1863	69°·9	11″·18	De.

Measures wanted.

β 506. R.A. $1^h\,25^m{\cdot}1$, Dec. +14° 44′. Mag. 4, 11.

1878	12°·9	1″·02	β.

β 510. R.A. $1^h\,42^m{\cdot}1$, Dec. +15° 43′. Mag. A 8, B 12, C 8.

A B	1878	337°·4	1″·59	β.
A C	78	326 ·4	53 ·56	„

Probable change in A C.

β 511. R.A. $1^h\,42^m{\cdot}6$, Dec. −2° 1′. Mag. A 8·5, B 8·5, C 12.

A B	1878	159°·2	29″·67	β.
B C	78	316 ·0	3 ·69	„

Certain change in A B (Σ 171).

JOSEPH GLEDHILL.

Mr. Edward Crossley's Observatory,
Bermerside, Halifax.

Meteor Notes for October.

THE Orionids should be looked for between the 14th and 22nd. There are a large number of contemporary showers visible, and meteors are very frequent in the mornings of this month, especially at the epoch of the Orionids. In 1869 the horary numbers A.M. observed by Major Tupman were:—14th, 38; 15th, 52; 16th, 67; 17th, 81. The writer also observed many meteors on the morning of the 16th in 1877. The chief showers of the month are as follows:—

A.M. 103° +12° (15th–16th), 105° +50°, 115° +29°, 133° +79° (2nd–5th), 133° +21° (16th), 130° +47°, 140° +28° (15th–18th), 120° +15°, 121° ±0°, 100° +1°, 150° +13° (11th).

P.M. 60° +20°, 43° +22°, 81° +23°, 62° +47°, 33° +51°, 62° +37°, 40° +40°, 25° +44°, 290° +70°, 42° +31°, and 28° +10° (13th).

These showers may nearly all be reobserved, during the absence of moonlight, from about the 9th to the 23rd.

Fireballs should be looked for on the 13th, 15th, 17th–18th, 22nd, 24th, and 29th.

For further notes on October showers see 'Observatory,' Vol. i. p. 188 and Vol. ii. pp. 203–205.

Recent Observations.—On Aug. 9–12 the following numbers of meteors were counted at Debenham, Suffolk:—

	h m h m	m	Meteors.		
Aug. 9...	10 30 to 11 30	60	16	Much cloud.	J. G. Cornish.
10...	10 30 to 11 5	35	12	Much cloud.	J. G. Cornish.
11...	10 0 to 10 30	30	25	Fine night.	Serg. Payne.
11...	10 33 to 11 3	30	38		V. Cornish.
11...	11 0 to 11 30	30	32		Serg. Payne.
12...	9 47 to 10 51	60	43	Pretty clear.	J. G. Cornish.

Mr. V. Cornish deduced the chief radiant of the Perseids at 47¼° +57½°. The table shows that on the 11th, 10^h to $11\frac{1}{2}^h$, 95 meteors were seen, =64 per hour for one observer, which, considering the low altitude of the Perseid radiant, represents a somewhat active return of the shower. A fireball, =¼ ☽, was seen by Mr. Payne on July 30, path from 342° −9° to 360° −3°, and probably from a centre at β Aquarii. A meteor, = ♀, was recorded by the writer as proceeding from the same point on July 30, 1878 (see 'Observatory,' Vol. ii. p. 165), and the catalogues of different observers give other instances indicating a radiant at 324° −12° for July 26–31 (8 bolides), and =Tupman, 326° −13°, July 28, 1870.

In the moonlight on Aug. 4 several fine meteors were observed at Debenham chiefly between $9^h\ 55^m$ and $11^h\ 5^m$, and on the subsequent nights when observations were continued several extremely foreshortened paths were registered:—

	h m		°	°		°	°	
Aug. 4...	10 40	>1	291½	+ 6	to	291	+ 2	=Vega.
4...	11 5	<♃	231	+20	to	229	+14	
4...	14 0	=♃	164	+62	to	197	+40	
10...	10 38	=♃	70½	+45½	to	72	+42	
11...	11 17	6	330	+55	Path 20′.			Lacertid.
12...	11 29	4	40	+60½	Path 20′.			Perseid.
12...	12 30	♀	53	+17	to below ♄.			
17...	10 10	6	63	+37½	Path 20′.			62° +35°, Aug. 1879, D.
17...	......	5½	261	+48	Path 10′.			266° +47°, Aug. 1879, D.
25...	9 55	6	10½	+39½	Path 8′.			Andromedee.

In the 'Gazetta Piemontese' of Aug. 27 and 28 Signor P. F. Denza gives some results of observations made at Italian stations on Aug. 9–12, 1879. The number of meteors seen was as follows:—

Observer and station.	August 1879.				Totals	
	9th.	10th.	11th.	12th.	seen.	mapped.
G. Ballatore, Fossano	6	31	14	...	51	51
Prof. Parnisetti and four assistants, Alessandria	41	111	37	...	189	189
D. P. Maggi and two assistants, Volpeglino	32	181	304	159	676	270
G. Manzi and assistants, Piacenza	...	...	169	...	169	169
D. V. Capanni, Marola	...	30	...	...	30	
G. Grablovitz, Trieste	...	...	23	17	40	40

Thus the 11th appears to have been the most favourable date. The Italian observers also recorded 295 shooting-stars on 12 nights in July.

Dr. N. de Konkoly sends a list of 258 meteor-paths recorded at several stations (chiefly O'-Gyalla) in Hungary during the year 1878. They were noted on April 19–21, July 25–29, and Aug. 9–13. The value of these annual observations might perhaps be increased if the magnitudes and other details of each meteor were included in the tables.

At Bristol on the three nights Aug. 21, 22, and 23, 211 shooting-stars were observed by the writer in watches extending in the aggregate over 16¼ hours. There was a very rich and well-defined shower of 52 Draconids from a point in 291° +60°. 21 of them were recorded on the 21st, 11 on the 22nd, and 20 on the 23rd. On the latter night notes were retained of the number seen each hour as follows:—

Period of watching.	August 23, 1879. Meteors seen.	Draconids.	
9h 15m to 10h	8	4	Sky very clear all night.
10h to 11	11	3	
11 to 12	10	3	
12 to 13	12	4	
13 to 14	18	3	
14 to 15	14	3	
5¾ hours.	73	20	

The shower of Draconids is evidently one of special character. The meteors are rather slow and short, often leaving yellowish trains. The night of the 24th was overcast throughout; but on the 25th, before midnight, 14 meteors were observed in clear spaces between the storm-clouds, and of these 4 were Draconids; so that the duration of the radiant certainly extends over the five nights, Aug. 21–25. The following bright meteors were registered from this stream:—

	h	m		°	°		°	°	°	
Aug. 21...	11	23	1	338	+40	to	347	+31	11	Slow.
21...	11	48	1	283	+71½	to	275	+75	4½	Slow.
21...	12	10	1	11	+79	to	46	+77	8	Slow.
22...	11	42	1	289	+48	to	288½	+42	6	Slow.
22...	13	28	1	247	+58	to	233	+55	9	Slow.
22...	15	2	1	280	+51	to	274	+45	7	Swift.

	h	m		°	°		°	°	°	
Aug. 23...	9	18	=♃	340	+53	to	360	+43½	16	Slow; train.
23...	10	7	<♃	341	+33	to	347	+23	11	Slow; train 5 sec.
25...	8	30	Sirius.	309	+19	to	312	+ 5	14	Rather swift.
25...	9	57	=♀	79	+76	to	89	+67	9	Slow; train.

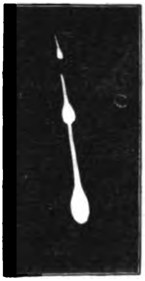
About 15 others were rather brilliant, =2nd mag. stars, and nearly all had trains. The shower is evidently the same as Mr. Greg's No. 78 at 282° +60°, June 28–Aug. 25, and he very properly ascribes to it considerable strength. It has been seen by many observers, notably by Major Tupman on Aug. 20–25, 1871, at 280° +58°. The observations this year on Aug. 21-23 also included two bright Cepheids:—

	h	m		°	°		°	°	
Aug. 21.........	12	46	>1	337	+36	to	337½	+29	Slow; train.
22.........	14	50	<♃	326	+30	to	325¾	+22	Slow; brilliant.

These, with several other meteors belonging to the same stream, give a radiant at 330° +69°, which agrees with the following previous determinations:—

	°	°	
Aug. 6–31......................	335	+67	Greg and Herschel.
Aug. 28, 1869	340	+65	Schiaparelli and Zezioli.
Aug. 25–Sept. 1, 1878......	335	+64	Sawyer (6 meteors).
July 6–Aug. 25, 1876–77...	330	+70	Denning(31 meteors).

The probable occurrence of two bright meteor-showers on Aug. 19–22 from Cepheus and Draco (339° +66° and 299° +66°) was pointed out by the writer in the 'Observatory' for September, p. 131, and the positions are both clearly confirmed by the recent observations, which also give several other well-marked radiants. About 10–15 swift meteors, with streaks, diverged from each of the points 46° +47°, 61° +50°, and 62° +35°, near α, μ, and ϵ Persei. At the latter place a stationary meteor =Sirius was seen on Aug. 22, 11^h 33^m, and a bright long-pathed meteor, leaving a streak of 30°, of the same stream, was noted on Aug. 25, 12^h, shooting from 37° +40° to 342° +32°; path =44°. On the 23rd there was a very exact radiant of 8 slow, faint meteors at 350° +47°, and about 12 were seen from 319° +30° on the 3 nights. A few brilliant meteors with streaks were traced from 24° +42°, and there were other centres at 40° +31°, 343° +14°, 266° +47°, 315° +47°, 350° ±0°, and 253° +64°. About 15 Aquariads were seen also; but the radiant was not accurately derived.



Of the few meteors recorded before midnight on the 25th, one which appeared at 9^h 57^m was a small bolide, and at the end-point gave a brilliant flash (notwithstanding the moonlight), which many persons mistook for lightning. In traversing its nearly vertical path of 9° the meteor produced three outbursts, and left very short brightish streaks in the positions of its two latter scintillations.

W. F. Denning.

CORRESPONDENCE.

To the Editor of 'The Observatory.'

The Eclipse of 1879, *July* 18.

SIR,—

The August number of your review, 'The Observatory,' which I have just received, contains a note by Mr. Arcimis giving the times of the beginning and end of the Solar eclipse of July 18 as observed by him at Cadiz, and compared with those given in the 'Nautical Almanac' published by this Observatory for the use of the Spanish navy.

The same eclipse was observed here by four persons using different chronometers, which every one compared with the standard sidereal clock before and after the observation of each contact, the error and rate of the clock being obtained, as usual, by transits of stars observed with the meridian-circle. The means of the times of the contacts by the four observers were as follows:—

	h	m	s	
Beginning..	18	17	15·8	Sid. M.T. at San Fernando.
End	19	58	16·0	

Mr. Arcimis's place of observation at Cadiz is 4′ N. and 21^{s}·6 W. of this Observatory; the difference of the effect of parallax between the two stations being therefore nearly +16^{s} for the beginning, and that of the longitude −21^{s}·6, it is easy to see that at least the time of the first contact as observed by Mr. Arcimis is wrong.

Errors of the same kind in observations of occultations by the same gentleman have been detected here more than once.

With respect to the difference between the observations of Mr. Arcimis and the data in the 'Nautical Almanac,' I think it is sufficient to say that the times of the beginning and end of eclipses given in the said work are intended only to prepare for actual observation; and this being the object, the beginnings of Solar eclipses are calculated to a few seconds; but not so the end. This circumstance, the tabular errors, and those of the observation by Mr. Arcimis, are sufficient to explain the discrepancy between his observations and the data in the 'Nautical Almanac.'

I have the honour to be, Sir, yours most obediently,

CECILIO PUJAZON.

Instituto y Observatorio de Marina de
San Fernando, 1879, August 8.

The Red Spot on Jupiter.

SIR,—

Several numbers of your journal, as also several of the 'Astronomical Register,' have contained communications on the remarkable rose-tinted spot on Jupiter, to which I called attention in the 'Observatory' for January last. It is gratifying to know of the interest so many widely separated observers have manifested in this phenomenon.

I wish to state that I recommenced observing this spot on July 2, 1879, and since that date have made upon it about thirty observations. Quite a number of drawings have also been made when it was on different parts of the disk, and particularly when decided changes have been noted in its environment.

As I will presently show, it is now situated *very nearly* on the same part of the planet's surface as it was fourteen months ago. Its centre is very near the 251st meridian, and also near the parallel of 40° south of the Jovian equator. The only appreciable difference in its appearance now and last year is the very perceptible elongation of the major axis of the oval and the shortening of the minor. The elliptical form is still distinctly preserved, except that the *preceding* end is slightly more acute than the following. The longer axis is precisely parallel to the equator of Jupiter. For the last fifty-six days it has preserved a very uniform appearance, with slight exceptions, which will appear in the notes. From a large number of micrometer measures I find the mean major axis to be $14''\cdot56$, and the minor $3''\cdot85$. Its colour has remained very persistent, whether observed at night or in full daylight.

As Mr. Dennett has given in the 'Astronomical Register' for June a table of observations made between July 9 and Nov. 23 of last year, I will simply continue his table, adding a margin for notes. The assigned meridians are derived from the ephemeris of Mr. Marth, as given in the 'Monthly Notices' for April. All the times are given only to the *nearest* minute. I may add that I am now making a series of very careful observations of the times of transit of the centre across the central meridian of the disk, relatively to a redetermination of the axial rotation.

Date.	G.M.T.	Meridian at centre.	Notes.
1879.	h m	°	
July 2...	22 21	233	The rose-tinted elliptical cloud seen July 9, 1878, far advanced on the eastern disk. The major axis about ½ *longer* than last year, and the minor *shortened*. Colour much deeper than that of the belts. Drawing made at $16^h\ 10^m$ Glasgow M.T. = $22^h\ 21^m$ G.M.T.
" 4...	22 46	190	The spot fully on eastern disk. It was watched till after sunrise, $16^h\ 55^m$. As the sunlight grew stronger its deep pink colour showed more strikingly. In the advancing daylight the broad equatoreal belts seemed brown bands, while the rose-colour of the spot maintained its ascendency.
" 5...	19 46	232	Preceding end near central meridian.
" 6...	17 21	295	Slight haze. Spot appears paler than usual. Preceding end near western limb.

Date.	G.M.T.	Meridian at centre.	Notes.
1879.	h m	°	
July 7...	1 56	246	Centre of spot nearly up to central meridian. At this time of day (19^{h} 45^{m} G.M.T.) the colour is scarcely distinguishable from that of the belts. At *times*, however, the red hue is *conspicuous*.
„ 8...	18 36	282	Following end past central meridian.
„ 9...	22 34	216	On eastern part of the disk. Measured the axis. Belts *brown*, *sharp*, and *ridged*. Drawing made. Observed till long after daylight. Red tint continued very strong.
„ 10...	19 16	247	The centre near central meridian. The equatoreal belt north of it very *dense*, *brown*, and *ridged*. Micrometer measures and drawing made.
„ 18...	17 41	315	At 11^{h} 3^{m} G.M.T. the preceding end very nearly in contact with western limb. Belts very sharp and unusually white. Appearance very similar to ridges of white clouds. Along the centre of the two equatoreal belts ran a fine purple line. The spot had a *border* of white. Appearance very peculiar. Drawing made.
„ 22...	18 31	228	Preceding end near central meridian.
„ 24...	21 26	275	East end past central meridian.
„ 27...	18 38	266	Centre past central meridian.
„ 29...	18 1	185	On eastern disk. By the eye the spot was suspected to have *elongated*. The suspicion confirmed by micrometer measures.
„ 31...	23 19	319	Preceding end near western limb.
Aug. 1...	17 1	240	Centre nearly up to central meridian.
„ 4...	15 58·5	295	Between centre and western limb.
„ 6...	16 24·6	252	Very near central meridian.
„ 7...	22 11	253	„ „ „
„ 8...	16 18	190	On eastern disk. Had been visible 25 minutes.
„ 8...	18 0	251·5	Very near central meridian. Nearly all the cloudy masses called belts seem collected south of the equator except the broad and distinct northern equatoreal belt. The few fine dark streaks left much resemble the dusky bands on Saturn. The southern hemisphere *dense* with clouds except in the vicinity of the spot; here the cloud masses seem not to approach.
„ 11...	15 41	260	Centre past central meridian.
„ 13...	15 37	199	Advanced on eastern disk.
„ 15...	18 39	250	Very near central meridian.
„ 16...	14 33·8	252	„ „ „
„ 18...	16 11	253	„ „ „
„ 20...	17 48	253	„ „ „
„ 22...	19 25	253	„ „ „
„ 24...	21 0·3	252	Centre very near central meridian. For the first time *very fine* short streaks were observed extending out from the ends parallel to the equator. I fear the spot is about to merge into the belts; if so we can make but few more transit observations of the centre for determination of axial rotation.

Date.	G.M.T.	Meridian at centre.	Notes.
1879.	h m	°	
Aug. 25...	16 51·5	252	Centre very near central meridian. No indication of the projecting streaks from ends to-night. The difference in the *illumination* of the northern and southern hemispheres of the planet very marked. The dusky rings on the northern hemisphere more than ever like those of Saturn. Not seen as straight lines on a *flat surface*, but as parallel circles on a *sphere*.

When I read Mr. Dennett's notes in the February number of the 'Observatory,' it occurred to me at once that it was very strange I had failed to see this spot during the months of August, September, October, and November of last year. That I failed to see it between the dates of July 18 and August 12 was a matter of course, since I was absent from the observatory with the eclipse expedition of Prof. Eastman. But between the dates of August 15 and Nov. 18 I made thirty-five carefully-noted observations of the eclipses, occultations, transits, &c. of Jupiter's satellites; and besides on eighteen different nights between August 18 and Nov. 23 I accommodated seventy-five persons with views of Jupiter. The case seemed to present such a reversal of the law of chances that I have taken the pains to compare the times at which this spot was due at the central meridian with all my observations of last year. Before I give the result of this comparison it is important to consider whether the spot is now on the same part of the surface of the planet as it was in July 1878. At my observation, July 9, 1878, made at $18^h\ 36^m$ G.M.T., the meridian at the centre of the disk was 222°. At that time the centre of the spot was between 40 and 50 minutes of time east of the central meridian. Having now watched this spot on *numerous* occasions, and having carefully noted the time it requires to transit the central meridian of the disk, I am sure that 45 minutes is a *reasonable estimate* of its distance eastward on that night. But $45^m = 25^\circ\!\cdot\!4$ of axial rotation, and $222^\circ + 25^\circ\!\cdot\!4 = 247^\circ\!\cdot\!4$, which is its position *now* within the limits of probable error of observation. It is then certain that *last* year, as *this*, the centre of the spot preceded the zero meridian about *three hours*. In the following table I give:—*first*, the date; *second*, the time the spot was due at the centre of the disk; *third*, my observed time; *fourth*, character of the observation; and *fifth*, the *difference of times*.

Date.	Spot near centre. G.M.T.	Observation. G.M.T.	Phenomenon observed.	Diff.
1878.	h m	h m		h m
July 10	15 15	16 11	Search for spot.	0 56
Aug. 15	14 32	15 46	I. Tr. Ing.	1 14
" 31	17 34	16 55	I. Sh. Eg.	0 39
Sept. 10	15 45	16 48	III. Ec. Re.	1 3

Date.	Spot near centre. G.M.T.	Observation. G.M.T.	Phenomenon observed.	Diff.
1878.	h m	h m		h m
Sept. 20	13 56	14 11·5	II. Ec. Re.	0 15·5
„ 22	15 34	16 33	I. Oc. Dis.	0 59
Oct. 2	13 46	13 34	I. Sh. Eg.	0 12
Nov. 2	14 13	13 5	I. Ec. Re.	1 8

I find by numerous trials that this spot is plainly visible in my telescope with a power of 275 for more than *two hours* before and after passing the middle point of its visible arc of rotation. Here then are at least eight different occasions on which I observed Jupiter *closely* when this spot *should have been seen*. In several of them it should have been near the *middle of the disk*. Even considering its longitude 240° (as Mr. Dennett has done), it could not *possibly* have passed out of sight by axial rotation. I cannot think it could be *present* and escape my attention unless its character had *wholly changed*, for any one who sees it *now* and as it *was* on 1878, July 9, a year ago, will at once detect it on any part of the disk. For instance, on the morning of July 2, 1879, as soon as my eye glanced on the disk I recognized it, though seen very *obliquely* to the line of sight, and seen *unexpectedly* after the lapse of a year. Now whether this spot has periods of "recurrence," as Mr. Trouvelot suggests, or whether at certain times it has a very rapid proper motion on the surface of the planet, I cannot pretend to determine. On July 9 of last year I saw it extend beyond a white spot north of it, but *which was in motion* is unknown. It is just to say that my note for July 15 of last year was not intended to refer to the spot, but only to the *disturbed condition* of the belt region. It is important, I think, to remember that both *preceding* and following the observation of July 9 the belt region was in a state of unusual agitation.

I am, Sir, yours truly,
C. W. Pritchett.

Morrison Observatory,
Glasgow, Missouri, 1879, August.

"A startling Astronomical Discovery."

Sir,—

In the 'Echo' of March 14, 1879, there appeared an article by Mr. R. A. Proctor under the above heading, and in it my name was freely used. I ask space for a few words in reply to the groundless charges made against me in that article; at the same time I feel that I do so at a disadvantage, because what I have to say cannot be published probably for five or six months after the charges were made.

With the tone of Mr. Proctor's article I have nothing to do; he no doubt has a good reason for writing thus about astronomical matters; but when he deliberately puts a construction upon what I have written, which the words were not intended to convey, it is perhaps worth while to say a few words in reply.

The words in question are:—"One could hardly resist the conviction that it was a shadow: yet it could not be the shadow of any known body; and if produced by a comet, it must be one of more than ordinary density, although dark bodies have been seen crossing the Sun which were doubtless comets. No change in the position of the shade could be detected after three hours' watching"*.

These words, I think, clearly express, and they certainly were intended to convey the idea, that I had effectually resisted the conviction that what I saw was a shadow, and that I knew it could not be the shadow of any known body; and to place this beyond doubt I added the words, "No change in the position of the shade could be detected after three hours' watching." Yet Mr. Proctor, in an absurd argument, endeavours to prove that I thought this dark shade on the Moon was the shadow of some body nearly as large as the Moon; and, in the effort to make the facts suit his shallow explanation, he even goes so far as to tell me that I exaggerated what I did see, although he had no means, except from my description, of knowing what I saw, and was for that reason incompetent to correct me. One would have expected a writer who has had so much to do with the observations of other people, and who knows the danger of twisting them to suit a theory, to hesitate before doing so in this instance.

How Mr. Proctor could venture to charge me with ignorance of the commonplace fact he offers as the explanation of the dark shade, I am at a loss to conceive, unless it was because the article had to be written in keeping with its heading, and the probability amounted almost to a certainty that any such statement published in an evening paper in London would never be seen by me in this out-of-the-way place, and therefore would not be contradicted.

In order to clear the way for any future attempt of his to explain the dark shade, I may here say that for nearly twenty years I have been familiar with the fact that there was an apparent shade over part of the old Moon, and with its appearance as seen through a telescope; and since 1864 I have known its effect upon the photographic plate, which is so well marked in every good set of lunar photographs.

Further, what I saw when on the mountains was darker and remarkably different from any thing of the sort I had seen before; and when I mentioned the fact as one of those observed at Woodford, I carefully avoided any attempt to explain the cause of what I saw, and I did not think it necessary to tell the readers of an astronomical paper that I knew there was a slight shade over part of the old Moon. It did, however, occur to me that the shade in question might vary in intensity, and that therefore what I saw should be recorded.

Mr. Proctor says "the phenomenon is one in which we terrestrials are very considerably interested, for what happens on the Moon in this way may at any time happen to the Earth."

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* 'Observatory,' No. 23, p. 375. "Notes of an Astronomical Experiment made on the Blue Mountains, near Sydney, N.S.W."

Precisely so; and I am sure he does not need to be reminded that such phenomena have happened on the Earth—witness the dry fogs of 1783, 1831, and others which might be mentioned, quite thick enough to hide all terrestrial features under them from an observer outside the Earth, and these remained stationary not for hours only but for weeks in spite of strong winds. Whether they would all have appeared from outside the earth dark as a shadow is doubtful, for they may have reflected light as clouds do; but it is certain they were thick enough to hide all terrestrial features under them, and in some cases there seems to be no doubt that they were like dark shadows over parts of the Earth.

I am, Sir, yours faithfully,
H. C. RUSSELL.

Sydney Observatory,
1879, July 17.

ζ Herculis.

SIR,—

Would one of your readers kindly inform me as regards this well-known double:—

(1) Whether any attempt has been made to obtain its parallax?

(2) Whether spectroscopic observations have established any proper motion; and, if so, whether any difference has been detected in the line-displacement of the two component stars?

I am, Sir, yours faithfully,
J. EDMUND CLARK.

York, 1879, Sept. 22.

[From spectroscopic observations of ζ Herculis recently made at Greenwich on two nights, the motion in the line of sight would appear to be about 25 miles a second receding. This is to be taken as referring to the brighter star, the *comes* being too faint to bear the dispersive power required to determine the motion.—ED.]

NOTES.

THE ASTRO-PHYSICAL OBSERVATORY AT POTSDAM.—The Astronomische Gesellschaft has just concluded a very successful meeting (the 8th) at Berlin. Professor Krüger presided, and about sixty astronomers from Sweden, Norway, Holland, Russia, England, and Germany were present. Many thanks are due to the resident astronomer at Berlin, Professor Förster, for his untiring energy. Professor Krüger has been re-elected president, and the meeting for 1881, it is settled, shall take place at Strassburg.

One interesting event in connection with the meeting just over was what may be regarded as the formal opening of the new observatory at Potsdam. This observatory has been specially designed and fitted up for the study of astronomical physics, and is wonderfully perfect in its arrangements.

Situated at a height of about 320 feet above the sea-level, on the top of a pine-clad hill overlooking Potsdam and its picturesque surroundings, the observatory commands a magnificently uninterrupted view on all sides. No smoke there. No disturbing vibrations. No noise and clamour and traffic of streets. There there are clear heavens. There there is great calm. There there are the songs of the morning stars. The observatory has been built by the Government, and every thing has been done to render it as efficient for work as possible.

Prof. Vogel and his colleagues are provided with charming homes in the observatory grounds; passing these, we reach the observatory itself. It is a rather low, flat-roofed, extended building of somewhat oblong form with three domes—one larger one with a diameter of 33 feet; two smaller ones, each with a diameter of 23 feet. The material employed in the structure is brick of various colours relieved with bands of encaustic tiling. The general exterior effect is most artistic; and, indeed, it may be remarked here that throughout the interior as well the artistic has never been lost sight of.

And this is as it should be; for surely it is a mistake to suppose that art and science are unrelated to each other. Science deals with laws; art is dependent upon laws; and all laws are linked in wondrous chain of interaction. The fundamental laws which shape celestial orbits have harmonics in the right proportions of arch or dome, or hall.

Prof. Vogel and his colleagues are each provided with separate studies and with separate laboratories; and these last are fitted most completely and conveniently for spectrum analysis, and for photography and chemistry. There are also a library, workshops, and a council-room. A 6-horsepower gas-engine in the basement is intended to drive a Siemens's dynamo machine.

In the large dome is erected a new refractor—the objective, of 11·8 inches aperture, being the work of Schröder, while the beautifully-finished mounting comes from the workshops of Repsold.

In one of the smaller domes there is an 8-inch refractor by Grubb, and in the other a 6-inch refractor by Steinheil.

These few lines sketch in a general way the principal features of the new Potsdam observatory; but it is not easy to convey, without entering into very minute details, an idea of the elaborate completeness of all its various parts.

It is told in northern story that Thor crossed to the cloud-walled palace of Odin Allvater by a rainbow. In this lovely myth may we not read something of the power of the spectrum as a bridge to the unknown? We cannot wish the Potsdam astronomers better than that the deeper spirit of this myth may rest upon them, and that, crossing again and again by this bridge, they may bring treasures from those dimly seen lands which stretch so infinitely around our little and partial knowledge. M. L. H.

Berlin, 1879, Sept.

THE FORT-WORTH ECLIPSE EXPEDITION, 1878.—The party which observed the Eclipse of 1878, July 29, at Fort Worth, Texas, consisted of Messrs. Leonard Waldo and Willson, of Harvard College Observatory, Prof. Rees, Mr. Pulsifer, and Mr. Seagrave, besides local volunteer observers. The Report of their observations is now published under Mr. Leonard Waldo's editorship. Their instruments comprised two 5-inch refractors, a 4-inch, two 3-inch, and smaller telescopes; a Browning spectroscope with dispersive power of ten prisms, a direct-vision spectroscope, and a spectroscope with two large prisms; transit instruments and chronometers; three photographic cameras, polariscopes, &c.

Eight sketches of the corona were made by as many different persons, and four of these, which have been selected for publication together with Mr. Willson's and Prof. Lockett's drawings, present considerable diversity in detail, though they agree in showing the outer corona to be much elongated along the ecliptic, and narrower than the Sun's diameter in the transverse direction. The inner corona was seen as an annulus all round the Sun, three times as bright as the wings or rays of the outer corona.

Mr. Willson, observing with a 3-inch telescope, represents a more complex outline, with two rounded masses on the southern side reaching 13′ from the Moon's limb, and much brighter than the wings of the outer corona, extending in the transverse direction to 45′ or more from the limb. Mr. Waldo considers Prof. Lockett's drawing to be a capital representation of the corona as he saw it himself with the naked eye. The four contacts and the duration of totality were noted by several observers, and a large portion of the Report is devoted to the determination of the longitude and latitude of the station. Passing over this, we come to the reports of the several observers.

Mr. Leonard Waldo superintended the photography, assisted by a local photographer. Three cameras were employed, but they were none of them provided with equatoreal motion, and had merely a rough movement in zenith-distance, without any in azimuth, so that the details on the photographs are to a great extent obliterated by the Sun's motion. In the first camera the back-combination of a Dallmeyer lens was used, having a focal length of 48 inches, and with this three photographs were taken with exposures of 60^s, 65^s, and 17^s respectively, showing the corona extending to a distance of 25′ from the Sun's limb, at about pos.-angle 120° and 300°. (The pos.-angle of the ecliptic was about 105° and 285°.) A number of striæ also are shown, which represent either prominences or a greater intensity of coronal light, and Mr. Waldo has taken much pains to determine their approximate position-angles. Owing to the absence of any indication of the four cardinal points on the published photograph it is not easy to identify these; but the matter is of less importance as prominences can be mapped with much greater accuracy by ordinary spectroscopic observation without an eclipse. It would seem, after making allowance for the elonga tion caused by the Sun's motion, that the greatest extension of the corona as photographed was along the ecliptic. The other two cameras had portrait-lenses with bi-refracting prisms placed between the lenses, so that in the one the two images were in a nearly horizontal line, and in the other in a nearly vertical line. A photograph was taken with each, the exposure being 145ˢ. One of these is badly defined and overexposed, and none of the differences in the two images can with certainty be ascribed to polarization; but the other, with less separation of the images, is fairly sharp; and, from an examination of the two images, Prof. Pickering concludes that tangential polarization of the light of the corona is shown. From this conclusion, however, Dr. Hastings dissents, and thus the question remains unsettled. In the record of his eye observations, Mr. Waldo notes that he saw flitting shadows moving swiftly towards the east and south-east. These might be about 100 feet apart, occurring three in a second. Also after second contact he records a minute point of light separated from, but in a line with, a prolongation of one of the Sun's cusps. He adds, "I do not know whether this is a phenomenon similar to that which produces 'Baily's beads' or not."

Mr. Willson observed with a 3-inch by Secretan, using an inverting eyepiece, power 45, in which was a glass plate ruled with concentric circles 6′ apart, divided into arcs of 45° by radiating lines. At first he observed the corona with a red shade, which he had forgotton to remove. The shade absorbs all rays beyond 1350 of Kirchhoff's scale and only transmits 0·4 per cent. of the rays at D, 2·1 per cent. of those at C, 9·5 per cent. of those at B, and 17·6 per cent. of those at A. With it the Moon appeared surrounded by a faint glow, perfectly uniform in structure, and diminishing in brightness rapidly and regularly outwards to a pretty well-defined limit about 4′ or 5′ from the limbs. On removing the shade, 15ˢ after commencement of totality, the light of the corona was found to vary considerably in different parts, and its outlines were well-defined, but very irregular. On the south side were two rounded masses of pale rosy light like cumulus cloud, at pos.-angles 230° and 180° reckoned from the north pole of the Sun's axis, reaching about 13′ from the Moon's limb. On the opposite side was a bright band, with its edge parallel to a tangent at the N. pole. Mr. Willson found no trace of granulated or striated structure, and nothing whatever of the nature of streams or rays in these portions. In the direction of the solar equator there was a very much fainter light on each side, perhaps with a tinge of green, conveying a very decided impression that it was of a totally different character from the other portions of the corona. On the preceding side this light was traced certainly more than 45′ from the limb. The prominences seen were of a pale pink and not of a ruby colour as Mr. Willson had expected.

Professor Rees, observing with a 5-inch, saw faint dots of light almost like faculæ at each cusp before totality. The corona was silver-grey and wing-shaped, the wings lying in the ecliptic. The wing on the preceding side was indented at 40′ from the limb, on the following side it extended further. The light of the wings appeared made up of rays parallel to the ecliptic.

In the two-prism spectroscope a continuous spectrum without bright lines was seen, with a slit ·025 mm. in width. On opening the slit to ·127 mm. the same appearance was presented, except that dark lines (C, D, and others) were noticed.

Mr. Pulsifer used his Browning ten-prism spectroscope mounted on a 4-inch Alvan Clark refractor, and with the slit tangential observed the reversal of the Fraunhofer lines at the commencement of totality. From the length of these lines, which only reach one third across the spectrum, the tangential thickness of the reversing layer was found; and from this Mr. Pulsifer infers its minimum height above the Sun's photosphere to be 524 miles. The C line was not shortened like the others, but extended right across the spectrum.

Mr. Seagrave noted one bright line quite faint and not very well-defined in the spectrum of the corona, making use of a ten-prism Browning spectroscope, mounted on a 5-inch refractor. Without the spectroscope he saw the inner corona form on the *n. p.* side of the Sun about 30ˢ *before totality*.

The Report of the Fort-Worth Expedition is a most valuable contribution to our knowledge of Solar Physics, and testifies to the energy and ability of all who took part in the observations. Perhaps the results arrived at would have been more readily appreciated if the drawings had all been carefully oriented in the same way, and the position of the Sun's axis had been marked on each. As is usual in eclipse reports, there is a good deal of confusion in this respect, and the position-angles are given with respect to the vertex, without reference to the ecliptic or solar equator. For the convenience of the reader, we have converted the angles as given into position-angles reckoned from the N. pole of the Sun's axis in the direction *n f s p*.

THE UNITED-STATES NAVAL OBSERVATORY, Washington, will gratefully receive for its library *separate copies* or *reprints* of memoirs published in the Transactions of Societies or in journals. The volumes of Transactions are regularly received, but often many months after the appearance of the reprints of particular papers, which are therefore especially valued. It is also requested that all communications of this nature, and correspondence relating to them, may be addressed to "The Library, U.S. Naval Observatory, Washington, U.S.A."

Agents of the Smithsonian Institution abroad will receive large parcels for transmission. The smaller ones will be received more quickly if they are sent by mail.

As far as possible, the publications of the Observatory will be distributed to all working astronomers.

JOHN RODGERS, Rear-Admiral, U.S.N., Superintendent.

Washington, Aug. 18, 1879.

M. Struve, who has just returned from a visit to America to inspect the 26-inch Washington refractor, has, we understand, authorized Messrs. Alvan Clark, the makers of that instrument, to supply a 30-inch object-glass for the Pulkowa Observatory, for a sum of 32,000 dollars; and Mr. Alvan G. Clark has come over to England to make arrangements for procuring the disks of glass.

Two new Minor Planets, Nos. 202 and 203, were discovered by Prof. Peters at Clinton, New York, on Sept. 21 and Sept. 25 respectively. Editor.

Elements and Ephemeris of Palisa's Comet, 1879 *d*[*].

By S. C. Chandler, Jun.

The comet whose discovery by Palisa was telegraphically announced on the 23rd of August was easily found on the evening of that day with a telescope of 3 inches aperture. It appeared as a round bright disk, about two or three minutes of arc in diameter.

From observations obtained on several evenings, as yet only provisionally reduced, I have calculated the following elements of the orbit:—

Perihelion Passage, 1879, October 4·351. Washington M.T.

	° ′	
Long. Perihelion,	202 23·6	Mean Eq. 1879·0
Long. Node,	87 12·0	
Inclination,	77 4·9	

Log. Perihelion Distance, 9·99697.
Motion, Direct.

The following finding ephemeris will be convenient for picking up the comet:—

Ephemeris for Washington, Midnight.

1879.	R.A. h	m	s	Dec. °	′	Log. *r*.	Log. Δ.	Light.
Sept. 30 ..	14	11	12	+24	20	9·9979	·2012	·400
Oct. 2 ..		20	20	22	18			
4 ..		29	6	20	16	9·9970	·2063	·392
8 ..		45	35	16	12	9·9981	·2134	·378
16 ..	15	15	8	+ 8	24	0·0076	·2335	·330
Nov. 1 ..	16	4		− 7	0	0·039	·283	·227

The comet will be nearest the earth about the 24th of September, and will be brightest about the 30th. It is now easily visible in a very small telescope and will continue so for several weeks to come.

The Satellites of Mars.

The following are the Greenwich Mean Times of elongation E. or W. of the outer satellite Deimos, derived from the data given in

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[*] From the 'Science Observer,' 1879, Sept.

Prof. Asaph Hall's memoir (*cf.* 'Observatory,' Vol. ii. No. 19, p. 230):—

Elongations:—Pos.-angle 53° or 233°. Distance 67″.

	G. M. T. h.		G. M. T. h.
Oct. 10	14·9 *p*	Oct. 21	8·4 *f*
11	6·0 *f*	22	14·7 *f*
12	12·3 *f*	23	5·9 *p*
13	18·6 *f*	24	12·2 *p*
14	9·8 *p*	25	18·5 *p*
15	16·1 *p*	26	9·6 *f*
16	7·2 *f*	27	15·9 *f*
17	13·5 *f*	28	7·1 *p*
18	19·8 *f*	29	13·4 *p*
19	11·0 *p*	30	19·7 *p*
20	17·3 *p*	31	10·8 *f*

The satellite revolves in $1^{d}\ 6^{h}{\cdot}30$; thus the elongation *p* occurs at $21^{h}{\cdot}15$ on Oct. 11, $15^{h}{\cdot}15$ after the elongation *f*, and similarly in other cases. The semi-minor axis of the apparent ellipse is about $13''{\cdot}5$; and the semi-diameter of the planet being about $11''{\cdot}5$, the satellite passes 2″ N. of the limb at inferior conjunction, and 2″ S. at superior conjunction, moving in the direction *n p s f*, since the S. pole of Mars is turned towards us. [Pos.-angle of Axis of Mars 143°, Lat. of centre of Disk 12° S. on Oct. 31, according to Mr. Marth's ephemeris, 'Monthly Notices,' 1879, June.]

On Sept. 21, about 15^{h}, three weeks before the date when Prof. Hall expected it to be visible in the Washington 26-inch refractor, Mr. Common picked up the outer Satellite with his new 3-feet silver-on-glass reflector, of which he has given a description in the present Number. At $15^{h}\ 43^{m}$ the pos.-angle was 237° 27′ or 4° from elong. *p*, which would therefore fall about $17^{h}{\cdot}7$, whereas Prof. Hall's ephemeris would place it at about $16^{h}{\cdot}4$. According to Mr. Common's observation, therefore, $1^{h}{\cdot}3$ should be added to the times of elongation given above. Prof. Hall had estimated the prob. error of his ephemeris at $2^{\circ}{\cdot}6$ of longitude, corresponding to 13^{m} in the times of elongations &c.

With regard to the inner satellite Phobos, the prob. error is so great, viz. $36^{\circ}{\cdot}5$ in the longitude, that an ephemeris could not be relied upon even for finding the satellite.

*Satellites of Saturn**.

LONGITUDES of the five inner satellites reckoned from superior conjunction with the planet's centre:—

Greenwich Noon.	Mimas.	Encel.	Tethys.	Dione.	Rhea.
	°	°	°	°	°
Oct. 2	69·4	181·3	245·6	193·2	98·5
7	179·6	55·3	119·4	131·2	137·3

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* From an Ephemeris by A. Marth, Astron. Nachr. No. 2273.

Greenwich Noon.	Mimas.	Encel.	Tethys.	Dione.	Rhea.
	°	°	°	°	°
Oct. 12	289·9	289·3	353·3	69·2	176·1
17	40·1	163·3	227·1	7·2	214·9
22	150·3	37·3	100·9	305·2	253·7
27	260·5	271·2	334·7	243·2	292·4
Nov. 1	10·6	145·1	208·5	181·2	331·2
Motion in longitude					
for 1 day	382·0	262·8	190·8	131·6	79·8
2 days	764·1	525·6	381·5	263·2	159·5
3 „	1146·1	788·4	572·3	394·8	239·3
4 „	1528·2	1051·2	763·0	526·4	319·0
5 „	1910·2	1314·0	953·8	658·0	398·8
Semiaxes of orbits :—					
Major a....	30″·3	38″·8	48″·1	61″·6	86″·0
Minor b....	4 ·4	5 ·7	7 ·0	9 ·0	12 ·5

The values of the longitudes are to be interpolated for the time for which the places of the satellites are required by help of the motions in longitude for 1, 2, 3, 4, or 5 days.

[By taking $\frac{1}{10}$ of these quantities respectively, the motions for 2·4, 4·8, 7·2, 9·6, and 12·0 hours are obtained at sight.]

The rectangular coordinates x and y (in seconds of arc) for any satellite, reckoned parallel to the axes of the ring, are:—

$x = a$ Sin Long. $y = b$ Cos Long.

Astronomical Memoranda, 1879, *November.*

Sun. Nov. 1, sets $4^h\ 32^m$, rises $18^h\ 57^m$; Nov. 30, sets $3^h\ 53^m$, rises $19^h\ 46^m$.

Equation of time:—Sun *before* clock, Nov. 1, $16^m\ 18^s$; Nov. 11, $15^m\ 52^s$; Nov. 21, $14^m\ 1^s$; Dec. 1, $10^m\ 52^s$.

Sidereal Time at Mean Noon:—Nov. 1, $14^h\ 41^m{\cdot}6$; Nov. 11, $15^h\ 21^m{\cdot}0$; Nov. 21, $16^h\ 0^m{\cdot}5$; Dec. 1, $16^h\ 39^m{\cdot}9$.

Moon. Last Quarter, Nov. 6, $17^h\ 55^m$; New, Nov. 13, $12^h\ 38^m$; First Quarter, Nov. 20, $6^h\ 55^m$; Full, Nov. 28, $8^h\ 57^m$.

Occultations:—Nov. 4, $11^h\ 1^m$ δ Gemin. Oc. D. 140°, $11^h\ 37^m$ δ Gemin. Oc. R. 208°, $16^h\ 32^m$ 63 Gemin. Oc. R. 243°; Nov. 7, $15^h\ 5^m$ B.A.C. 3398 Oc. R. 292°, $16^h\ 40^m$ B.A.C. 3407 Oc. R. 277°, $18^h\ 6^m$ π Leonis Oc. R. 267°; Nov. 8, $13^h\ 38^m$ 34 Sextantis Oc. R. 210°; Nov. 10, $17^h\ 37^m$ *q* Virginis Oc. R. 176°; Nov. 18, $6^h\ 36^m$ σ Capric. Oc. D. 98°; Nov. 22, $7^h\ 55^m$ 16 Piscium Oc. D. 149°; Nov. 23, $10^h\ 57^m$ 45 Piscium Oc. D. 85°; Nov. 26, $8^h\ 24^m$ μ Arietis Oc. D. 102°; Nov. 27, $11^h\ 34^m$ 9 Tauri Oc. D. 137°; Nov. 28, $8^h\ 18^m$ 62 Tauri Oc. D. 72°; Nov. 29, $16^h\ 32^m$ 118 Tauri Oc. R. 193°; Nov. 30, $10^h\ 19^m$ 5 Gemin. Oc. R. 242°. The angles at disappearance (D.) and reappearance (R.) are reckoned from the *apparent* N. point towards the right hand round the Moon's circumference as seen in an inverting telescope.

Mercury is an evening star, at greatest elongation E. (21° 56′) on Nov. 21, when he sets in the S.W. about 1 hour after the Sun.

Venus is a morning star approaching greatest elongation (on Dec. 4). She is in conjunction with θ Virginis and 2′ S. of the star on Nov. 25 at 4^h. The illuminated portion of the disk is 0·130 on Oct. 15 and 0·388 on Nov. 15, and the corresponding diameters 48″·9 and 30″·6.

Nov. 1,	R.A. $11^h\,49^m{\cdot}7$	Dec. 0° 19′ S.,	tr. $21^h\,7^m$,	rises $15^h\,5^m$.
Nov. 30,	13 21 ·6	6 22 S.,	20 46	15 16

Mars is in opposition on Nov. 12, retrograding from Taurus to Aries. His diameter is 23″·0 on Nov. 1 and 20″·4 on Nov. 30.

Period of rotation $1^d{\cdot}025 = 1^d\,0^h{\cdot}60$.

Nov. 1,	R.A. $3^h\,27^m{\cdot}6$,	Dec. 18° 22′ N.,	tr. $12^h\,43^m$.
Nov. 30,	2 47 ·9,	17 13 N.,	10 10

Jupiter is an evening star, in quadrature on Nov. 26. He is in conjunction with σ Aquarii and 2′ S. of the star on Nov. 20 at 0^h. His diameter is 41″·2 on Nov. 1 and 37″·5 on Nov. 30.

Daily rate of rotation $870^\circ{\cdot}60 = 2^r\,150^\circ{\cdot}60$.

Nov. 1,	R.A. $22^h\,21^m{\cdot}2$,	Dec. 11° 41′ S.,	tr. $7^h\,38^m$,	sets $12^h\,42^m$.
Nov. 30,	22 27 ·5,	10 59 S.,	5 51 ,	10 58

Jupiter's Satellites:—Nov. 1, $8^h\,32^m$ ii. Tr. I., $11^h\,24^m$ ii. Tr. E.; Nov. 2, $8^h\,30^m$ iv. Tr. I.; Nov. 3, $8^h\,0^m$ ii. Ec. R.; Nov. 6, $9^h\,57^m$ i. Tr. I.; Nov. 7, $7^h\,17^m$ i. Oc. D., $8^h\,20^m$ iii. Oc. R., $10^h\,7^m$ iii. Ec. D., $10^h\,51^m$ i. Ec. R.; Nov. 8, $6^h\,45^m$ i. Tr. E., $11^h\,3^m$ ii. Tr. I.; Nov. 9, $5^h\,20^m$ i. Ec. R.; Nov. 10, $5^h\,12^m$ ii. Oc. D., $10^h\,37^m$ ii. Ec. R.; Nov. 11, $7^h\,39^m$ iv. Ec. R.; Nov. 14, $8^h\,38^m$ iii. Oc. D., $9^h\,11^m$ i. Oc. D.; Nov. 15, $6^h\,19^m$ i. Tr. I., $8^h\,38^m$ i. Tr. E.; Nov. 16, $7^h\,16^m$ i. Ec. R.; Nov. 17, $7^h\,45^m$ ii. Oc. D.; Nov. 19, $5^h\,47^m$ ii. Tr. E., $6^h\,20^m$ iv. Tr. E.; Nov. 22, $8^h\,13^m$ i. Tr. I.; Nov. 23, $5^h\,34^m$ i. Oc. D., $9^h\,11^m$ i. Ec. R.; Nov. 24, $5^h\,1^m$ i. Tr. E.; Nov. 25, $6^h\,15^m$ iii. Tr. E.; Nov. 26, $5^h\,30^m$ ii. Tr. I., $8^h\,23^m$ ii. Tr. E.; Nov. 27, $9^h\,4^m$ iv. Oc. D.; Nov. 28, $5^h\,10^m$ ii. Ec. R.; Nov. 30, $7^h\,30^m$ i. Oc. D.

Saturn is in Pisces, nearly stationary.

Nov. 1,	R.A. $0^h\,42^m{\cdot}4$,	Dec. 1° 37′ N.,	tr. $9^h\,59^m$,	sets $16^h\,11^m$.
Nov. 30,	0 37 ·5,	1 12 N.,	8 0	14 10

	Outer Ring.		Inner Ring.		Ball.
	Maj. Axis.	Min. Axis.	Maj. Axis.	Min. Axis.	Diam.
Oct. 27...	44″·14	6″·22	29″·35	4″·13	17″·6
Nov. 16...	43″·21	5″·74	28″·73	3″·82	17″·3

The south side of the rings is visible, the elevation of the Earth above their plane being 8° 6′ S. on Oct. 27 and 7° 38′ S. on Nov. 16, and of the Sun 9° 22′ S. and 9° 40′ S.

Neptune is in Aries, in opposition on Nov. 2.

Nov. 1,	R.A. $2^h\,35^m{\cdot}5$,	Dec. 13° 18′ N.,	tr. $11^h\,52^m$.
Nov. 30,	2 32 ·5,	13 4 N.,	9 55

EDITOR.

The Radiant-point of Comet I., 1870.

Sir,—

With reference to Dr. Dreyer's letter in the 'Observatory' for August, p. 120, the radiant-point of Comet I. 1870, as given in Professor Herschel's "List of the Radiant-points of Comets" in the B. A. Report for 1875, p. 233 (and quoted by me in the 'Observatory,' vol. ii. p. 165), has been already corrected by Prof. Herschel in his report on the "Progress of Meteoric Astronomy during the Year 1878" ('Monthly Notices,' vol. xxxix. p. 294). The theoretical radiant-point of this comet is there given at 25°·5 +45° instead of at 43°·5 +53°, as in the B. A. Report above referred to, and is within a few degrees of Dr. Dreyer's position at 27°·9 +48°·4.

The corrected place of the radiant-point (N. of γ Andromedæ) accords fairly well with several meteor-showers seen at the end of July and early in August, but the agreements may be purely accidental. The number of known meteor-radiants is now so great that they must obviously often correspond with the dates and positions of cometary radiants, though no physical connection may necessarily exist between them. In fact, though there are now 70 comets which present orbital resemblances to certain observed meteor-streams, yet none of these can be regarded as unquestionable agreements*, except the comet-meteors of April 20, Aug. 10, and Nov. 13 and 27.

I am, Sir, yours faithfully,

W. F. Denning.

Ashleydown, Bristol,
August 2, 1879.

Jupiter in September 1879.

Sir,—

On the night of the 29th of September last the sky was unusually good for observing, and a somewhat careful examination of the surface of this noble planet was made. No special attention was paid to the less striking colours of the different bands; but the central coloured zone and the faint bands to the north and south were looked at with care.

11^h 30^m G.M.T.—At a glance it was seen that the great central zone was not uniformly coloured red. The deepest colour lay along the lower (N.) edge of the northern equatoreal band. Resting on the southern edge of this band, and extending to near the lower edge of the southern equatoreal band, lay a rounded rosy cloud, sloping down to the northern belt to right and left. The space between this and the eastern limb of the planet was without colour except near the limb, where it was bluish perhaps.

The "red cloud" was seen as usual this autumn, and just above and in advance of it lay four or five faintly-coloured small spots.

To the south of the grand central zone only two distinct bands were seen; to the north four, some very faint.

I am, Sir, yours faithfully,

Joseph Gledhill.

Mr. Edward Crossley's Observatory,
Bermerside, Halifax.

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* Monthly Notices R. A. S. Vol. xxxix. p. 287.

The Red Spot or Cloud on Jupiter.

SIR,—

I see in your October number a notice from Prof. Pritchett respecting the remarkable red spot which drew some letters at the beginning of the present year. The first time I turned the telescope on Jupiter this year, July 24^{d} $12^{h}\cdot 8$, I noted that "the red spot . . . . is brilliant, close to the W. limb, retaining all the beautiful colour." At this time I estimated its centre as being about in long. 245°. I will now tabulate my observations as Prof. Pritchett has done, adding the long. ± of the centre of the spot.

Date.		G.M.T.	Longitude of Centre.	Longitude of Spot.	Notes.
1879.		h m	°	°	
Aug.	5	12 24	316	250	Spot close to *p* limb, extending seemingly a quarter across the disk, "and now has a light grey trail *following* it to past the central meridian."
„	9	11 54	177	265	Spot just coming round *f* limb.
„	12	11 54	273	240	Midway between the central meridian and the *p* limb. Definition was very poor.
„	29	11 15	291	240	Red spot just fully within the limb.
„	31	12 0	260	258	Opposition night. Red spot has a narrow red trail extending from its *f* side. Its central point has just past the central meridian.
Sept.	2	12 24	217	275	The spot has just come round the *f* limb.
„	3	11 24	332	245	Passing round the *p* limb.
„	7	12 12	243	256	The *p* end of the red spot a little past the central meridian.
„	9	12 50	207	250	Spot just fairly within the *f* limb.
„	14	12 25	226	273	„ „ „ „
„	17	12 36	324	255	Spot close to *p* limb.
„	26	12 36	240	275	Spot midway between the *f* limb and the central meridian.
„	27	10 42	322	260	Spot close to the *p* limb.
Oct.	1	11 18	226	275	Spot just within the *f* limb.
„	4	10 54	304	265	Spot approaching the *p* limb.
„	6	10 36	234	270	Midway between *f* limb and central meridian. Suspicions of a fine belt leaving the *p* end of the spot.
„	11	10 42	271	266	Centre of the spot just past the central meridian. There were occasional suspicions of streamers *p* and *f*.
„	16	10 39	302	262	Approaching *p* limb.

After reading Mr. Pritchett's list of observations it seems puzzling certainly that he missed the red spot. Referring to his list of estimations of times last season when the spot should have passed the central meridian, the corresponding times prepared with the aid of my observations are:—

Date.	Spot near centre. G.M.T.	Date.	Spot near centre. G.M.T.	Date.	Spot near centre. G.M.T.
1878.	h m	1878.	h m	1878.	h m
July 10	13 58	Sep. 10	14 14±	Oct. 2	12 30
Aug. 15	13 8	„ 20	12 20	Nov. 2	13 54
„ 31	16 9	„ 22	13 58		

From the above list it will be noticed that the failures on Aug. 31, Oct. 2, and Nov. 2 are most singular. The spot seems this year to have sensibly deepened in tint, though not in shading. Its present magnitude is enormous, it being at least 24,200 miles in length, and nearly one quarter of that in breadth.

The spot seems to be drawing a very considerable amount of attention, observations being recorded and published from places so far separated as Glasgow, Missouri, U.S.A., 94½° W., and Moscow, 37½° E.

I am, Sir, yours faithfully,

Southampton, 1879, Oct. 18. FRANK C. DENNETT.

Phenomenon of Jupiter's Satellite.

SIR,—

I send you an extract from a letter received by me this morning from Mr. B. Kidd, Bramley, Guildford. It will be interesting to know if other observers saw the phenomenon. The last occasion I fancy (I speak from memory) when the retrogression was noticed was by Admiral Smyth a great many years ago.

I am, Sir, yours truly,

N. S. GODFREY.

St. Bartholomew's Vicarage,
Southsea, 1879, Oct. 18.

"Last evening (Oct. 16) at 8^h 37^m to 8^h 40^m I saw a novelty, to me. Jupiter ii. Satellite Occultation. Disappearance, with Kellner and Barlow about 100 linear on 6-in. (of 8¼-in. With-Browning). I saw No. ii. go up to and touch Jupiter's disk. Then I changed eyepiece for a "C" Browning about 180; and I saw No. ii. again separated from Jupiter, looking clean and well defined. It went behind Jupiter, and, to my surprise, I saw it *through the planet* for some time. The moment I could no longer see it, I stepped to the lantern and looked at my watch; it was 8^h 40^m, or 3 minutes after the 'Nautical Almanac' time. I had compared my watch at noon with an expensive one just from the makers, and to-day I went to the post office at 10 A.M., and my watch (Dent) was not ¼ of a minute wrong." B. KIDD.

1879, Oct. 17.

NOTES.

THE AUGUST PERSEIDS.—The August meteors were duly watched for at the Royal Observatory, Greenwich; but the sky on the whole was unfavourable for observation. The paths of 207 meteors were, however, in all recorded, about one half of these being from the Perseus radiant, and the remainder from other radiants. On August 7, 8, and 13 the sky was overcast: on August 9 and 10 it was much clouded, and on these nights 27 meteors only were noted; on August 12 it was only partially cloudy, and 52 meteors were seen; but on August 11 the sky was very fine, and 128 meteors were observed. The distribution of meteors through the night on August 11 and 12 was as follows:—

Meteors seen	Before 10^h.	10^h to 11^h.	11^h to 12^h.	12^h to 13^h.	13^h to 14^h.	14^h to 15^h.	After 15^h.	Total.
Aug. 11 ..	0	6	32	32	22	29	7	128
12 ..	1	9	17	8	10	6	1	52

The following Table gives the number of meteors classified according to their radiants and magnitudes:—

Radiant.	> 1st mag.	1st mag.	2nd mag.	3rd mag.	4th mag.	Totals.
k Persei	7	47	39	6	0	99
Polaris	0	21	14	1	0	36
c Lacertæ	1	10	6	0	0	17
δ Andromedæ..	1	7	2	2	1	13
α Pegasi	0	7	3	1	0	11
Uncertain	0	15	16	0	0	31
Totals....	9	107	80	10	1	207

The observations were made by Messrs. Ellis, Nash, Greengrass, Hugo Simmons, and A. Pead, of the magnetical and meteorological department.

OBSERVATIONS OF MARS AT MADEIRA *.—Mr. Green's discussion of the fine series of observations of Mars which he made at Madeira in 1877 has been recently published in Vol. xliv. of the Memoirs R. A. S. Mr. Green made 41 drawings in all during the months of August and September, his instrument being a 13-inch silver-on-glass reflector; and from these 12 have been selected for publication, presenting a complete set of views of the planet at intervals of about two hours. They have been drawn on the stone by Mr. Green himself, and may thus be relied on as reproducing his original drawings with as much fidelity as can be obtained in lithography. From the Madeira series of views Mr. Green has formed a chart of Mars differing in the form of several of the markings from previous charts of the planet, the changes being supported by a reference to the drawings of other observers in 1877. Of course these changes have given an opportunity for the introduction of new names, 17 being added to the 47 already appropriated. The features of Mars seem now to be looked upon as prizes for the successful competitors in the exhibition of drawings of that planet, and we trust that the industry of observers will be greatly stimulated in the present and future oppositions by the prospect of earning immortality in perhaps the cheapest way available. But as we can hardly hope that the improvement in our powers of vision will continue to supply new features on Mars in sufficient number to reward eager aspirants after fame, it might, perhaps, be well to consider the seas, lands, bays, &c. as challenge-cups to be held only until a better drawing of the particular feature was produced. Otherwise it is to be feared that the study of this planet will languish as soon as the stimulus now provided is exhausted.

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* Memoirs R. A. S. Vol. xliv.

Mr. Green has, of course, only followed precedent, and he has, on the whole, exercised his christening powers with moderation; but the system does not seem a good one. Unfortunately Mädler began by attaching mere letters to the various markings, a system which was too severe a trial to the memory to be endured; whilst titles derived from fancied resemblances, such as the "hour-glass," the "hook," the "eye," &c., varied with each observer's fancy and led to great confusion even in the case of the few markings to which they were applied. But if a satisfactory nomenclature could not be founded on the neutral ground of classical mythology, it is worth considering whether the canonization of living observers is altogether decent. There would at any rate be less objection to the apotheosis of the dead. There is another question which suggests itself in looking through Mr. Green's list of names:—Have the oceans and seas and bays any real existence on Mars, or are they as imaginary as in the case of the Moon? Certainly a comparison of Mr. Green's chart of Mars with a chart of the earth seems to show that the Martial seas must be very unlike our seas in their action on the land. On Mars the so-called continents have rounded outlines, whilst the seas reproduce the spiky forms of our own continents. In our ignorance of the true character of the markings on Mars it would be more scientific to call them simply "spots," as we are content to do in the case of Jupiter, until we have a little more evidence to go upon.

WASHINGTON CATALOGUE OF STARS.—A second edition of this valuable catalogue has been published, in which Prof. Yarnall has included the observations in the six years 1872–1877, made with the view of completing the places of stars which had been insufficiently observed in the catalogue as first published. In the course of the work some 450 new stars have been added, and the catalogue now comprises about 11,000 stars, nearly all of which have been observed several times in R.A. and N.P.D. in the course of the thirty-three years of observation (1845–1877) represented by this catalogue. The value of the work is thereby greatly increased, and it may be expected to prove of the greatest use to astronomers.

It is with great regret that we hear of the sudden death of the author of this catalogue on February 27 last, just as the work was completed. Prof. Yarnall had been for 26 years Astronomer at the Washington Observatory, and a large majority of the observations, together with the whole of the reductions, were made by him. The completed volume only reached him, when he was already unconscious, an hour before his death. It forms, as Admiral Rodgers justly observes, a fitting memorial, coming at the close of the long professional life of its author—a life that was marked by rare qualities of patient labour, untiring devotion to duty, and personal gentleness, which comes from a pure and simple life.

SUN-SPOTS[*].—Prof. R. Wolf from his statistics of sun-spots finds that the relative number for 1878 is 3·4 only, those for the 1st, 2nd, 3rd, and 4th quarters being respectively 5·7, 4·1, 1·5, and 1·9, whilst the dearth of spots still continues in the first quarter of 1879, so that the epoch of minimum certainly falls later than 1878·5 and probably later than 1879·0, making the last period certainly greater than 11·3 and probably greater than 11·8 years. The following are the relative numbers for each year since last minimum:—

1867.	1868.	1869.	1870.	1871.	1872.	1873.	1874.	1875.	1876.	1877.	1878.
7·3	37·3	73·9	139·1	111·2	101·7	66·3	44·6	17·1	11·3	12·3	3·4

The diurnal ranges of magnetic declination observed at three stations compare with Prof. Wolf's formula, as depending on Sun-spots, as follow:—Prague, obs. 5′·65, calc. 6′·04; Christiania, obs. 5′·19, calc. 4′·77; Milan, obs. 5′·30, calc. 5′·20—being generally smaller than those for 1877, thus supporting Prof. Wolf's assumptions and periods.

MOTIONS OF SATURN'S SATELLITES[†].—In consequence of the near approach to commensurability in the periods of Titan and Iapetus, 5 times the former being $79^{d}\cdot7265$ or not quite $0^{d}\cdot4$ greater than once the latter ($79^{d}\cdot3296$), there will be a long inequality of the fourth order in the motions of these two satellites, having a period of 200 revolutions of Titan (about 8¾ years), as was pointed out in 1826 by Plana (Mem. R. A. S. Vol. ii. p. 350). At that time, however, the excentricity of the orbit of Iapetus was not known, but it has since been determined by Capt. Jacob from two years' observations of the satellites of Saturn made by him at Madras in 1856–1858 (Mem. R. A. S. Vol. xxviii.). M. Abel Souchon, a member of the Bureau des Longitudes, has now taken up the problem neglecting, however, the terms depending on the mutual inclination of the two orbits, which is so large (12° 47′) that the method of expansion in series fails; and he finds that, assuming (as Laplace has done) the mass of the perturbing satellite Iapetus to be $\frac{1}{200}$ of the mass of Saturn, the perturbation of the mean motion of Titan would be $129''{\cdot}5 \sin(5l'-l+130^\circ\ 13'\ 48'')$, whilst if we take the mass of Iapetus to be only $\frac{1}{1000}$, the coefficient would be reduced to $2''{\cdot}6$. A long series of observations is urgently wanted before any thing can be decided as to this and other points in the motions of the satellites. As regards inequalities arising from near approach to commensurability in the periods of Rhea and Titan, Dione and Enceladus, Tethys and Mimas, M. Abel Souchon remarks that they are unimportant, the inequality in the mean motions n and n' of Rhea and Titan depending on $7n'-2n$, amounting only to $0''{\cdot}10$ if the mass of Titan be taken as $\frac{1}{500}$, and being insensible for a mass not exceeding $\frac{1}{1000}$.

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[*] Astronom. Mittheilungen, Nos. 48, 49.

[†] Astronom. Nachrichten, No. 2263.

Prof. Asaph Hall has discussed all the available observations of Hyperion with a view to determining the elements, and more particularly the periodic time and motion of the apsides, as to the latter of which there is still a doubt whether it is direct or retrograde. The best series of observations are those made by Mr. Lassell in 1852, and by the Washington observers in 1875, and these give the following elements, respectively, referred to G.M.T. and the plane of the equator:—

Perisaturnium...	1852, Nov. 17·5208.	1875, Oct. 27·8380.
ϖ	$240^\circ\ 10'{\cdot}9$	$172^\circ\ 59'{\cdot}7$
e	$0{\cdot}12011$	$0{\cdot}11885$
a	$217''{\cdot}05$	$216''{\cdot}56$
☊	..	$120^\circ\ 12'{\cdot}0$
ι	..	$6{\cdot}12{\cdot}1$

Assuming 394 revolutions to have occurred in the interval, the anomalistic period $T=21{\cdot}2673026$ days; and taking the smaller retrograde motion of the apsides,

$$\delta\varpi = -2^\circ{\cdot}928616 \text{ in 1 year.}$$

Comparing the positions of the satellite computed from these elements with the observations of 1848 and 1878, the result is that for 1848 the value of T requires to be slightly increased, and that of $\delta\varpi$ diminished, whilst the observations of 1878 indicate that T must be diminished by $0^{d}{\cdot}007$ and $\delta\varpi$ also diminished. The observations of 1848, however, and those made since 1875, do not fix the position of the satellite in its orbit with much certainty, since they were made when the plane was seen nearly edgewise. From 1882 to 1888 Prof. Hall thinks that Hyperion can be followed completely round its primary with the Washington refractor, and should be carefully observed. From the elements it appears that Hyperion moves nearly in the plane of the orbit of Titan, and in consequence of the excentricities, the two satellites can approach very near each other. Also the value of a shows that Hyperion is moving in a larger orbit than that which corresponds to the periodic time and Bessel's mass of Saturn. The next step which Prof. Hall hopes to undertake is the calculation of the action of Titan on Hyperion.

THE INTERIOR OF SATURN*.—Taking Laplace's formula for the ellipticity of a homogeneous spheroid, Prof. Hall has applied it to the case of Saturn, and finds that the theoretical value of the ratio of the axes is 1·1859, whilst the observed value is 1·1087 according to Bessel, or 1·1223 according to Kaiser. These results show that Saturn is less flattened than it would be if homogeneous and that the density increases from the surface towards the centre. Since the mean density is only about ¾ that of water, we may infer that the fluid of the surface of Saturn has a very small density. A somewhat similar result was arrived at by Laplace in the case of Jupiter, the theoretical ratio of the axes being 1·10967, whilst the value found by direct observation is 1·0770, and the more accurate value deduced from the theory of the satellites is 1·0747.

PHOTOGRAPHS OF THE ECLIPSE OF JULY 19†.—M. Janssen has taken at Marseilles photographs of the Sun 12 inches in diameter during the late partial eclipse, and *à propos* of these he calls attention to the value of photography in such cases. In the first place the observation of contacts may be obtained photographically by M. Janssen's revolver. Then from micrometric measures of photographs taken during the progress of the eclipse, the relative positions of the Sun and Moon may be determined. Hitherto only total or annular eclipses have been utilized for this purpose‡, but the observation of partial eclipses will doubtless treble the opportunities of improving the lunar tables by the help of these phenomena. Photographs may also be studied with advantage to determine whether any deformation of the solar granules is perceptible at the limb of the Moon as the result of refraction by a lunar atmosphere, and also to measure the heights of lunar mountains at the limb. In the case of the Marseilles photographs the granules show no sensible differences at the Moon's limb.

Astronomers will await with much interest the publication of the results of the measures of M. Janssen's photographs of the late partial eclipse. In a case of this kind every thing depends on the degree of accuracy attainable in the measures; and it must be confessed that in this respect the result of the experimental application of photography to the late Transit of Venus has been disappointing.

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* Astronom. Nachrichten, No. 2269.

† Comptes Rendus, Vol. lxxxix. No. 6.

‡ M. Janssen would seem to have overlooked the long series of micrometric observations of *partial* eclipses made at Greenwich. The method was first applied to the eclipse of 1833 by Prof. (now Sir G.) Airy at Cambridge, and has since been applied by him at Greenwich to the eclipses of 1836, 1845, 1846, 1858, 1860, 1870, 1874, and 1875. The micrometric measures have been completely discussed, and from the final equations of condition the corrections to the Lunar Tables have been deduced and published in the 'Greenwich Observations' and 'Monthly Notices.' Notwithstanding all the efforts that have been made, it has not been found possible to observe more than these *nine* eclipses in the course of 46 years.

SWIFT'S COMET.—We learn from the 'Science Observer' for July that Mr. J. C. Howard of Boston, U.S., succeeded in following the comet with a 3-inch telescope up to July 25; and Mr. Clacy with a 6-inch saw it well as late as July 29, when it was within 2′ or 3′ of the place predicted in Mr. Chandler's ephemeris, given in the 'Science Observer' for June. Mr. Swift writes that he saw the comet on July 14 with an aperture contracted to 2 inches, and thinks he could have seen it with 1½ inch.

THE AUGUST METEORS*.—From observations made on the night of August 11, M. Chapelas found the mean horary number of meteors to be 114·2 for midnight, a number greater than that found for the maximum of 1848, hitherto considered the most remarkable of the century. The nights of Aug. 8, 9, and 10 were unfortunately cloudy; but some breaks in the clouds indicated the probable intensity of the meteoric shower; and the observations of subsequent nights show, by means of a curve, that the mean number of meteors per hour was 138·4 on Aug. 10, or 28·4 more than the maximum of 1848. The observation of the shower in 1880 will allow of the determination of the true period of recurrence of the August meteors, which, so far, would seem to be 31 years. As usual, the radiants were near the zenith in the constellations Cepheus, Camelopardus, and Perseus.

It seems difficult to reconcile this statement with the accounts of other observers.

VOLUME XLIV. OF THE R. A. S. MEMOIRS has been recently published. Its Contents are:—"On a general Method of Treating the Lunar Theory," by E. Neison. "Determination of the Solar Parallax from the Opposition of Mars, 1877," by Maxwell Hall (the value found is $8''\cdot789 \pm 0''\cdot060$). "Observations of Mars at Madeira in August and September 1877," by N. E. Green. "Double Star Observations made in 1877–8 at Chicago with the 18½-inch Refractor of the Dearborn Observatory, comprising—i. A Catalogue of 251 New Double Stars with Measures; ii. Micrometrical Measures of 500 Double Stars," by S. W. Burnham, M.A. "Les Longueurs du pendule à secondes à Pulkova, à St. Pétersbourg, et aux différents points de la Russie occidentale, corrigées de l'influence produite par la flexion des supports du pendule construits par M. Repsold," by Prof. Savitsch.

The first two memoirs are dated 1877, May, and 1877, Nov. respectively, so that there has been a delay of two years in their publication. Considering the importance of early publication as a means of advancing science, it is to be hoped that the Council of the R. A. S. will give their attention to the question, and introduce some reform in the present system. There seems to be no sufficient reason why each memoir should not be published independently, as a separate part of the volume of 'Memoirs,' the several parts being afterwards collected into one volume, as in the case of the several Nos. of the 'Monthly Notices.' In any case the first two memoirs, consisting of 120 quarto pages, might naturally have formed Part i., and have been published a year and a half ago.

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* Comptes Rendus, Vol. lxxxix. No. 8.

It is a notorious fact that many papers are printed in the 'Monthly Notices' which would be more suitable in every respect for the 'Memoirs,' simply because their authors are unwilling to have them indefinitely postponed, until it pleases the executive to bring out a volume of 'Memoirs.' And the evil aggravates itself; for the fewer memoirs are received, the longer the delay, and the longer the delay, the more unwilling are authors to submit to it. They will naturally seek a more speedy channel of publication. But possibly we ought not to be surprised at such a trifling delay as two years in the case of Vol. xliv., considering that the Eclipse Reports in Vol. xl., which the Senior Secretary has appropriated to himself for the last *nine* years, are still unpublished.

CHANGES IN JUPITER.—According to the 'Chicago Tribune' the measures of the planet made at the Dearborn Observatory indicate that considerable changes take place in the outline of the planet from day to day. Two sets of measures taken at an interval of a week indicated a difference of 5° in the direction of the major axis, which, according to the first set, lay 3° north of the east end of the middle belt, and according to the second 2° south. The writer presumes that these changes are due to atmospheric waves similar to the well-known barometric variations on the Earth, but on a much larger scale, as the atmosphere of Jupiter is of enormously greater depth and density than our own, being probably some 2000 miles in depth.

THE FLIGHT AND FALL OF METEORS.—Professor C. U. Shepard, in a paper on "Meteorites," read before the Connecticut Academy of Sciences, states the number of supposed independent falls of such bodies, whereof specimens are preserved in museums, as about three hundred and fifty, which number is increasing at the rate of between three and four per year. In the northern hemisphere there are two regions where falls of meteorites have been most frequent. "These regions," says Professor Shepard, "are apparently situated where they have been similarly influenced by the earth's magnetic polarity. The regions are on opposite sides of the hemisphere, have similar areas, and are in analogous directions and at similar distances from the two terrestrial north magnetic poles." The author calls attention to the highly magnetic constitution of nearly all meteorites, and to the fact that each mass, whether large or small, of a detonating meteor, maintains during its aerial flight a fixed axial direction. He infers that "if a strong magnetic force is found to attend these bodies, the perplexing subject relating to their high and variant velocities may receive some elucidation." He thinks that the great objection to the theory of the volcanic origin of meteorites, viz., that their velocity is too great, may be obviated by the hypothesis that acceleration may be produced by the electro-magnetism of the earth.—*Popular Science Monthly.*

PROF. C. W. PRITCHETT of the Morrison Observatory at Glasgow, Missouri, proposes to supply Kansas City with standard time by means of an electric time-ball placed above the Union Depot, and dropped each day precisely at noon. Mr. Nettleton, Manager of the Union Depot, and Mr. McMullin, Superintendent of the Chicago and Alton Railroad, are considering the matter and propose to make the necessary arrangements as soon as practicable.

Such a ball has been in operation at New York for over a year without a single failure, and is always within 0·5 of a second of absolute correctness.

Although this will be no little trouble to Prof. Pritchett, he proposes to operate the time-ball the first year without charge, if the railroad companies will provide the connections, ball, &c.—*Kansas City Review of Science.*

WE learn that a new observatory has been founded at Kalocza in Hungary, by the Cardinal Archbishop Haynald, and that Dr. Charles Braun has been appointed Director. The building is nearly finished, and the instruments already procured comprise:—two equatoreals by Merz (one of 7 Paris inches and the other of 4), a transit-instrument of 2½ inches aperture by Cooke, an astronomical clock and two auxiliary clocks with electric contact, a chronograph, a chronometer, a theodolite with four microscopes reading directly to 2″, a reflection circle, a Zöllner astro-photometer, a direct-vision Browning spectroscope, and a more powerful spectroscope still to be supplied. In a few years a transit-circle of moderate dimensions will be added. Dr. Braun appeals to the liberality of established observatories to aid him in the formation of an astronomical library, by presents of their publications.

AT the meeting of the Manchester Literary and Philosophical Society on Oct. 7, the President, Dr. Joule, described a simple means for checking the oscillations of a telescope. It consists of a leaden ring placed centrally about the axis of the tube of the telescope and attached thereto by three or more elastic caoutchouc bands. He had employed two of these rings for his telescope, one placed near the object-glass, the other near the eyepiece. Their united weights were only one quarter of that of the telescope tube, but nevertheless they diminished the time required for the cessation of vibration to one sixth of what it was before their application.

THREE new Minor Planets have been discovered in the present month, viz.—No. 204 on Oct. 8 and No. 205 on Oct. 13 by Herr Palisa at Pola, and No. 206 on Oct. 15 by Prof. Peters, at Clinton, New York. EDITOR.

The Satellites of Mars.

THE following are the approximate Greenwich Mean Times of elongation *p* or *f* of the outer satellite *Deimos*, derived from the data given in Prof. Asaph Hall's memoir (*cf.* 'Observatory,' Vol. ii. No. 19, p. 230):—

Elongations:—Pos.-angle 53° or 233°. Distance 67″.

	h.		h.		h.
Nov. 1 ..	17·1 *f*	Nov. 11 ..	19·5 *f*	Nov. 21 ..	6·8 *p*
2 ..	8·3 *p*	12 ..	10·7 *p*	22 ..	13·1 *p*
3 ..	14·6 *p*	13 ..	17·0 *p*	23 ..	19·4 *p*
4 ..	5·7 *f*	14 ..	8·1 *f*	24 ..	10·5 *f*
5 ..	12·0 *f*	15 ..	14·4 *f*	25 ..	16·8 *f*
6 ..	18·3 *f*	16 ..	5·6 *p*	26 ..	8·0 *p*
7 ..	9·5 *p*	17 ..	11·9 *p*	27 ..	14·3 *p*
8 ..	15·8 *p*	18 ..	18·2 *p*	28 ..	5·4 *f*
9 ..	6·9 *f*	19 ..	9·3 *f*	29 ..	11·7 *f*
10 ..	13·2 *f*	20 ..	15·6 *f*	30 ..	18·0 *f*

The satellite revolves in $1^{d}\,6^{h}{\cdot}30$; thus the elongation *p* occurs $15^{h}{\cdot}15$ before the elongation *f*, and conversely. The semi-minor axis of the apparent ellipse is about 17″; and the semi-diameter of the planet being 12″, the satellite passes about 5″ N. of the limb at inferior conjunction, and 5″ S. at superior conjunction, moving in the direction *n p s f*, since the S. pole of Mars is turned towards us.

Mr. Common writes that Deimos is now pretty plain, and ought to be seen by many observers. He has seen it several times lately and has got a number of measures of position, but cannot find Phobos. We have not yet heard of any observations of the satellites in America.

Observers may find advantage in reducing the glare of the planet by means of a dark glass covering half of the field-lens of a positive eyepiece, so that simultaneous measures of the planet and satellite may be made, the planet being viewed through the dark glass, whilst the satellite is seen through the clear part of the field.

Satellites of Saturn.*

LONGITUDES of the five inner satellites reckoned from superior conjunction with the planet's centre:—

Greenwich Noon.	Mimas.	Encel.	Tethys.	Dione.	Rhea.
	°	°	°	°	°
Nov. 1	10·6	145·1	208·5	181·2	331·2
6	120·7	19·0	82·2	119·1	9·9
11	230·7	252·8	315·9	57·0	48·6
16	340·7	126·6	189·5	354·9	87·2
21	90·7	0·4	63·2	292·7	125·8

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* From an Ephemeris by A. Marth, Astron. Nachr. No. 2273.

Greenwich Noon.	Mimas.	Encel.	Tethys.	Dione.	Rhea.
	°	°	°	°	°
Nov. 26	200·6	234·1	296·7	230·5	164·4
Dec. 1	310·5	107·8	170·3	168·2	202·9
Motion in longitude					
for 1 day	382·0	262·7	190·7	131·5	79·7
2 days	764·0	525·5	381·4	263·2	159·4
3 „	1146·0	788·3	572·2	394·7	239·2
4 „	1528·0	1051·0	762·9	526·3	318·9
5 „	1910·0	1313·8	953·6	657·9	398·6
Semiaxes of orbits :—					
Major a....	29″·4	37″·8	46″·8	59″·9	83″·7
Minor b....	3 ·9	5 ·0	6 ·2	8 ·0	11 ·1

The values of the longitudes are to be interpolated for the time for which the places of the satellites are required by help of the motions in longitude for 1, 2, 3, 4, or 5 days.

[By taking $\frac{1}{10}$ of these quantities respectively, the motions for 2·4, 4·8, 7·2, 9·6, and 12·0 hours are obtained at sight.]

The rectangular coordinates x and y (in seconds of arc) for any satellite, reckoned parallel to the axis of the ring, are :—

$x = a$ Sin Long. $y = b$ Cos Long.

Astronomical Memoranda, 1879, *December.*

Sun. Dec. 1, sets $3^{h}\ 53^{m}$, rises $19^{h}\ 47^{m}$; Dec. 31, sets $3^{h}\ 58^{m}$, rises $20^{h}\ 10^{m}$.

Equation of time :—Sun *before* clock, Dec. 1, $10^{m}\ 52^{s}$; Dec. 11, $6^{m}\ 38^{s}$; Dec. 21, $1^{m}\ 47^{s}$; *after*, Dec. 31, $3^{m}\ 9^{s}$.

Sidereal Time at Mean Noon :—Dec. 1, $16^{h}\ 39^{m}{\cdot}9$; Dec. 11, $17^{h}\ 19^{m}{\cdot}3$; Dec. 21, $17^{h}\ 58^{m}{\cdot}7$; Dec. 31, $18^{h}\ 38^{m}{\cdot}2$.

Moon. Last Quarter, Dec. 6, $7^{h}\ 43^{m}$; New, Dec. 12, $23^{h}\ 4^{m}$; First Quarter, Dec. 19, $23^{h}\ 16^{m}$; Full, Dec. 28, $4^{h}\ 16^{m}$.

A partial Eclipse of the Moon occurs on Dec. 28. Magnitude of the Eclipse 0·167. The Moon rises at Greenwich at 3 46^{m}.

First contact with Penumbra $1^{h}\ 52^{m}{\cdot}3$, with Shadow $3^{h}\ 37^{m}{\cdot}4$.
Last „ „ „ 6 59 ·9, „ 5 14 ·8.

Occultations :—Dec. 1, $10^{h}\ 38^{m}$ 44 Gemin. Oc. R. 188°; Dec. 2, $16^{h}23^{m}$ B.A.C. 2683 Oc. R. 228°; Dec. 3, $13^{h}\ 17^{m}$ 54 Cancri Oc. R. 160°; Dec. 4, $15^{h}\ 18^{m}$ *o* Leonis Oc. R. 284°; Dec. 19, $10^{h}\ 56^{m}$ *κ* Piscium Oc. D. 126°; $11^{h}\ 0^{m}$ 9 Piscium Oc. D. 89°; Dec. 22, $5^{h}\ 6^{m}$ 101 Piscium Oc. D. 63°; Dec. 24, $15^{h}\ 4^{m}$ 66 Arietis Oc. D. 141°; Dec. 25, $5^{h}\ 52^{m}$ 36 Tauri Oc. D. 150°; $16^{h}\ 48^{m}$ 62 Tauri Oc. D. 84°; Dec. 26, $6^{h}\ 24^{m}$ *k* Tauri Oc. D. 131°; Dec. 27, $5^{h}\ 12^{m}$ 132 Tauri Oc. D. 81°. The angles at disappearance (D.) and reappearance (R.) are reckoned from the *apparent* N. point towards the right hand round the Moon's circumference as seen in an inverting telescope.

Mercury is in inferior conjunction on Dec. 10, after which he is a morning star, at greatest elongation W. (22° 30′) on Dec. 30. On Dec. 24 he rises in the E.S.E. at 18^{h} 10^{m}, nearly 2^{h} before the Sun, and is then very favourably placed for observation.

Venus is a morning star, at greatest elongation W. (46° 47′) on Dec. 4. The illuminated portion of the disk is 0·388 on Nov. 15 and 0·557 on Dec. 15, and the corresponding diameters 30″·6 and 21″·6.

Dec. 1, R.A. 13^{h} 25^{m}·4 Dec. 6° 40′ S., tr. 20^{h} 45^{m}, rises 15^{h} 18^{m}.
Dec. 31, 15 31 ·6 16 15 S., 20 54 16 17

Mars is nearly stationary in Aries. His diameter is 20″·2 on Dec. 1 and 14″·8 on Dec. 31. Period of rotation 1^{d}·025 = 1^{d} 0^{h}·60; Hourly rotation 14°·62; Pos.-angle of Axis 142°; Lat. of Centre of Disk 18° S. [from Mr. Marth's Ephemeris].

Dec. 1, R.A. 2^{h} 47^{m}·0, Dec. 17° 11′ N., tr. 10^{h} 5^{m}, sets 17^{h} 41^{m}.
Dec. 16, 2 40 ·4, 17 11 N., 9 0 16 36
Dec. 11, 2 45 ·7, 17 51 N., 8 7 15 47

Jupiter is an evening star, advancing through Aquarius. His diameter is 37″·4 on Dec. 1 and 34″·4 on Dec. 31.

Dec. 1, R.A. 22^{h} 27^{m}·9, Dec. 10° 56′ S., tr. 5^{h} 47^{m}, sets 10^{h} 55^{m}.
Dec. 31, 22 44 ·0, 9 16 S., 4 5 9 22

Daily rate of rotation 870°·60 = 2^{r} 150°·60; Hourly rate 36°·275.

Jupiter's Satellites:—Dec. 1, 6^{h} 57^{m} i. Tr. E.; Dec. 2, 5^{h} 36^{m} i. Ec. R.; 6^{h} 44^{m} iii. Tr. I.; Dec. 3, 8^{h} 8^{m} ii. Tr. I.; Dec. 5, 7^{h} 48^{m} ii. Ec. R.; Dec. 6, 5^{h} 31^{m} iii. Ec. R.; 8^{h} 56^{m} iv. Sh. I.; Dec. 7, 9^{h} 27^{m} i. Oc. D.; Dec. 8, 6^{h} 35^{m} i. Tr. I.; 8^{h} 54^{m} i. Tr. E.; Dec. 9, 7^{h} 31^{m} i. Ec. R.; Dec. 12, 4^{h} 59 ii. Oc. D.; Dec. 13, 6^{h} 19^{m} iii. Ec. D.; Dec. 14, 8^{h} 19^{m} iv. Oc. R.; Dec. 15, 8^{h} 33^{m} i. Tr. I.; Dec. 16, 5^{h} 54^{m} i. Oc. D.; Dec. 17, 5^{h} 22^{m} i. Tr. E.; Dec. 19, 7^{h} 41^{m} ii. Oc. D.; Dec. 20, 5^{h} 6^{m} iii. Oc. D.; 8^{h} 40^{m} iii. Oc. R.; Dec. 21, 5^{h} 46^{m} ii. Tr. E.; Dec. 23, 7^{h} 4^{m} iv. Sh. E.; 7^{h} 53^{m} i. Oc. D.; Dec. 24, 5^{h} 1^{m} i. Tr. I.; 7^{h} 21^{m} i. Tr. E.; Dec. 25, 5^{h} 51^{m} i. Ec. R.; Dec. 28, 5^{h} 37^{m} ii. Tr. I.; 8^{h} 30^{m} ii. Tr. E.; Dec. 30, 4^{h} 59^{m} ii. Ec. R.; Dec. 31, 7^{h} 1^{m} i. Tr. I.

Saturn is stationary in Pisces; in quadrature on Dec. 30.

Dec. 1, R.A. 0^{h} 37^{m}·4, Dec. 1° 12′ N., tr. 7^{h} 56^{m}, sets 14^{h} 6^{m}.
Dec. 16, 0 37 ·0, 1 13 N., 6 57 13 7
Dec. 31, 0 38 ·1, 1 24 N., 5 59 12 10

	Outer Ring.		Inner Ring.		Ball.
	Maj. Axis.	Min. Axis.	Maj. Axis.	Min. Axis.	Diam.
Dec. 6...	41″·91	5″·44	27″·87	3″·62	16″·8
Dec. 26...	40″·47	5″·35	26″·91	3″·56	16″·2

The south side of the rings is visible, the elevation of the Earth above their plane being 7° 27′ S. on Dec. 6 and 7° 36′ S. on Dec. 26, and of the Sun 9° 57′ S. and 10° 15′ S.

Neptune is in Aries.

Dec. 1, R.A. 2^{h} 32^{m}·4, Dec. 13° 3′ N., tr. 9^{h} 51^{m}.
Dec. 31, 2 30 ·2, 12 54 N., 7 51

EDITOR.

Publications received:—Astronomical Observations and Researches made at Dunsink Observatory, Part iii. (Dublin, 1879)—Dr. N. von Konkoly, *Beobachtungen angestellt am Astro-physicalischen Observatorium in O-Gyalla in Ungarn* (H. W. Schmidt, Halle, 1879)—E. Crossley, J. Gledhill, and J. M. Wilson, *Handbook of Double Stars* (Macmillan, 1879)—Prof. Piazzi Smyth, *Colour in Practical Astronomy spectroscopically examined* (Trans. Roy. Soc. Edinburgh, Vol. xxviii.); *Carbon and Hydrocarbon spectroscoped and spectrometed in* 1879 (Phil. Mag. 1879, Aug.)—Ernst Hartwig, *Durchmesser der Planeten Venus und Mars* (Publication der Astron. Gesellsch., W. Engelmann, Leipzig, 1879)—Arthur Schuster, *On Spectra of Metalloids: Spectrum of Oxygen* (Phil. Trans. 1879, Part i.); *Easy Method for adjusting the Collimator of a Spectroscope* (Phil. Mag. 1879, Feb.); *On Spectra of Lightning* (Phil. Mag. 1879, May)—Prof. Asaph Hall, *The Motion of a Satellite* (The Analyst, 1879, Sept.)—G. H. Darwin, *Determination of Secular Effects of Tidal Friction by a Graphical Method* (Proc. R. S. No. 197, 1879)—Prof. Harkness, *On the Colour Correction of Achromatic Telescopes* (Amer. Journ. 1879, Sept.)—*Popular Science Monthly*, 1879, July–Oct. (Appleton, New York)—*American Journal of Science*, 1879, July–Oct. (Newhaven, Conn.)—*Science Observer*, 1879, June and July (Boston Scientific Society, Mass.)—*Kansas City Review*, 1879, July–Oct.—*Vierteljahrsschrift der Astronomischen Gesellschaft*, 14 Jahrgang, iii. Heft (Leipzig, W. Engelmann)—*Sirius*, 1879, July–Oct. (Leipzig, Karl Scholtze)—*Natural History Journal*, 1879, Sept. (York). EDITOR.

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THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

No. 31. NOVEMBER 1. 1879.

A Determination of the Sun's Mean Equatoreal Horizontal Parallax from Declination Observations of Mars and Stars made at the Observatories of Leiden and Melbourne near the time of Opposition, 1877.

When Mr. Ellery's important pamphlet, containing the meridian observations of *Mars* and comparison-stars made at Melbourne in 1877, reached me, I proposed to myself to compare these observations with the corresponding ones made at Greenwich, and to compute the resulting solar parallax. I found, however, that, unfortunately, the same list of comparison-stars had not been used at the two Observatories, Mr. Ellery having adopted the list issued by the Washington Observatory, whilst at Greenwich the 'Nautical Almanac' stars were observed; so that no comparison could be made between the extensive series of *Mars* observations which had been secured both at Greenwich and at Melbourne. Lately, however, Prof. H. G. van de Sande Bakhuyzen has been so kind as to send me the fine series of observations of *Mars* made at the Leiden Observatory near the time of Opposition, 1877, the Washington list having been also adopted at Leiden; and these I have compared with the corresponding Melbourne observations.

The assumed positions of the Observatories are:—

	Latitude.	Longitude.
Leiden	$52^{\circ}\ 9'\ 19''.96$ N.	$0^{h}\ 17^{m}\ 56^{s}.2$ East of Greenwich.
Melbourne..	37 49 53 ·4 S.	9 39 54 ·8 ,, ,,

The effect of parallax on the declination is given by the expression $\pi . \frac{r}{\Delta} . \sin(z-n)$, where π is the Sun's mean equatoreal horizontal parallax, r the radius of the Earth at the place of observation, Δ the planet's distance from the centre of the Earth, z the observed zenith-distance cleared from refraction, and n the angle of the vertical. All the tabular quantities have been interpolated with second differences from the 'Nautical Almanac.' The small correction for change of error of the ephemeris in passing from the meridian of Leiden to that of Melbourne has been found from the errors of the tables at successive passages of *Mars* over the meridian of Leiden, assuming $\pi = 8''\cdot 9$. I have only made comparisons of the observations on those days when *Mars* and stars were observed at Melbourne either on the same day as at Leiden, or on the following day, so that the change of error has merely either to be carried back for 9½ hours or carried forward for 14½ hours.

The combining weight is proportional to

$$\frac{(\text{Factor of parallax})^2}{(\text{Probable error of combination of observed places})^2}.$$

The separate determinations are given in the following Table:—

Date.	Parallax.	Weight.
July 20 & 21 21	8″·937	4·142
23	8·899	3·333
30	8·904	3·929
Aug. 4 & 5 5	9·036	2·339
6 & 7	9·234	2·703
8	9·025	4·400
15	8·813	4·351
17	9·100	4·909
21	9·375	3·009
22 & 23	8·833	4·600
30	8·906	2·447
Sept. 1	9·170	4·804
2	8·719	3·305
4 & 5	9·392	3·421
Sept. 12 & 13	8″·742	3·635
17	8·811	2·949
21 & 22	8·976	2·816
25	8·729	2·858
27	8·873	4·172
30 & Oct. 1	9·082	2·571
2	8·654	2·382
3	8·785	3·544
4	8·976	3·551
5	8·901	3·352
6	8·646	2·285
9	9·055	3·370
14	9·058	1·857
15	9·101	2·997
20 & 21	9·029	2·690

The resulting mean solar parallax is $8''\cdot 960 \pm 0''\cdot 051$, assuming that the probable error of a single observation of declination is $0''\cdot 5$.

In the light of recent researches on the subject we must, I suppose, consider this value of the solar parallax to be too large; it is, however, to be remarked that Prof. Winnecke obtained the value 8″·96, and Mr. Stone that of 8″·94, from meridian observations of *Mars* in 1862. And although Prof. Newcomb's discussion gave the result 8″·855, still, as Mr. Stone has pointed out, the smaller value depends chiefly on the use of the Santiago observations, which he declares to be inadmissible. It appears, therefore, that from meridian observations of *Mars* we get a parallax of not less than 8″·9, and we are almost forced to the conclusion that there is something in the *method*, which persistently tends to give a large result; if this be so it introduces another very perplexing element into researches on solar parallax.

The value 8″·96 corresponds to a mean distance of the Sun from the Earth of 91,229,000 miles. A. M. W. Downing.

*The Eclipses of the Satellites of Mars.**

When the Satellites of Mars were first discovered, some very exaggerated statements were made as to the tides which would be produced by them in the Martian waters. It was not long, however, before these statements were corrected. Very little calculation showed that the smallness of their masses, notwithstanding their proximity to the planet, would prevent the formation by them of tides of any importance, and that they would not, in this respect, have the very beneficial influence which some writers were at first inclined to claim for them, of preventing all tendency to stagnation in the seas and oceans of the planet.

On the other hand, the interest which properly belongs to these bodies was to some extent wrongly diminished by the suggestion (made in one of our newspapers) that the satellites would be rarely seen from Mars, because they would be almost always either eclipsed or obscured by the effect of morning or evening twilight.

It may therefore be interesting to notice (although in a very rough and elementary manner) how far this suggestion is true or otherwise. Their light at all times is comparatively small, somewhere about $\frac{1}{70}$ of that which we receive from our moon being the amount that would probably be received from the inner Martial moon, while that from the outer moon would practically be of very little account; it would probably be about 20 times less than that from the inner moon.

At the same time we ought not to diminish the small amount of light given by them, by charging upon them a proneness to suffer eclipse greater than is really the case.

Let us take the diameter of Mars to be 4200 miles. We may roughly take the diameter of its shadow to have this same value at so small a distance from the centre of the planet as 5800 miles for the inner and 14,500 for the outer satellite. These we take to be their distances according to the determination of Professor Asaph Hall. We will also consider their orbits to be circular.

The inner satellite, revolving in $7^h\ 40^m$ in an orbit whose circumference is about 36,400 miles, would pass centrally through the shadow (a distance of about 4200 miles) in about 53 minutes.

The outer, revolving in about $30^h\ 17^m$ in an orbit whose circumference is about 91,000 miles, would pass centrally through the shadow in about 84 minutes. The inner satellite Phobos remains above the horizon of any place for about 5½ hours at a time. Out of these 5½ hours it cannot therefore be eclipsed for more than about 53 minutes.

The outer satellite Deimos remains above the horizon of any place for about 60 hours at a time. This time would not, I believe, suffice for *three* total eclipses to be fully seen. Two might be seen during their whole duration, and the satellite (speaking roughly) would be eclipsed between 2 and 3 hours out of the 60.

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* From a Lecture delivered at Gresham College.

Let us take an example or two of what might occur. Supposing sunset to take place at such an hour as on the Earth we should call 6 P.M. Phobos might rise soon afterwards in the west. In about 3½ hours the sun would be about 51° below the horizon, and the satellite about 54° above it in the east; and if we suppose them at the time in question to be seen from a place on the equator of Mars, and to be both apparently moving in the equator of the heavens, the satellite will now be in the middle of an eclipse, which, having commenced at about 9 P.M., will finish about 10 P.M. At about 11½ P.M. it will set in the east, about 5 A.M. will rise again, and for the greater part of the time between this hour and the rising of the Sun at about 6 A.M. another eclipse will be going on—*i. e.* two eclipses during the same night, but only *one* between a rising and setting of the satellite.

In the above we roughly speak of Martian hours as corresponding with those of the Earth, the difference in the length of their diurnal rotations being small. If by the Martian hour we mean $\frac{1}{24}$ of its rotation period, this would equal about $1^h\ 1\frac{1}{2}^m$ of terrestrial time.

Again, we may suppose the Sun to set about 6 P.M. and Deimos to rise soon afterwards. A total eclipse lasting about 84 minutes will shortly take place. If we suppose this to begin when Deimos is a few degrees above the horizon, it will have increased that distance rather more than 72° at the next sunset, and soon after midnight will be in the middle of another total eclipse. The Sun will rise and set again and Deimos will pass below the horizon a little before the third sunrise, its phase nearly full and another eclipse being about to take place; while *two* have taken place during one passage of the satellite from the eastern to the western horizon. All this has been upon the assumption that the orbits of the satellites are in the same plane as that of Mars about the Sun. The inclination of their orbits to this plane may, however, be approximately taken, for convenience' sake, as about 30°.

Phobos may therefore be elevated above the line joining the centres of Mars and the Sun 5800 miles, multiplied by the tangent of 30°, and Deimos by 14,500 miles, multiplied by the same tangent. This tangent being $\frac{1}{\sqrt{3}}$, these products amount respectively to about 3300 miles and 8300 miles.

The radius of the shadow of Mars being taken as 2100 miles at the distance of the satellites, it is evident that they both may often escape an eclipse altogether. Upon an average Phobos may escape in about one revolution out of every three and Deimos in about three out of every four.

All this is doubtless a very rough and elementary method of investigation; but I think it is sufficient to show that if there be any observers on Mars, the eclipses of the satellites will not be so frequent and long continued as to prevent their being seen far more frequently uneclipsed and in suitable positions for observation. There are many other interesting points connected with these bodies, one of the most remarkable perhaps being the enormous size, the wondrous amount of light which Mars would afford, the beautiful detail of its surface which would be visible to any observer upon Phobos or Deimos. It would fill up in the one case nearly $\frac{1}{4}$, in the other about $\frac{1}{11}$ of the width of the celestial hemisphere. Its area would be in the two cases respectively about 6400 times and 1000 times that of the Moon as seen by us; its light some 2500 times and 400 times as great.

We must not, however, enter into further details. Any one may easily work them out and find in so doing a useful exercise of ingenuity. Suffice it to call attention to the very interesting description given by Mr. Marth at the R. A. S. meeting of Nov. 1878, of the transit of the Earth and Moon across the Sun's disk which will be visible from Mars on Nov. 12, 1879, and of the frequently recurring transits of Phobos and Deimos.

E. Ledger.

Spectroscopic Observations of the Comet Palisa 1879 d.

About the time of the discovery of this new comet I was in Germany and Belgium, so that I saw it only on the 4th October at 7^h O'-Gyalla mean time.

The comet was large enough, but very washy and, for spectroscopic observations, a very faint object. With power 208 I saw it granulated and with a condensation in the middle.

I made an attempt to observe it with the spectroscope, but was not so successful as I wished. I was also often disturbed in my work by clouds, and later also by moonlight.

The spectrum of this comet was, in comparison with other telescopic comets, very faint; and I succeeded only in observing the spectrum with a very ingenious spectroscope which was made after the drawing of Professor H. C. Vogel, of Potsdam, in my observatory workshop, from an achromatic eyepiece by Rheinfelder at Munich, and three small direct-vision prisms by my friend Merz, of Munich.

I saw in the spectrum two very faint bright bands and suspected a third of the same character, which was nearer the more refrangible end of the spectrum; but this was so extremely faint that it was very difficult to identify its presence. As far as it was possible to decide, the bands were much more sharply defined than in the spectrum of Brorsen's comet.

The first band was in the yellowish green, the second at the limit of the green and blue; but as to the position of the third it is difficult to say any thing, since it is so faint that its presence even is doubtful.

There was also a very faint continuous spectrum present, but this was more like a misty lustre than a spectrum; it was not so narrow as in the Comet Coggia 1874, but as broad as the bands were long. The continuous spectrum was emitted not only from a nucleus or condensation, but also from the whole body of the comet. From this I should conclude that the reflective power of this comet was greater than that of other comets.

On October 6, I looked at the comet at 6^h 35^m M.T. with the 6-inch Merz refractor, the air being tolerably steady and transparent, and it was then possible to see well enough also the third band in the spectrum. Using a power of 140, it appeared granulated and I thought I saw in the centre a very small nucleus. I measured the diameter with a Browning filar micrometer and found it 2′ 42″. The comet's light was found to be polarized when examined with a quartz Nicol polariscopic apparatus.

After these preliminary observations, I observed the spectrum with the 10½-inch Browning reflector, comparing it with the spectrum of Geissler tubes containing Olefiant gas, and after finding a resemblance I began to make measures on the broad bands.

Nothing was more difficult than to make these observations, because, as soon as I illuminated the micrometer of the spectroscope, the broad bands disappeared. After much trouble, I illuminated the micrometer with a Geissler tube containing nitrogen; and so I measured each broad band four times, and found the follwing mean values in wave-length:—

I. 5598 tenth-metres.
II. 5156 „
III. 4887 „ (?)

To the third I attach a note of interrogation, because I do not attribute much exactitude to the measure of its position.

After this I measured also the three broad lines (resembling those of the comet) in a Geissler tube containing Olefiant gas (C_2H_4) and found from the mean of four sets of measures the following wave-lengths:—

I. 5577 tenth-metres.
II. 5166 „
III. 4845 „

Thus there is a difference (Comet $-$ C_2H_4) of $+21$ tenth-metres in the first band, -10 in the second, and $+42$ in the third (doubtful) in wave-length.

The continuous spectrum was very faintly visible between 5493 and 5201 tenth-metres of wave-length.

I do not attribute to these observations the same precision as to other observations of this sort; but I think there is no doubt that the substance composing the self-luminous parts of this celestial body is some other substance than Olefiant gas, for it is hardly possible for an observer to make so large an error of observation as to change to this extent the value of the numbers found.

The brightness of the comet diminishes and the atmospheric conditions are still very unsatisfactory for observation of this faint object near the west horizon, and I have therefore not continued to observe it further. NICOLAS DE KONKOLY.

O'-Gyalla Observatory, Hungary,
1879, October 16.

*The Brightness and Distribution of the Fixed Stars**.—Part I.

THOSE who view and admire the starry canopy above us—so fittingly associated, in the oft-quoted language of a great philosopher, with the moral nature of man—can hardly fail to remark how largely their pleasure in the grand prospect is due to the endless variety in its brilliancy. Just as the magnificence of mundane potentates is fully brought out only by the presence of a long train of inferiors more modestly arrayed, so Sirius and Capella would be less splendid had they not a multitude of lesser luminaries to heighten their glory by contrast. And how many hundreds of twinkling points, almost lost in the wide abyss, are there for every star of highest rank! In the proportion of common soldiers to captains and of captains to corps commanders, this silent host of heaven is not unlike the less stately armies that tread earth instead of ether. And if astronomers have hitherto interested themselves less in questions of precedence and seniority than in the particular spot on the field occupied by each individual in the great array when drawn up for review; if, dropping the figure, differences of lustre and the number of stars of the various grades have occupied less of their attention than the comparatively dry details of right-ascension and declination, with all the refinements of precession, nutation, aberration, proper motion, parallax, refraction, &c. affecting these—they are now making some amends for their neglect. The methodical study of stellar brightness belongs almost entirely, however, to the present century, Sir W. Herschel's first paper calling attention to the importance of the subject having appeared in the publication quaintly termed "Philosophical Transactions" of the London Royal Society in 1796.

Sir W. Herschel here mentions the number of variable stars, constantly increasing under new discoveries, very naturally predicts that closer observation will be likely to show variability in objects previously unsuspected, and recommends that careful comparisons be made from time to time between neighbouring stars all over the heavens, so that any change occurring may be at once detected. The original comparisons accompanying this paper have been of but little use, however; they are interesting chiefly as having been the first attempt to introduce scientific methods into this unexplored territory of the astronomical realm. They were made without the aid of any instrument, and consisted of such indefinite statements as—"Star No. 7 about equal to No. 4, and just perceptibly fainter, or decidedly brighter, than No. 12." The difference of brightness which Herschel considered as "just perceptible" seems to have been from one fifth to one fourth of a magnitude.

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* From the 'Popular Science Monthly' (New York), 1879, August.

That his least appreciable difference should have some constant relation to the traditional "magnitude" was to have been expected, bearing in mind what this oldest and most universal scale of reference was intended to express. The fixed stars were assigned to classes of brightness, we learn, before the Christian era; and the very term "magnitudes," used from the first to designate these classes, shows the state of knowledge under which the study had its origin, for, as we now know, the apparently greater size of the brighter stars is due only to imperfections of the eye. All visible stars—all that existed, that is, for the early astronomers of the Mediterranean—were included in six magnitudes, the first containing the dozen or score of brightest stars in the heavens, the second perhaps twice as many ranking next to these, and so on out in gradually increasing circles. The work of the ancients has in this case been well preserved, no modern innovator having been found bold enough to disturb this time-honoured system of reckoning. Still, as in the days of the "Father of Astronomy," the two chief stars of Orion serve as examples of the first magnitude, while his Belt and the Dipper in the northern sky furnish types of the second order. But, while astronomy was yet in its infancy, observers had noticed that the stars were not assorted into well-defined orders, in which all the individuals were equally bright; and so, in assigning to a star its magnitude, they would often add that it was "smaller" or "larger" than the mean of that magnitude. They thus practically trebled the number of their classes. The same division into thirds of a magnitude is still employed by those who judge of brightness by eye-estimates, though some are content with dividing into halves, and some undertake to be exact to tenths. Now, even though no scientific precision was attained, or even thought of, in this original apportionment of visible stars among the six magnitudes, to which all later estimates are adjusted, we would expect to find one principle underlying such a classification, making it that of greatest convenience. It must be just as easy to tell a fourth-magnitude star, for instance, from a fifth-magnitude as from a third, and there must be as little doubt in distinguishing between the fifth and sixth magnitudes as between the third and second. The numbers expressing magnitudes, then, must actually represent a scale of equal differences as measured by the sensibility of the eye. When an astronomer pays attention to differences of lustre, measured also by the sensibility of the eye, but closer than the founders of the science cared to notice, he naturally finds that he can distinguish the same number of intermediate grades between two adjoining magnitudes, whether faint or bright. Herschel's estimates, having been of this character, are, as we have seen, subject to the same condition.

The system of comparisons introduced by Herschel was not followed by later astronomers. Determinations of brightness in which accuracy is sought are now made by means of instruments constructed expressly for the purpose. These instruments, called photometers—measurers of light, that is, their office being to show the amount of light that one star gives as compared with others—add nothing to the discriminating power of the eye, it should be stated. In deciding a question as to which is the brighter of two stars situated sufficiently near together, no appliance yet invented can assist. But they have these three advantages: they facilitate comparison between faint stars, they furnish a means of comparing distant stars, as though side by side, and they give results in a numerical form. That is to say, we get by means of them a definite difference, which may be expressed as a fraction of a magnitude. The magnitude, we see, is no longer regarded as a class, but as a fixed point on a continuous scale—a striking example of that progress of science in all its branches from a qualitative to a quantitative stage, on which philosophers delight so to insist.

But how are measures of light to give us fractions of a magnitude? How can the vague, qualitative relation, the brighter the light the higher the magnitude, become an exact and quantitative one? The discussion of this question may be of use by showing that, even with matters of so uncertain a nature, science does not proceed by guesswork. It is a general law, that the human senses measure ratios and not differences. If I am carrying a small weight, for example, and the addition of an ounce is required to make the burden perceptibly heavier to me, two ounces will have to be added in order that I may notice a difference when I carry twice the weight, and a whole pound when I carry sixteen times the weight. Similarly with the other senses, and, in no slight degree, with the emotions as well. Sensibility to grief and joy, as the experience of every one will attest, becomes feebler with an increase of the amount sustained. So, a faint sound can be heard only in comparative silence, and our footsteps surprise us by their resounding din on the floor of an empty hall, though no louder, as reflection easily assures us, than when the hall is filled with a bustling multitude. So, though the stars give us their whole light in the daytime, our eye, with the stimulus of an illuminated atmosphere, fails to discover them. This law, first stated by Fechner, is, in mathematical language, the excitement of a nerve varies in arithmetical progression as the exciting cause varies in geometrical progression, or degrees of sensation correspond to logarithms of the quantities perceived. Since, as just shown, the scale of magnitudes is that of equal differences in sensation, it must be at the same time that of equal ratios of light. We must thus have a constant light-ratio between each magnitude and the one next it, and these magnitudes must be logarithms of the quantities of light given, this ratio being taken as the base of our system. In fact, one of the first discoveries in photometry was that such a ratio actually exists; that, for example, if each star rated as third magnitude by good observers gives as much light as 2½ stars of the fourth magnitude, a star of the fourth equals 2½ of the fifth, and so on. Here was a practical confirmation of the character ascribed to ancient estimates of magnitude, and, at the same time, of Fechner's law.

This relation affords us the means of substituting exact measurement for estimates on an ill-defined scale by different observers, among whom a perfect agreement as to standard is out of the question. The idea that each observer has of the meaning of second or fifth magnitude is derived entirely from tradition and confirmed by habit, very much as are his notions of the significance of ordinary adjectives of degree—the only precaution observed being to alter the estimates of antiquity as little as possible, a vague limitation at best. Measures with the photometer depend no less on estimates with the eye, but the determination in them, as to the exact agreement of two lights, is subject to far less uncertainty.

Photometers agree in this particular, whatever their differences in mechanical construction. Seidel, of Munich, who was twelve years in comparing the light of but 208 fixed stars, used an apparatus where two stars seen through a telescope with divided object-glass, each out of focus, were made of the same brightness to the eye by diffusing or concentrating the light of one of them, its half of the object-glass being drawn out or in. The stars thus appeared as two disks, of different sizes but equally bright, and the amount of light given by each was taken as proportionate to the area covered by its disk. The same Dr. Zöllner who has lately become so conspicuous in "spiritualist" investigations, invented a much more convenient style of photometer, with which he made some interesting researches into the comparative light of the planets. Other astronomers, European and American, have also used it. With one of these instruments, belonging to the observatory of Harvard University, Mr. Peirce finished, a few years ago, perhaps the most extensive and methodical photometric work that has yet been done. His measures included the visible stars, about five hundred in all, of the zone between 40° and 50° of north declination—those passing overhead in the Northern United States and Canada. The light of a kerosene lamp, in Zöllner's photometer, shines through a small round hole in a thin metallic plate, so as to form an imitation of a star, slightly brighter than the real stars with which it is compared. The light of this artificial star, having been polarized by passing through one Nicol prism, is partially cut off by turning another. The proportion of light so cut off depends on the angle of the second prism from parallelism with the first. Having thus found the amounts of light given by two or more stars, as compared with a fixed light, their differences of magnitude are calculated by applying the rule involving logarithms, alluded to above.

Owing to the labour involved in making any large number of photometric comparisons, the less accurate but more convenient method of eye-estimates has not yet been entirely superseded. It becomes necessary, then, to find some way of reducing different observers to one uniform scale, in order to have their work available for determination of variability and questions of distribution. Mr. Peirce, in his 'Photometric Researches,' recently published by the Harvard College Observatory, has shown that this may be done by the simple process of counting. When we find in any catalogue a star recorded as of magnitude 4⅓, say, though we cannot tell exactly what degree of brightness this figure denotes, we yet know something definite, namely, that this observer classes the star in question as fainter than those he calls 4 and brighter than those he calls 4⅔. And we know something more: if in the northern hemisphere—a limited part of the heavens must be taken for the purpose, few catalogues being complete in southern stars—he classes 200 stars in all as brighter than 4⅓, while he calls 25 stars 4⅓ exactly, he means to tell us that his 4⅓-magnitude stars would all fall between 200 and 225 on a list of northern stars arranged in order of brightness. It is by considering the order which stars would follow when so arranged, leaving entirely out of view the numbers by which their magnitudes are expressed, that Mr. Peirce brings all observers to a single standard of reference; for he is justified in assuming that each of them attaches the same idea of brightness to the 50th or 150th star in his order, as an assumption of some such nature must be made to have their estimates of any service at all. To reduce to magnitudes these numbers expressing order of arrangement, we have to notice that equal ratios among them correspond to equal differences in magnitude. If we take a good catalogue and find the number of stars in it brighter than 2·0, and add to this number successively the number between 2·0 and 3·0, 3·0 and 4·0, and so on, we shall find that our series of numbers increases geometrically, the common ratio being nearly 3⅜. This is a remarkable fact, but it is not difficult to account for, on the supposition that the stars are uniformly scattered throughout space, or that portion of space in which visible stars are situated. In this case, the number of them out to any distance from our solar system must vary as the cube of that distance, while their light, supposing no important variations in real size and brightness among them, is inversely proportionate, in the mean, to the square of their distance. And since we have a constant ratio of light between each magnitude and the next, we must accordingly have a constant ratio of mean distance, equal to the square root of this ratio inverted, and a constant ratio of number, equal to the cube of the ratio of distance. Mr. Peirce adopted the ratio 3⅜. While introducing no perceptible change in the traditional magnitude-scale, except to rid it of irregularities, this number has the convenience of being exactly the cube of 1½. Considering differences in brightness as due exclusively to differences in distance, we may conclude that a star of the second magnitude, for instance, is just half as far again from us as one of the first, and two thirds as far as one of the third. The magnitude of any star, then, is to be regarded as a logarithm of the number expressing its ordinal rank, 3⅜ being the base of the system. We may thus find to what magnitudes the ordinal numbers, 200 and 225 in the example given, correspond, and take these as the superior and inferior limits of our observer's magnitude 4⅓. The probable corrected magnitude may be considered as halfway between these limits; and we cannot be more exact than this in our reduction, because his discrimination has not been close enough to admit of it. HENRY FARQUHAR.

[To be continued.]

Double Stars for November.

Σ 183. R.A. $1^{h}\ 48^{m}{\cdot}3$, Dec. +28° 13′. Mag. 7·5, 8·2.

A B	1864	9°·1	Oval.	De.
A B and C	1864	165 ·7	5″·7	„

A B probably binary.

Σ 186. R.A. $1^{h}\ 49^{m}{\cdot}7$, Dec. +1° 15′. Mag. 7·2, 7·2.

1877·0 Single.

The common proper motion is +0″·09 in R.A. and +0″·22 in N.P.D.

Σ 185. R.A. $1^{h}\ 51^{m}{\cdot}0$, Dec. +74° 45′. Mag. 7, 8·5.

1872 33°·8 1″·79 OΣ.

Σ 196. R.A. $1^{h}\ 52^{m}{\cdot}9$, Dec. +20° 26′. Mag. 8·5, 11.

A B	1877·7	52°	2″·49	β.
A C	66·8	166 ·3	35 ·97	De.
A D	62·9	361 ·6	182 ·5	Da.

OΣ 37. R.A. $1^{h}\ 55^{m}{\cdot}2$, Dec. +80° 56′. Mag. 7, 9.

1867·7 214°·6 1″·41 De.

Probably binary.

Σ 231. R.A. $2^{h}\ 6^{m}{\cdot}6$, Dec. −2° 57′. Mag. 6, 7·8.

1877 231°·0 15″·55 Fl.

Binary.

Σ 232. R.A. $2^{h}\ 7^{m}{\cdot}7$, Dec. +29° 50′. Mag. 7·5, 7·5.

1869 248°·4 6″·41 Du.

Probably binary.

Σ 234. R.A. $2^h\ 8^m{\cdot}6$, Dec. +60° 48′. Mag. 7·8, 8·7.
1872 221°·8 Elong. De.
Binary.

β 437. R.A. $2^h\ 12^m{\cdot}4$, Dec. +3° 39′. Mag. 8, 12–13.
1877·9 31°·5 7″·24 β.

o Ceti (β discovered B). Mag. B 13, C 10.
A B 1877·8 91°·5 74″·11 β.
A C ·8 82 ·8 115 ·62 „

JOSEPH GLEDHILL.

Mr. Edward Crossley's Observatory,
Bermerside, Halifax.

Meteor Notes for November *.

THE Andromedes (25° +43°) should be looked for on every fine night between the 24th and 29th, as there is reason to expect a considerable display this year. Moonlight will, however, interfere to no small extent, so that even if the shower occurs with exceptional intensity, only the brighter meteors may be well observed. The place of the radiant, the horary number of meteors, and the time of maximum are the chief points requiring attention. Other details, such as the apparent lengths of the paths and average duration of flight, should also be noted, though the brightness of the moon will greatly embarrass the observer, and in any case render the expected display far inferior to that of its predecessor in 1872.

On the mornings of the 13th–16th a watch should be instituted for the Leonids, though the portions of this stream encountered by the Earth during the last few annual returns have been of extreme tenuity and the radiant-point scarcely to be recognized. But though the prospect of a rich apparition of these meteors is very meagre, yet the observations should be annually sustained at this epoch to trace either the extinction of the shower or its intermittent revivals recorded in past years. No Leonids were seen last year, and in 1877 only 4 were recorded out of a total of 79 meteors seen on the mornings of Nov. 11–14 at Bristol. But in America Prof. Kirkwood wrote that "two observers saw 54 shooting-stars on the morning of Nov. 14, 1877, between 1.55 and 3.45 A.M., being at rate of 30 per hour. Nearly all of them were Leonids, a few being as bright as 1st mag. stars, with trains lasting several seconds." Observers who look annually for this shower should remember that there are several other well-defined streams of swift streak-leaving meteors proceeding from the same region, and that unless great care is exercised the meteors are ascribed to the wrong shower. These co-Leonid centres are at 141° +28°, 130° +20°, 147° +5°, 155° +36°, 133° +48°, 125° +40°, and 133° +31°.

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* See 'Observatory,' Vol. i. p. 227, and Vol. ii. p. 242, for further notes on November showers.

The Taurids should be looked for on about the 8th and 20th–23rd, and during the interval (7th–21st), nearly free from moonlight, the many contemporary showers of this period may be reobserved.

Fireballs are more than usually frequent on the nights of Nov. 1st–2nd, 4th, 6th–9th, 11th–15th, 19th, and 27th. The 19th is especially prolific in brilliant meteors.

Recent Observations.—270 shooting-stars were observed by the writer at Bristol during the period from Sept. 14–25; but there were only two really good nights for observations, viz. the 14th and 21st, when the following numbers were recorded:—

September 14.		September 21.	
Hour.	Meteors.	Hour.	Meteors.
10½–11	3	11–12	13
11 –12	18	12–13	19
12 –13	15	13–14	22
13 –14	24	14–15	23
14 –15	14	15–16	8
15 –15½....	5	16–16½....	7
5 hours.	79	5½ hours.	92

Sky very clear on both nights. There had been dark clouds and showers of rain in the early evening of Sept. 21; but at 11 P.M. the sky cleared and the stars shone with exceptional brilliancy until daybreak. On the 14th, between $13^h\ 39^m$ and $13^h\ 49^m$, no less than 10 meteors were seen. On the 21st, $15^h\ 4^m$ to $15^h\ 33^m$, not one was registered, though the horary rate of apparition during the preceding four hours had been about 20!

On the 15th the sky was watched from 8^h to $10^h\ 45^m$, when increasing fog obscured the stars. 26 meteors were seen in the 2¾ hours.

On the 20th, 23 meteors were recorded in 2 hrs., between 12^h and 15^h. The sky was cloudy between 13^h and 14^h, though pretty clear at other times.

On the 25th, 39 meteors were noted between $13\frac{1}{4}^h$ and $16\frac{1}{2}^h$. The evening was very stormy, though soon after 11^h the sky became clear enough for watching, but only five meteors were seen in the bright moonlight before 13^h.

There was no radiant-point of very decided intensity denoted by these observations. The meteors seem rather to have belonged to a large number of distinct though somewhat feeble streams, of which several were determined with precision.

		°	°	Meteors.	
I.	Sept. 14–25..	99	+43	15	Very swift bright meteors with streaks.
II.	Sept. 14–21,,	88	+43	8	
III.	Sept. 15–25..	30	+36	16	Not swift; max. Sept. 15; Triangids.
IV.	Sept. 14–25..	31	+19	14	Slowish; max. Sept. 21; 10 meteors.
V.	Sept. 14–21..	76	+44	11	Swift with streaks at α Aurigæ.
VI.	Sept. 14–25..	76	+32	10	Two showers of Aurigids.
VII.	Sept. 20–21..	192	+79	9	Very very slow; all 2–3 mags.
VIII.	Sept. 14–25..	82	+75	8	Rather swift; streaks.
IX.	Sept. 15–25..	355	+18	8	Slowish brilliant meteors; ? 2 fireballs.
X.	Sept. 21–25..	31	+52	5	Slow; faint meteors =D. (77), 134.
XI.	Sept. 14–21..	76	+56	7	Meteors not very swift; max. Sept. 20.

Two morning showers were seen before dawn on Sept. 26, at 103° +11° and 108° +24°. They were also traced in 1877, Sept. 15–16, at 101° +11° and 113° +24°. Nos. I. and II. are close together, though evidently distinct showers.

The shower at β Aurigæ (No. II.) was also observed last year on the morning of Sept. 26, at 87° +42° (see 'Observatory,' Vol. ii. p. 143).

The radiant No. VII., at 192° +79°, is remarkable as having supplied some of the slowest meteors ever seen by the writer, the duration ranging from 3 to 6 seconds.

A radiant-point preceding β Ursæ Minoris was seen by Sawyer on Aug. 10–11 this year, at 180° +78°, and by the writer at 215° +76° (bright slow meteors), and a position was also derived from the Italian observations (1872) at 180° +75° (10 meteors), Aug. 24–Sept. 14. In 1877, on Oct. 2–18, an exact radiant-centre of a few small slowish meteors was denoted at 186° +71°.

The position No. IX. of a shower of Pegasids agrees very closely with a radiant seen by Corder on Sept. 21, 1876, at 352½° +16½. Two small bolides, low in Cetus, were recorded this year with paths as follows:—

Sept. 21..	14h 36m	= ♀.	From 28° −19° to 37° −27°	11½°
25..	13 36	= ♀.	From 48 −16 to 58 −22	11

In each case there were two outbursts, and bright streaks remained upon the courses.

These meteors give a projected radiant at 339° +32° (just N. of η Pegasi), and it is therefore doubtful whether they belonged to Corder's shower, though a very slight error in the path-directions would originate the discordance in the positions. Two radiants of September fireballs were deduced by the writer (see 'Observatory,' No. 29, p. 131), at 332° +31°, September 6–13, and 335° +37°, September 25; and the latter of these accords remarkably well with that of the two fireballs recently recorded from the point 339° +32°, September 21–25, 1879.

The shower No. X. in the list, at 31° +52°, situated a little S. of χ Persei, gave meteors of very similar appearance to that of a stream detected on October 8, 1877, at 32° +50°.

Prof. A. S. Herschel, at Edinburgh, observed a fine meteor on Sept. 23, 8h 10m P.M., crossing the star ζ Ursæ Majoris. "As it fell almost vertically downwards it increased in size and brightness, until in the middle of its course it surpassed by two or three times the apparent brilliancy of Venus, and so it continued until near the end of its course, when it faded out not very suddenly. Its colour was steel-blue, and it had little if any tail, and left no streak upon its course. The apparent track of the meteor was from ζ Ursæ Majoris towards, and to within four or five degrees of, a point one or two degrees left of the star ξ Ursæ Majoris, which was just faintly discernible in mist quite close to the N. by W. horizon. The whole duration of the meteor's flight was between 3 and 4 sec.; length of path about 25 degrees."

Mr. E. F. Sawyer at Boston, Mass., found the shower of Perseids on Aug. 10–11 a very ordinary one. The best radiant was at 44½° +57°. On Aug. 11 a stationary meteor and 7 other paths gave a radiant at 38° +60°. He determined other centres at 5° +57°, 17° +42°, 23° +37°, 180° +78°, 344° +42°, and 358° +55°, from 4 to 7 meteors each.

Mr. H. Corder observed a shower of Draconids on about Aug. 16 from the point 286° +61° (10 meteors). At the end of August he recorded 31 paths from ill-defined showers in Perseus and Auriga, with probable radiants at 62° +39°, 79° +53°, 45° +60°, and 29° +48°. On Sept. 7 there were 6 meteors from 45° +34°, and on the morning of the 21st a position at 46° +48° (α Persei) was well ascertained from 5 swift meteors.

W. F. Denning.

CORRESPONDENCE.

To the Editor of 'The Observatory.'

The Inner Satellite of Mars.

Sir,—

My paper on "A Tidal Theory of the Evolution of Satellites" was merely a short abstract, and I intended to leave the full investigation (of which I am correcting the proofs at present) to explain the theory in detail. I will, however, say, in answer to Professor Kirkwood's query, as to how the theory would explain the fact that the Martian day is longer than the month of the inner satellite, that such a result must necessarily follow from the effects of solar tidal friction. I hope at some future time to examine the case of Mars numerically.

I am, Sir, yours faithfully,
G. H. Darwin.

Trinity College, Cambridge,
Oct. 1, 1879.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

No. 32. DECEMBER 1. 1879.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, 1879, November 14.

LORD LINDSAY, M.P., F.R.S., &c., *President*, in the Chair.

Secretaries: Mr. J. W. L. GLAISHER, M.A., F.R.S., and
Mr. A. COWPER RANYARD, M.A.

MR. GLAISHER reported that since the last Meeting of the Society 169 presents had been received, amongst which might be mentioned Professor Challis's 'Lectures in Practical Astronomy;' 'Handbook of Double Stars,' by Edward Crossley, Joseph Gledhill, and J. M. Wilson; a book on "Auroræ," by J. Rand Capron; a present from Dr. Henry Draper of the original negatives of photographs relating to oxygen in the Sun; also photographs of the Moon, presented by Dr. Gould of Cordoba. Thanks were voted to the Donors.

Prof. Adams read a note on the Ellipticity of Mars, and its effect on the Motion of the Satellites:—It is shown by Professor Asaph Hall's able discussion of his observations of the satellites of Mars that the planes of the orbits of both satellites are nearly coincident with the planet's equator. It becomes interesting to inquire whether this state of things is a permanent one or whether the present near approach to coincidence of the three planes in question is merely fortuitous.

The equator of Mars is inclined to its orbit at an angle of about 27°; and therefore if the orbits of the satellites preserved a constant inclination to the orbit of Mars, as they would do if the Sun's action were the only disturbing force, their inclinations to the planet's equator, and still more their inclinations to each other, would in the course of time become considerable.

In No. 2280 of the 'Astron. Nachrichten,' Mr. Marth has found the motion of the nodes of the orbits of the satellites on the orbit of Mars due to the Sun's action; and he concludes that unless there is some force arising from the internal structure of Mars which counteracts or greatly modifies the Sun's action on the nodes of the orbits, those nodes will be in opposition to each other about a thousand years hence, when the mutual inclination of the orbits will amount to about 49°.

It is well known that if there were no external disturbing force, the effect of the ellipticity of a planet on the orbit of a satellite would be to make its nodes regrede on the plane of the planet's equator, the orbit preserving a nearly constant inclination to that plane. Laplace has shown that if the effects of the Sun's action and of the planet's ellipticity be both taken into account, then the orbit of the satellite will preserve a nearly constant inclination to a fixed plane, which passes through the intersection of the planes of the planet's orbit and its equator and is intermediate between these two planes. The angles which the fixed plane makes with the latter planes depend on the ratio of the rates of retrogradation of the nodes caused respectively by the action of the Sun and the ellipticity of the planet. If, as in the case of our Moon, the Sun's action produces a much more rapid motion of the nodes than is caused by the planet's ellipticity, then the fixed plane will very nearly coincide with the planet's orbit. But if, as in the case of the inner satellites of Jupiter, the motion of the nodes caused by the planet's ellipticity is much more rapid than that due to the Sun's action, the fixed plane will nearly coincide with the planet's equator. Now I find that the case of the satellites of Mars is similar, in this respect, to that of the inner satellites of Jupiter. The mean distance of the first satellite from the planet is only about $2\frac{3}{4}$ of the planet's radii, and that of the second satellite only about 7 such radii, while the periodic times of both are very small compared with the period of Mars. Hence it arises that for both the satellites the rate of motion of the nodes due to ellipticity greatly exceeds that due to the Sun's action. It is true that the actual ellipticity of Mars is still unknown, and it is probably too small to be ever capable of direct measurement; but we are not without means of determining the probable amount of this ellipticity within limits which are close enough fully to justify the assertion just made. From the observed ratio of the apparent diameter of Mars to the apparent distance of either of the satellites, combined with the ratio of the known time of the planet's rotation to the periodic time of the satellite, it may be deduced that the ratio of the centrifugal force to gravity at Mars's equator is about $\frac{1}{220}$. Hence it follows that if the planet were composed of homogeneous matter the ellipticity would be about $\frac{1}{176}$. If, on the other hand, the density followed the same law as in the case of our earth, the ellipticity would be about $\frac{1}{228}$. In all probability the actual ellipticity lies between these limits.

The following Table shows the annual motions of the nodes of the two satellites, due respectively to the Sun's action and to the ellipticity of the planet, supposing that ellipticity to have either of the values just mentioned. It also shows the corresponding inclinations of the fixed planes to the planet's equator.

	Annual motion of node due to Sun's action.	Annual motion of node due to ellipticity, supposing		Inclination of fixed plane to planet's equator, supposing	
		Ellipt. = $\frac{1}{118}$.	Ellipt. = $\frac{1}{228}$.	Ellipt. = $\frac{1}{118}$.	Ellipt. = $\frac{1}{228}$.
Sat. I.	0°·06	182°	113°	31″	50″
Sat. II. ..	0·24	7°·35	4°·57	50′	1° 19′

From this table it may be inferred that the fixed plane for the first satellite is inclined at less than 1′ to the plane of Mars's equator, and that the inclination of the fixed plane for the second satellite is about 1°.

The ellipticity will also produce rapid direct motions of the apses of the orbits, especially in the case of the first satellite. According to Professor Hall's determination, the orbit of this satellite has a sensible eccentricity, and therefore future observations will enable us to determine the motion of the apse, and thence to find a very accurate value of the ellipticity.

If further observations should show that the orbits of the satellites are sensibly inclined to their fixed planes, it will be possible to determine the motions of the nodes on their respective fixed planes, and thence to deduce another value of the ellipticity.

It is extremely desirable that the few observers who are in possession of adequate means should obtain as many good observations of the satellites as possible during the present opposition of Mars, so that no opportunity may be lost of throwing some light on both of the points which have been mentioned above.

Mr. Marth. The inclination of the equator of Mars to its orbit has till lately been assumed to be 27° or 28°; but the observations of 1877 made by Asaph Hall and Schiaparelli tend to show that a considerable correction is required. Of course, observations made during one opposition only merely indicate the direction of the pole of the planet's equator; and it is necessary, in order to find the pole itself, to have also observations in oppositions when the place of Mars is considerably distant. If one makes fair allowance for this, I think one can only deduce that the equator of Mars is really almost coincident with the planes of the orbits of the satellites. One difficulty about it is that Hall and Schiaparelli disagree somewhat; and it is to be hoped that the discrepancy may be cleared up, and that careful observations are being made for determining what is the position of the axis. As regards the effect of the ellipticity, it is possible perhaps that in 1881 the motion of the node will be already perceptible; but if this year's observations are not far more abundant than at present there is any sign of, I am afraid that they will not be sufficient to fix the node, and we shall have to wait until the year 1890. The opposition in 1890 will not be favourable for northern observers; but no doubt 1892 or 1894 will decide this question to a degree of accuracy which we have hitherto had no chance of hoping for. I may mention with regard to Mr. Common, that up to this time he is the only observer in Europe who has seen both these satellites. Deimos is behind its predicted place about 4°, and Phobos in advance about 50°, and the time of revolution of Phobos must be shortened by a second and a half. I can only hope that Mr. Common will be successful in following both satellites as long as possible. Unfortunately, in order to fix the node with any degree of certainty, it is necessary to get observations very close to the planet. Mr. Common goes near, but not sufficiently so as to fix the node. There is no question that these satellites are a great treat from more than one point of view; and in time they will enable astronomers to determine the ellipticity of the planet with an accuracy which could not have been hoped for.

Mr. Stone, who was received with a very cordial welcome, read a paper on the evidence of a past connexion between four widely separated southern stars. It appeared that the four stars ζ^1 and ζ^2 Reticuli (distant 5′), *e* Eridani (distant 19° from ζ Reticuli) and ζ^2 Canis (distant 33°) had a common proper motion amounting to 189″ since Lacaille's time, and that both *e* Eridani and ζ^2 Canis were very slowly separating from ζ Reticuli and from each other, the velocity of separation being greater the greater the distance of the stars. Thus they form a triangle, which may have been closed up at some time, perhaps 400,000 years ago.

Also a paper on the comparison between the R.A.'s and N.P.D.'s of the 'Nautical Almanack' and the Cape Catalogue for 1880, in the course of which he observed:—In my Cape work the 'Nautical Almanack' stars have not been so carefully observed as a greater number of small stars; but a considerable number have been observed, and the results are interesting on one point. The refraction tables I used at the Cape were Bessel's refraction tables, without the change which he introduced in passing through Z.D. 85°. Individually I cannot think that such a change is either theoretically or practically admissible. It seems to me absurd to introduce an arbitrary break in the refractions as you pass a zenith-distance like 85°. If Bessel's refraction tables do require it, then I should say that undoubtedly the theory is incomplete or defective, and we should see whether we cannot get something better in the way of a set of refraction tables than those at present in existence. I divided the stars into four groups of N.P.D., and the corrections to the 'Nautical Almanack' N.P.D.'s are:—

N.P.D.	40°–60°	60°–90°	90°–105°	105°–124°
Corr. to N.A. ..	−0″·33	−0″·35	−0″·15	−0″·40

These are only mean results for the groups; there may be larger residuals at certain parts of the circle. We may, I think, apply the mean correction −0″·31 to all of them. If we do that it becomes a point of interest that they should be put in order of R.A.; and when put in order of R.A. the following result came out:—

R.A.	0^h–6^h	6^h–12^h	12^h–18^h	18^h–24^h
Corr. to N.A.....	−0″·24	−0″·11	+0″·30	+0″·06

Considering the seasons at which the stars in the several groups are observed, the result will amount to this, that in the dry season that we get in December, January, and February, our refraction tables are relatively too big, whilst in the wet season that we get in May, June, and July the refraction tables are relatively too small. Though there is a great change of temperature contemporaneous with this change of moisture, it is yet extremely probable that the humidity is a sensible factor in the refractions. Certainly this observation shows that the correction for temperature has not been sufficient to carry us over the complete reversal that takes place between northern and southern observatories. The barometer used was the standard barometer of the Observatory, and the thermometer was the dry-bulb thermometer and had an error of about +0°·55. I have not applied it, because I have compared the N.P.D.'s amongst themselves, and they agree sufficiently closely to satisfy me, and there I have left it. The refractions practically are not exactly Bessel's mean refractions, but Bessel's mean refractions diminished by something very small.

Mr. Waters. With reference to Mr. Stone's first paper and the very interesting work which he showed, will he be good enough to say whether the stars which are moving with a common motion are all of the same magnitude?

Mr. Stone. They are nearly all of the same magnitude, viz. the 4th and 5th magnitude. So far as I could infer the masses from the brightness, there would be no very great difference in the masses.

Mr. Dunkin. What is the angular distance between the stars?

Mr. Stone. The first two are only about 5′; the others about 19° and 33°.

Mr. Dunkin. That seems a great distance for the stars to be physically connected with each other.

Mr. Stone. Yes, it does; but still their relative proper motion might bring them together in a period of about 400,000 years—a period which geologists would not consider long. (Laughter.)

Mr. Ranyard. I want to know whether the relative proper motions are sufficiently large to enable one to say that their directions all meet at a point.

Mr. Stone. No. I merely show that the resultant of the proper motions of each star relatively to the other two falls within the angle of the triangle formed by the three stars. The observations are not sufficient to determine the term depending on the square of the time which enters into the expression of the proper motion.

Mr. Dunkin. It ought to be understood that the 'Nautical Almanack' places compared with those given by Mr. Stone are really the Greenwich places from the 1864 catalogue; so that when Mr. Stone compares them with the 'Nautical Almanack' he compares the Cape with Greenwich.

Mr. Christie read a paper received from Prof. J. C. Draper on a photograph of the solar spectrum showing fine dark lines in certain spaces near G, which correspond in position with bright lines of the spectrum of oxygen. [The photograph, which was taken with a Rutherfurd diffraction-grating heliostat and concave mirror, was exhibited at the meeting, and Mr. Christie indicated on the black-board the positions of the fine absorption lines which Prof. J. C. Draper considered to represent the spectrum of oxygen in the Sun.]

Mr. Ranyard. Did I understand that there has been no attempt to photograph the spectrum of oxygen, but that this has been entirely work upon the solar spectrum?

Mr. Christie. So far as this work is concerned. Mr. Draper has simply sent me the photograph with a description. In this photograph there is no comparison with the spectrum of oxygen.

Mr. Ranyard. I take it that this is no new experiment with regard to the spectrum of oxygen. This is merely a theory of Prof. Draper's, that under certain conditions the oxygen lines would break up, but with bright spaces opposite the lines where oxygen shows with any dark lines which happen to be upon those spaces.

Mr. Christie. The experiment is with regard to the solar spectrum. In this paper there is no comparison with the oxygen spectrum. But I omitted to mention that Prof. J. C. Draper calls attention to the fact that those spaces between the dark lines in the Sun are actually less bright than other spaces on the more refrangible side of G; and from that he infers that there is a certain absorption in those spaces, and that, as there are oxygen lines there, that absorption must be due to oxygen.

Mr. De la Rue. I was recently in Hamburg, and I there saw the mounting of a telescope for Dr. Winnecke, at Strassburg, made by Messrs. Repsold. It is a remarkable piece of mechanical engineering. I think it will prove extremely rigid, but at the same time very mobile. But I do not wish to pass any eulogium upon this mounting, but only to call attention to a new conical pendulum which the Messrs. Repsold were so good as to describe to me, and which I think will solve, or at all events go far in the direction of solving, the very important problem of a conical pendulum, which will give a true and continuous motion without any very great variation of rate. The pendulum is a steel rod, with a ball and a prolonged end, driven by a crank with a slot in it. When at rest, this rod will be vertical, with the end engaged in the slot, and capable of moving along it according to the velocity. This rod is clipped quite tight at the top, as if held in a vice. When motion is given to the pendulum it will move conically, and will gradually move away from the centre. The main force which tends to bring the pendulum to the vertical is not the force of gravity, but the elasticity of the rod. In reality it is a conical pendulum very much upon the principle of the balance-wheel and spring of a watch. Messrs.

Repsold did not show me any experiments, because I had not time to see them; but they showed me a record of them; and it would appear from that, that when the driving weight on the clock was increased to 15 times its usual amount, the variation in the rotation was not more than 0·4 per cent. It really appears to me that this is the most valuable idea that we have had with regard to the solution of this very important point.

The President. As I understand, this is an instance of a pendulum which is rigidly fixed at the point of suspension. Therefore all the motion in the pendulum is got from the elasticity of the metal at the point of suspension.

Mr. De la Rue. Yes, and in the whole rod.

The President. This steel rod would have to be of extremely uniform diameter, otherwise it would move in an ellipse.

Mr. De la Rue. It was a circular rod; and I imagine that great care was taken in the construction of the pendulum in the hands of such skilful mechanicians as Messrs. Repsold.

The President. Is this a pendulum giving a 2-second rotation, or 39-in. pendulum?

Mr. De la Rue. That would entirely depend upon the gearing of the clock that was to drive the equatoreal. I think the pendulum was intended to be about 2 feet long.

A Fellow. If the action of the pendulum is not at all due to gravity, the length has nothing to do with the rotation; and if there is a weight at the bottom, as appears in the drawing on the board, I should take it that gravity has something to do with it.

Mr. De la Rue. It was more a *façon de parler* to show that the main force involved in bringing it to the vertical was the elasticity of the rod.

A Fellow. The rotation really depends upon the strength of the rod, and not upon its length.

Mr. De la Rue. That is perfectly true, but I have not gone into any of those questions; I have only given a short account of the pendulum.

The President. I am very much obliged to Mr. De la Rue. I must say that it opens quite a new field for pendulum work.

Mr. Neison. Did Messrs. Repsold drive it under very different temperatures?

Mr. De la Rue. The only experiment which Messrs. Repsold brought under my notice was the question of the differences of driving weight. It is not intended in any way to be a pendulum which is to go from year to year at the same rate, because that is easily adjustable. If we can get a pendulum which will drive a telescope one hour or two hours at a perfectly uniform rate, we shall have accomplished what has never been accomplished before.

The President. I think a pendulum can be made to do so very nearly perfectly; I am sure that mine does.

Mr. Maxwell Hall read a paper on the Nebula in the Pleiades, of which he had made a drawing, as seen in his 4-inch refractor at

Jamaica, where he was favoured with splendid skies. He finds the nebula to be elliptical in shape, with a major axis of 45′ and a minor of 30′, enveloping Merope and stretching in the *s p* direction or away from Alcyone. With respect to the supposed variability of this nebula, after summarizing the evidence he concluded that its visibility depends greatly on the state of the air, and not on large aperture of the telescope.

Dr. Schuster read a paper on the Polarization of the Corona:—

We have every reason to believe that polarization which has been observed in the solar corona has the same cause as the polarization of our atmosphere, and that it is therefore due to the scattering of light by particles which are small compared to a wave of light. We possess one or two measurements of the amount of polarization in the corona, and we know that this polarization is comparatively small near the edge of the Sun, increases at first as we move away from the Sun, reaches a maximum and then decreases rapidly. We have at present no knowledge of the meaning of these changes and of the amount of polarization which we have a right to expect if our explanation of its cause is correct. In the following paper I have endeavoured to supply this deficiency. I began the calculation in the hope of getting a rough idea only of the amount of polarization which we might expect; but it appeared that even such observations as we can make in the short time available during a total solar eclipse may yield most important information on the constitution of the solar atmosphere. I shall show how combined measurements of the polarization at different distances from the Sun, and of the decrease in intensity of the total light of the corona with increasing distance from the Sun, may furnish us answers to the following questions:—

1. In what way is the scattering matter distributed in the solar atmosphere?

2. What part of the light sent out by the corona is due to scattering matter?

3. Is the scattering matter projected outwards from the Sun, or is it falling into the Sun from outside?

Even if our observations are incomplete we may obtain rough answers to the two first questions, while the third will necessitate more careful measurements.

The first step in the investigation is necessarily the calculation of the polarization shown by a single particle when placed at different distances from a luminous sphere. I have already given the results of this part of the work in the 'Monthly Notices of the Roy. Astr. Soc.' Vol. xxxix. p. 46. The light scattered by a particle may be divided into two parts. The first is unpolarized, and the same as if the particle were self-luminous. This part decreases as the particle is removed from the centre of the luminous sphere. The second part of the light is partially or totally polarized, and is the same as if the luminous sphere were replaced by a luminous point, shining with an intensity which vanishes close to the luminous sphere, and increases with increasing distances from it. If a scattering particle is therefore placed close to the Sun, the light will be wholly unpolarized; if the distance from the Sun is large, the luminous sphere may be replaced by a luminous point.

Light scattered from an atmosphere of small particles is next investigated. The simplest supposition to make is, that the density of scattering particles does not vary with the distance from the Sun. The intensities of the two components of light (the radial and tangential component) and the polarization are expressed in the complete elliptic integrals of the first and second kinds. If the density of scattering particles varies inversely as any power of the distance from the Sun, the calculation becomes more complicated. If the power is even, the expressions can be reduced to elliptic integrals: if the power is odd, the integrations can be performed.

Without, however, completing the calculation, it can be proved that

The polarization must increase with increasing distances from the Sun, if the scattering matter is distributed according to any inverse power of the distance from the Sun.

Tables are added to the paper which give the amount of polarization at different distances from the Sun for various laws of distribution.

If the law of distribution of the scattering particles contains more than one term, the polarization does not necessarily always increase with increasing distance from the Sun; but it is shown that close to the Sun it always must. I have followed out the case that the law of distribution contains but two terms, each varying inversely as some power of the distance from the Sun's centre; the case of one of the exponents becoming zero is included. I have arrived at the following result:—

If the two terms are of opposite sign, the polarization will always increase with increasing distance from the Sun. If they are of the same sign, and if the two exponents differ by one only, the polarization will first increase, reach a maximum, and then decrease again. If the two exponents differ by more than two, the polarization will either steadily increase, or have first a maximum, then a minimum, and finally increase again. If the exponents differ by two, the behaviour will depend on the relative value of the constant factors.

A formula is given for the approximate calculation of the maximum of polarization for the case in which the constants are determined in such a way that the maximum is as near to the Sun as possible.

If instead of the luminous sphere we have only a luminous point, the investigation is much simplified, and we arrive at the two following laws:—

If the distribution of matter takes place according to any inverse power of the distance, the polarization is constant and independent of the distance from the luminous point.

If the distribution of matter surrounding a luminous point is expressed by the law $\frac{A_n}{r^n}+\frac{A_t}{r^t}$, *the polarization as looked at from a point outside the scattering atmosphere will steadily increase if* A_t *is negative, and steadily decrease if* A_t *is positive.*

In reality, as has been already said, the polarization of the corona shows a maximum at a distance of a few minutes from the Sun. It is, however, clear that this maximum is not due to any peculiar distribution of scattering matter, but to a large admixture of unpolarized light. The scattering particles themselves are no doubt self-luminous; we may, in addition, have light scattered by particles which are too large to polarize the light, or light due to luminous gases. The addition of this unpolarized light may easily produce the maximum which is observed. I first assume that this unpolarized light does not vary at different distances from the Sun. Under this assumption it is easy to find for a given position of the maximum, and for a given law of distribution of scattering particles, what must be the proportion of light due to the two causes, and also what the polarization at the maximum ought to be. A table is added to the paper in which these quantities are given. The most general case, which is what we have actually to deal with, is next investigated. It is shown how, by a combination of the polarimetric and intensity observations, we obtain a quantity which is a function of the distance from the Sun, and which *only* depends on the light due to scattering particles, and not on any addition of unpolarized light. Assuming the distribution of scattering matter to take place according to a law, $a+\frac{b}{r}+\frac{c}{r^2}$, we can, by constructing tables corresponding to the different terms in the expansion, find out what values of a, b, c, &c. agree most nearly with observation. Having found a law which agrees with observed facts, we easily deduce the relative amount of light due to the two causes at different distances from the Sun.

If our observations are sufficiently accurate, we may, for instance, find out whether in the above-mentioned expansion a is zero or not. If a has a finite value it will be exceedingly probable that the scattering matter is falling into the Sun; for if it was projected outward we should expect a rapid diminution and final vanishing of the quantity of scattering matter with increasing distance from the Sun's centre.

Mr. Ranyard. I have not had an opportunity of reading Dr. Schuster's paper at leisure; but from a hasty examination of it it seems that he has succeeded in making several new steps. I do not think, however, that it will be found to afford a means of determining the distribution of the matter of the corona, or that there is any simple law with regard to the distribution of the polarizing particles round the Sun. There seems to be no evidence tending to show that there is any concentric arrangement of the matter of the corona, and there is a great deal to show that a great part of the matter of the corona is arranged in great ray-like structures, some of which are nearly radial and some almost tangential; but there is nothing to show that there is a uniform distribution according to any simple law in the different parts of the corona as we pass away from the Sun's limb.

There are a great many interfering elements which cannot be eliminated. First there is the air polarization, extending all over the whole area of the corona; this no doubt varies in different eclipses according to the illumination of the clouds upon the horizon. There is also the possible incandescence of the light-dispersing particles to be taken into account, as well as the incandescence of the gas, which we know exists in the area of the corona; so that I do not think we shall ever be able to learn much from the polarization of the corona as to the distribution of the polarizing particles. But it is a most interesting field for investigation.

The President. The Society will join me in thanks to Dr. Schuster for his interesting paper. The Society will be happy to hear that Mr. Ranyard has been able to send the 41st volume of the Memoirs to the printer; and I hope it will soon be in a position to be presented to the Society.

Capt. Noble read a paper on two sketches of Jupiter showing the red spot. He remarked:—

In a paper read before the Society by Mr. Brett in June 1876, he directed their notice to the fact that the bright spot sometimes casts a shadow. On the night when my sketch was made the spot not only cast a shadow, but two shadows, which were visible one on its preceding and the other on its following side. I confess that no immediate explanation of this occurs to me.

Mr. Common. Did you observe the big spot?

Capt. Noble. Yes. Everybody has noticed the way in which the whole of the surroundings seem to conform to the contour of this spot, showing the enormous distance to which the disturbance indicated by it extended; but I am referring to the ordinary white spots. Mr. Brett put forward the theory that they cast shadows, and, I confess, sustained his theory by showing that in all cases the shadow fell where it ought to have fallen; but in my case, whilst I had a shadow where it ought to have been, I had a shadow where it ought not to have been.

Mr. Brett. I never intended to say that you saw a shadow. I never supposed that you would know one if you did.

Mr. Ranyard. There is an old drawing by Mr. Lassell in the 'Monthly Notices' with a shadow or dark marking all round a central white space; such markings have, I think, been spoken of as smallpox markings. I was at one time interested in the question of a possible connection between the bright markings on Jupiter and sun-spots, and I came to the conclusion that there was some evidence in favour of such a connection. In looking up this question I made a collection of copies of drawings of Jupiter which are now in the library of the Society. It is worthy of notice that sun-spots are beginning to be observed again; and here are these bright markings and the red appearance of the planet again in agreement with the theory. I should like also to remark that, as far as I remember, these elliptic markings have always been seen in the southern hemisphere of Jupiter.

Mr. Neison. Either the night or the night after Capt. Noble made his sketches of Jupiter, I was observing Jupiter under very good conditions, and I saw what Mr. Ranyard has been referring to—a white spot with a dark marking as nearly as possible all round it. There was at one end a long whitish streak, and at the other end a less distinct object, but a little shadow on one side. It was so utterly unlike the white spot on Jupiter that for nearly 20 minutes, I was not certain whether it was not a satellite in transit. It was simply a white spot, and there was a distinct dark shadow all round it which threw it up into relief.

Mr. Common. Is this the large spot that we hear so much about? (pointing to Capt. Noble's drawing of Jupiter on the black-board).

Capt. Noble. Yes, that is the large red spot.

Mr. Common. Then that ought to be much higher up. I have brought here a photograph of Jupiter which shows the true position of the spot. The night before last I saw the spot and there were a lot of markings between the two bands, and that white spot had a decided dark shading on one side. I noticed another thing, that the north band has lately got very much darker than it was a few months ago. I think Mr. Webb has alluded to that, and it is really worthy of attention.

Mr. Sadler. The shadows of these white spots, which resemble satellites in transit, were, I think, discovered about 20 years ago.

Mr. Stone. There were a large number of drawings made by Mr. Carpenter, with the great equatoreal at Greenwich, when I was there, which showed those markings very distinctly. They are apparently liable to very great changes.

Mr. Dunkin. In the 'Monthly Notices' about 20 years ago there are some drawings by Mr. Dawes and Mr. Lassell which show those bright markings very distinctly indeed. They are very interesting, showing nearly all the phenomena that we notice at present.

Mr. Christie. With regard to Mr. Carpenter's drawings, we have been lately considering whether we could not photograph them. There is an exceedingly valuable series of drawings of Mars made in 1861, and a series of drawings of Jupiter. But the difficulty is that the drawings are coloured, and in the case of Mars the yellow colour of the disk interferes with the photography. Capt. Abney has promised his assistance, and we hope that we may be able to produce some photographs in the course of time. At present we have taken a few photographs of the 28 drawings of Mars made in 1877, so that they might be available for distribution to a limited extent. A complete set has been mounted on this card which lies on the table.

Mr. Penrose. Has any one made spectroscopic observations of this spot on Jupiter?

The President. I have not heard of or seen any spectroscopic observations hitherto of this red spot in Jupiter. It would be an interesting observation if it could be obtained.

Mr. Christie. It would be a very difficult one. We made many attempts on Mars on the last opposition. A planet is really a very small object on the slit of a spectroscope, and it is difficult to identify or distinguish the spectrum of the different parts of the disk. Still it would be an interesting inquiry, and well worth trying, I think.

Mr. Ranyard. We have two or three copies made by Mr. Carpenter of some of his most interesting series of Jupiter drawings, which are in the library.

Captain Noble. In answer to the remarks which have been made, as to the mere vulgar abuse with which I was first greeted, I shall not condescend to notice it. Mr. Brett is no doubt the only person, according to his own account, who can draw at all. I will not condescend to notice mere pot-house abuse; but with reference to a remark which has been made about this white spot, my intention was not to call attention to the fact that white spots had been noticed on Jupiter, for I had seen Mr. Lassell's drawing and Mr. Dawes's drawings, but to the fact that there were shadows on both sides of this spot. I am exceeding anxious to hear an explanation of it. I believed originally that they were shadows; but when I see a dark spot on the side where the Sun is shining my limited knowledge of optics prevents my seeing how a shadow can be cast in this manner.

Mr. Neison read a paper on the diameter of the Moon, pointing out that Prof. Pritchard had assumed an erroneous value in the comparison with his photographs, and that the measures did not really agree very closely with the correct value. He had obtained an empirical formula representing the varying value of the diameter at different observatories and with instruments of different apertures. He also adverted to the decrease in the errors of Hansen's Lunar Tables since 1877 to which Mr. Lynn had called attention, remarking that unless there were an equal number of observations before and after full moon and in opposite parts of the year, we could not be certain that the decrease was real.

Mr. Christie. With regard to Mr. Neison's empirical correction for the Moon's semi-diameter, I should like to ask him how he has applied the correction for personality in the observers, because that seems to me to be a real difficulty in determining the diameter. Mr. Lynn has been putting together a number of determinations of the diameter with the altazimuth, and the discordances between different observers are very serious, 3″ or 4″ of arc, and quite systematic. In the face of such large discordances, I think it is rather difficult to determine what is the real truth as regards the diameter.

With regard to this very important point of the diminution of the Moon's error, I think, so far as I have examined the observations, that it is fully borne out. Of course, as Mr. Neison has pointed out, if the observations were not equally distributed throughout the year and throughout the lunation, we might have some discordance which would be merely apparent and not real; but, as a matter of fact, the observations seem to be very fairly distributed; and you have only to glance through the Moon's errors as shown in the Greenwich observations, and you will see that there is a very great diminution of error lately.

Mr. Dunkin. It is a very remarkable thing that soon after Mr. Lynn published his paper in March, the tabular error of the Moon began to increase, and in May and June the maximum errors in R.A. were almost as large as they were three years ago. In the year 1862, when Hansen's tables first came into use, the tabular error in right ascension was about $-0^{s}\cdot 5$, which gradually increased until last year, when it was found that the error began to diminish; but now, as I have just said, within the last few months it is beginning to increase again; so that it is a very difficult matter to say at present whether the errors of Hansen's tables are increasing or diminishing.

Mr. Stone. As to the difference between the photographic and the 'Nautical Almanack' semi-diameters, I think we may accept it as pretty well established that the semi-diameter depends upon the telescope. The 'Nautical Almanack' semi-diameter, as far as I know, has been got to represent pretty fairly the Greenwich observations. When Mr. Neison speaks of formulæ to represent the semi-diameter observations made at all the observatories, undoubtedly the object-glass used must be one element in the thing, and, I should say, almost the principal one. With regard to personality, that is a very serious thing; but that has been observed for a good many years. When I was attempting to determine the parallax of the Moon by observations made at Greenwich and at the Cape of Good Hope, I found that I was getting into a regular muddle with some of the observations made by Sir Thomas Maclear, in which there was a personality of a second and a half in observing the semi-diameter. When I took the north limb and the south limb, and discussed them separately, the results came out very well.

Mr. Dunkin. About ten years ago the Fellows may recollect that I published a short paper in the 'Monthly Notices,' in which was shown that a great personality existed in observing the first and second limbs of the Moon by the different observers at Greenwich, and this to such an extent that I was generally able to point out in one instance, without reference to the names of the observers, by whom certain observations were made, as this observer's method of observing the two limbs of the moon differed so much from the other observers. With regard to the semi-diameter, it is very difficult to obtain the true value, as it depends on the telescope employed as well as on personality in the observer; for instance, at Greenwich two values are used, one of which is applied to observations with the transit-circle, and the other to the altazimuth observations; and the proper thing to do is to deduce a separate semi-diameter for every telescope that you use. You see how difficult a matter it is to obtain a true value of the semi-diameter.

Mr. Neison. The formula referred to does take into consideration the aperture. It shows that it would make over half a second of arc difference for different apertures. As to what Mr. Dunkin has told us, 1878 was a most exceptional year, so far as the weather goes; and most likely a sufficient number of observations were not obtained in the autumn to counterbalance the spring observations.

Mr. Christie. I wish to mention that Mr. Maunder succeeded on Nov. 12 at Greenwich, with our 12¾-inch refractor, in getting some observations of Deimos, the outer satellite of Mars. That shows that the satellite is not such a very difficult object as might be supposed. The observations were made with the position-micrometer with illuminated wires, without any great difficulty, near the time of elongation.

The President. I saw the satellite, but was not able to measure it, three or four nights ago. The first night we looked for it for some time, but it was so thoroughly cloudy that we could not get good observations. The only published observations that I have seen anywhere are those of Mr. Common.

Mr. Common. Deimos was observed from Sept. 21 to last night, but Phobos only from Nov. 2 to last night, four or five nights, and perhaps twenty or thirty measures. Any person who can see Enceladus at elongation should with care see both the satellites of Mars if the precaution be taken to hide the planet. I have been surprised at the brightness of Deimos at elongation. It is quite as bright as Enceladus; but it is utterly useless to attempt observing or measuring unless you have the planet hidden, except perhaps in the case of Phobos. One night I saw Phobos although it was not a diameter away—I saw it distinctly in the glare of the planet.

Mr. Christie. Mr. Maunder's measures were made by viewing Mars through a dark glass placed in front of the field-lens and covering part of the field, which diminished the glare of the planet. By that means it was perfectly practicable to see Mars at the same time as the satellite, so as to measure its position more accurately.

The President. It may be within the recollection of some of the Fellows that at the May meeting one gentleman asked about some means of disseminating information amongst astronomers in England as to comets and new stars. I promised at that time that I would consider the matter, and I have thought of a way of doing it. I have prepared a circular, which has been addressed to about three hundred observatories, asking that the discovery of a comet, new star, or other object of immediate interest may be telegraphed to me at Dun-Echt Observatory, Aberdeen, on the understanding that I will distribute it by circular within twenty-four hours to all who would be likely to make useful observations.

Mr. Chambers. May I ask, as a matter of interest to a large number of Fellows, what probability there is of an early dissemination of the forty-first volume of the 'Memoirs,' the Eclipse Volume? The time is running on, and I think the Fellows would be anxious to know something about it.

The President. I think probably you were not in the room when I said just now that the last words in the preface and titlepage went to press about three days ago, and I hope the volume will be distributed as soon as possible.

The following were duly elected Fellows:—

Rev. J. L. Challis, M.A.; W. C. Armstrong; Prof. J. J. Åstrand; T. W. Bithrey; G. H. Darwin, M.A.; H. T. Vivian; R. R. Webb, M.A.

The following papers were announced:—

A. Marth. Ephemeris for Physical Observations of Mars, 1879.

Major J. Herschel. Note on the difference of variation of gravity at Revel and St. Petersburg, and on Grischow's pendulum-observations at other stations.

Prof. W. Harkness. On the Physical Configuration of Mars.

W. T. Lynn. On the change in the mean error of Longitude of Hansen's Lunar Tables since 1876.

Astronomer Royal. Mean areas of umbræ, whole spots, and faculæ upon the Sun's disk, as measured on photographs taken at the Royal Observatory, Greenwich, for each rotation of the Sun from 1873, July 11.

Prof. Tacchini. Observations of Comets *Palisa* and *Hartwig* made with the Merz equatoreal of the Collegio Romano.

R. L. J. Ellery. Conjunction of Mars and Saturn, observed at the Observatory, Melbourne.

R. Wilding. Comparison of Hansen's coefficients of the Moon's latitude with those of Plana, Damoiseau, &c.

R. F. Ward. A new system of Micrometry.

Maxwell Hall. The Zodiacal Light.

A. Bowden. Description of a self-registering Micrometer.

Lord Lindsay. Observations of Comet 1879 *d* (Palisa) made at Dun-Echt Observatory.

Lord Lindsay. Observations of the spectrum of Comet 1879 *d* (Palisa).

G. Calver. On the working of the speculum for the 37-inch Reflector.

Prof. A. Winnecke. Observations of the Solar Eclipse, 1879, July 18, made at the University Observatory, Strassburg.

The Meeting adjourned at ten o'clock.

Note on Hyperion.

The following considerations have been suggested to me by a letter from Prof. Asaph Hall, relative to Hyperion, published in the last No. of the 'Monthly Notices,' Vol. xxxix. No. 9.

The orbits of Hyperion and Titan are nearly in the same plane; and, representing by a, e, a', e' the semi-major axes and eccentricities, we have

$$a = 214'', e = 0\cdot115 \text{ (Bond)}; \; a' = 176''\cdot5, e' = 0\cdot029 \text{ (Bessel)}.$$

If we neglect e', which we may do in a preliminary general examination, we see that the least distance between the two satellites will be

$$q = a(1-e) - a' = 13''.$$

Let M be the mass of Saturn, m that of Titan; then, when the two satellites are closest, the ratio of the perturbing force acting on Hyperion to the mean attraction of Saturn will be

$$\frac{m}{q^2} : \frac{M}{a^2} = \frac{m}{M} \cdot \frac{a^2}{q^2} = 271 \frac{m}{M}.$$

In a work published in the 'Mémoires de l'Académie de Toulouse' (7^{me} série, tome ix.), of which a part has been since reproduced in the 'Comptes Rendus,' I have shown that

$$\frac{m}{M} < \frac{1}{11000},$$

a conclusion which I have arrived at by a study of the movements of Iapetus. If we supposed $\frac{m}{M} = \frac{1}{11000}$, we should arrive at this result—that in the movement of Hyperion the perturbing force may amount to about one-fortieth part of the force producing elliptic motion. It will be conceived that very considerable perturbations must result from this for Hyperion.

Another circumstance must contribute to augment these perturbations. Let T and T′ be the sidereal periods of Hyperion and Titan; then we have, according to Bessel, $T' = 15^d\cdot945$, and according to Bond, $T = 21^d\cdot18$; from which we find

$$4T' - 3T = +0^d\cdot24.$$

Lassell gives $T = 21^d\cdot284$, which leads to

$$4T' - 3T = -0^d\cdot072.$$

We see that, in the two cases, the difference $4T' - 3T$ is a small fraction of T or T′, and there will thence result inequalities of long period of the form

$$A \cos (3l' - 4l + \alpha).$$

These inequalities will be of the first order relatively to the eccentricities; they will therefore be considerable. We may thus account for the enormous variations which Prof. Asaph Hall has pointed out in the position of the major axis of Hyperion, and may from them deduce an exact value of the ratio $\frac{m}{M}$; but the mean motion of Hyperion is still too imperfectly known to allow of the problem being solved at the present time.

I will remark, in conclusion, that whilst the first five satellites of Saturn appear to move almost exactly in the plane of the ring, Iapetus diverges considerably from it. This tendency begins to show itself in the case of Hyperion, whose orbit makes an angle of about 1° with the plane of the ring. As we know, Laplace has made known the reason of this divergence in the case of Iapetus.

Paris, 1879, Nov. 17. F. TISSERAND.

The great Patch on Jupiter.

WRITERS on astronomy are usually rather wild in their descriptions of phenomena, and their illustrations are often ridiculous.

There is at present a very extraordinary phenomenon in the solar system, more extraordinary, to my judgment, than any thing I have ever had the good fortune to see during about 30 years of telescopic practice; and it is thus described by writers in two scientific periodicals supposed to be serious:—

One of them says it is "a beautiful feature like a flame." This occurs in the second line of his description. He cannot get even through two lines without forsaking the facts and telling you the thing is like something else, which, I need hardly say, it is not. You, however, not having seen it go away with a definite and totally misleading image in your mind, and expect at all events to see a bright and coruscating appearance. Most observers have seen it by this time; but for the benefit of those who have not I may as well say at once that the most astonishing characteristic of this "beautiful feature like a flame" is its dense darkness.

What does the other writer say? He sets out by calling it in his first line a "beautiful cloud." If he had said a "beautiful coal-scuttle" it would have conveyed about as exact an idea.

The word cloud conveys, to most minds, the image of something vaporous, transient, and indefinite. Surely the study of astronomy must have a fatal tendency to make men vague! Allow me to say that if you cut a bit of maroon velvet into an elliptical shape, about an inch long, and stick it on to an orange you will see something quite as like a cloud as the phenomenon in question, and a good deal more like the thing itself: but I should not set out to say it was *like* even that. I would tell you what peculiarities it actually exhibited, and you ought thereby to be able to produce a pretty correct drawing of it for yourself. The first notice of it in my observatory book reads thus—"Deep red patch better defined than the belts and of uniform intensity" (then occurs the word "penumbra" crossed out), and "bright border suspected all round patch equal in width to its minor axis." Observatory notes are not meant for casual readers; therefore at the present time, having carefully observed its passage across the disk on two favourable nights, I will describe it more fully. The last observation referred to was on the 13th of November; and I may as well quote the first line or two from the observatory book (which does not mention either flame or cloud):—"Spot same intensity as darkest part of N. equatoreal belt, but being better defined and of uniform density and of a full blood-red it looks incomparably more conspicuous than it;" and after some more particulars the entry concludes thus, "The sharpness of the edge of the spot is nearly equal to that of the limb." Now I will attempt to set in order the notes made, assuming that the reader has not seen the object, and affirming that it is totally unlike any thing he might expect to see, provided he is well acquainted with the usual habits and behaviour of Jupiter's disk and with most of the historical writings on the subject. It is waste of time, in most cases, to speak of planetary markings at all, since they are objects of supreme delicacy and subtlety and demand a correspondingly cultivated eye and great telescopic experience, not to mention that only instruments of the finest quality are of any use for their investigation. It is hardly necessary now to say that this is an elliptical patch lying on the surface of the south temperate zone of the planet, which is a bright zone occupying at present about 20° of latitude. I say deliberately lying *on* the surface, since it presents none of those appearances characteristic of the belts, which in bygone days I have endeavoured to prove are not on the surface, but intermingled in the viscous semi-transparent substance of the planet; neither does it show any shade or shadow.

The uniform density throughout the extent of the patch and the uniform definiteness of its boundary are its salient characteristics, and strike one at first sight with amazement.

The northerly equatoreal belt is at present tinged with deep red. I do not call it a red belt; but there is a tinge of deep red mingled with its usual dusky colour and gravitating towards it northern border. The colour of the patch is of the same deep dusky red as that. Blood-colour of course is not constant; and to call it blood-colour without qualification would be exaggeration; but a drop of blood thickened by standing matches it pretty well. I speak of it as seen in a silvered glass reflector. In a refractor I cannot say what colour it might not show; but I should be surprised to see it of a rose-colour, as it is often said to be. Surely it is not more like a rose than a geranium or a magpie! At first sight one is vividly impressed with the bright nimbus surrounding it; but after careful examination I concluded that its objective existence is open to question, since contrast so violent would give rise to such an impression. It is to be observed that the patch is a very long and narrow ellipse lying midway betwixt two dark zones, the bright interval on either side being equal to its minor axis, so that some time (say half an hour or so) after its internal contact with the limb it is enclosed on the north, south, and east by a border of brightness equally wide; and the critical question is, whether or not the observer's imagination does not complete the inclosure on the western end. This is a more delicate matter for observation than the unpractised reader would suppose. I have not been able to detect any boundary to the nimbus at the end towards the centre of the disk; and when the centre of the patch is on the meridian the question cannot fairly be answered at all, since the normal deterioration of brightness towards the limb is of itself sufficient to suggest the closing up of the ends of the nimbus. The question is rendered still more difficult by the fact that the nimbus intrudes slightly upon the straightness of the dusky belts between which it lies: at least such is my opinion. I admit that the nimbus is remarkably clear and uniform in brightness; but I am not satisfied that its brightness surpasses the normal vividness of the bright zone. My assistant, whose sight is keener though less practised than my own, insists on its objectivity.

There is one other fact worthy of notice, viz. that the intensity of the patch does, like the belts, suffer some deterioration in the course of its approach to the limb, but not, I think, to so great a degree as in their case, and there is no doubt that it can be seen well up to the limb.

To the best of my judgment, the major axis of the patch covers 32° of Jovian longitude, or about 24768 miles, and its ends are so well defined and its shape so constant that it offers the greatest temptation for the use of the micrometer to determine the rotation by the transits of the ends, checked, of course, by frequent measurements of the major axis. The double-image micrometer is an instrument admirably adapted to get the bisection of the disk by an end of the patch, and I have no doubt that this has been frequently and satisfactorily done before now. It is not my business in any case; but I know how much the accuracy of micrometrical measurement suffers under the high powers required by such an object, and an estimated bisection by a cultivated eye under a high power seems to me more reliable than a micrometrical one under a low power.

I have not had time to look up Cassini's account of his famous spot of 1665; but that appeared on "the upper edge of the southern belt" and lasted for two years. It will be very interesting if any other correspondences should be shown. JOHN BRETT.

Keswick Road, Putney.

White Spots on Jupiter.

OBSERVERS have been so much occupied in watching the behaviour of the wonderful red spot on Jupiter, which has been interesting us so much this autumn, that the ordinary white oval clouds on the equator seem to have been overlooked.

To me, however, they have proved of great interest; and not having seen the least notice of the phenomenon I am about to mention, I thought it might be worth while to write a few lines upon the subject.

On Aug. 29 I drew the planet; the red spot was just going round at 11.45, and immediately following it was a distinct white oval on the equator, the *f* end of the former being just S. of the *p* end of the latter.

On Sept. 5 this white spot had advanced so that the centre of it was just under the *p* end of the red; whilst on the 7th it altogether preceded it by a slight interval.

Again on Oct. 9 I drew the planet, and found a white oval coming round again; but whether the same one as that seen in September, I am unable to say, not having drawn the planet much, and, to increase the difficulty, by the time it was passing the red one (which took place about Oct. 20) another one appeared, or, rather, a group of three. The first was distinct; the second a small one and dull; the third tolerably plain and of the same size as the first.

I now began to make more careful observations, feeling sure that the white spots really had a motion independent of the red one.

Oct. 24, 7.15. The first white spot, A, slightly following the *f* end of the red.

Nov. 2, 8.50. A exactly preceded the red, its *f* end equal to the *p* end of the latter.

Nov. 5, 5.15. A and B (the small one) had passed, and C was immediately N. of the red.

Nov. 7, 6.20. C half its length, preceding the *p* end of the red.

Nov. 9, 8.30. C immediately preceding.

Nov. 11, 9.30 (?). C has gained half its length, and A was at the preceding side of Jupiter's disk when the red spot was at the other.

From these notes it would certainly appear that the white spots on the equator had an independent rotation relatively to the red, in about 40 days, whether the interval be taken from the observations here given, or from the fact that on July 28 a distinct white spot was in the same position with regard to the red as was observed on Sept. 7.

Having only a 4½-inch reflector, and no means of measuring except by eye estimates, I will not go further into this question, hoping that some other observers will be able to show it better from a more perfect series of sketches, or from measurements; for there are doubtless an immense number of drawings of the planet available, from the impetus which this red spot must have given to observers of planetary phenomena. H. CORDER.

Writtle, 1879, Nov. 14.

*The Brightness and Distribution of the Fixed Stars**.—Part II.

THERE are, it will thus be seen, three ways of stating the rank of the stars: by magnitudes or other devices to express differences of visual sensibility, by quantities of light, and by positions on a list arranged in order of decreasing lustre. These three are reduced to one, through Fechner's law connecting the first two, and the hypothesis of equable distribution connecting the second and third.

But before accepting this hypothesis of equable distribution as part of our knowledge, we must see how well it agrees with the facts. Observation must determine if the "ratio of light" and the "ratio of number" have actually the mathematical relation given above. On the scale adopted by Mr. Peirce, as we have seen, the distance of a star should be two thirds that of one one magnitude fainter, and its light, by the law of the inverse square, 2¼ times as great. But the actual ratio of light between successive magnitudes is found by photometric measurement to be not far from 2½—different observers varying from 2·3 to 2·8, but all giving values greater than the theory. By the fact, however, that the ratio thus found is constant or very nearly so for all grades of brightness, we are yet justified, notwithstanding the objection from its too high value, in determining magnitudes by counting, and so clearing individual estimates of much of their uncertainty and irregularity.

The conclusion seems unavoidable that a uniform distribution of stars does not hold even in the region of space immediately about our solar system. Since the ratio of number is smaller than it should be to correspond to that of light, the density in which the stars are aggregated—if the expression be permitted—must diminish as the distance increases. Our sun is therefore in a part of space more closely filled than are neighbouring parts. This is perhaps the most interesting result to which the study of photometry leads us, because it seems so strange at first sight—and even more strange when we remember that the nearest of the other suns is distant from us more than three years' journey of light. Truly, astronomy is without a rival in its special mission, to contradict on every point the evidence of superficial observation. We would most naturally suppose our universe to be as we are told it appeared to a distinguished visitor, when at once

> "The golden sun, in splendour likest heaven,
> Allured his eye; . . .
> . . . . where the great luminary,
> Aloof the vulgar constellations thick,
> That from his lordly eye keep distance due,
> Dispenses light from far."

But this appearance of standing aloof is wholly misleading, and moreover, as our sun would rank by no means first among the fixed stars if placed at the distance of the nearest of them, and would sink below the third magnitude if removed as far as Sirius, its real insignificance in the stellar firmament is almost as striking as its supremacy in its own planetary system. The reflection is an interesting one, how lamentably the grandest of poems must have suffered had its author been compelled to regard the true proportions of the sidereal universe; but for the lover of nature, it may be hoped, the glory of the Almighty handiwork will not be lessened through the disappearance from fancy of the universal sovereignty of the sun along the track made for it centuries ago by the vanished delusion that our earth was the unmoved centre of all things.

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* From the 'Popular Science Monthly' (New York), 1879, August.

It is very certain that an equable distribution could not hold throughout all space (for an infinite number of stars impartially scattered would, however vast the distances among them, give us a heaven shining like the sun in every part, with heat to correspond) unless, owing to the presence of innumerable dark bodies, or to a discontinuity in the luminiferous ether itself, as some physicists have suggested, light from remote distances is wholly or partly cut off before reaching us. But to this view, though it would agree with all the facts, that of a limitation of our firmament of stars, in extent and number, is generally preferred. That such a limitation exists we have other reasons for believing: prominent among these is the system of distribution which a census of the heavens brings to light. We could not expect an infinite universe of stars to show everywhere such a uniformity of plan.

How the fixed stars are actually distributed through space, an inquiry into which we are led by study of their number and brightness, it has been but recently found worth while to consider. So long as they were believed to be simply lights set in a hollow revolving structure that divided the waters beneath from the waters above; so long as the idea of the "firmament" retained the association with solidity that now only remains in the word; so long as the "spangled heavens, a shining frame" was a reality of opinion and not an unmeaning archaism of poetry—this question was never heard. No significance could attach to it, and it excited no curiosity. But when this solid celestial framework was broken up by the discovery of the earth's rotation, and its lights scattered afar on the deep ocean of unbounded space, when contemplation of the beautiful adjustments and proportions of our solar system had suggested the hope of discovering the same harmonies throughout the universe, it began to be asked if some of these far-distant orbs, or perhaps the mighty whole, our sun and its attendant planets included, were not connected in a system of similar character. Kant was one of the first to advance this idea. The elder Herschel, contenting himself with a working hypothesis to give form to his observations, supposed our firmament to be a mighty cluster of stars equally distributed within finite limits, so that the number visible in the field of his great reflector at any pointing would show the extent of occupied space in that direction; and he undertook to gauge the depths and discover the shape of this cluster by counting telescopic fields in different parts of the sky. The elder Struve considered the density of the stars as varying with the distance from the Milky Way, as does that of the atmosphere with its distance from the earth's surface; being equal in parallel planes*. Argelander, of Bonn, relieved his laborious task of cataloguing over 300,000 northern stars, by investigation into the subject; Mr. Proctor has devoted to it numerous memoirs and popular lectures, and speaks of it as his chief incentive to the labour of constructing his set of twelve star maps; Mr. Peirce gives it considerable space in his 'Photometric Researches.' From these sources we have a few conjectures and a few facts.

The richest parts of the sky, in bright and faint stars alike, are almost all about the Milky Way. This stream of suffused light follows, with some irregularities, the course of a great circle; and toward the plane of this circle, passing not very far, perhaps, from the sun, stars at all distances appear to become more densely packed. The Milky Way itself is evidence of this for the faintest magnitudes; and Herschel's star-gauges, from which he inferred for our cluster the shape of a disk or lens, give the comparison in a numerical form. Argelander's gauges show the same concentration in telescopic stars brighter than the tenth magnitude, and it is even more plainly to be made out from Mr. Proctor's chart of his great catalogue. If parallel to the great circle of the Milky Way two small circles be passed, each at a distance from it of 30°, having between them a broad belt about the celestial sphere somewhat like the torrid zone on the earth's surface, we shall leave two spherical caps whose united area will exactly equal that of the belt—just one hemisphere. From Argelander's gauges it may be calculated that the number of stars inside these 30° circles is to that outside nearly as 2½ to 1, for stars of the ninth magnitude, and about as 2 to 1 for the eighth, diminishing with brighter stars. This condensation increases without interruption to the Milky Way itself. The law holds also with stars visible to the naked eye, though not so conspicuously; for these, Mr. Peirce found the same ratio to be only as 4 to 3. He was also surprised to see that the stars were very little more numerous in the track of the Milky Way than at a distance of 20° from it, the decrease in density appearing almost suddenly about 30°. But as we approach the sun, the rate of condensation becomes greater again. Of the twenty stars classed as first magnitude by the best observers, fifteen are within the 30° circles; and of the five outside, but two, Arcturus and one far southern star, are equal in brightness to the average of the twenty. We have no right, however, unless we are dealing generally with a very great number of stars, to take light as a reliable indication of distance. Of our twelve nearest neighbours yet recognized, being all that have a parallax greater than one sixth of a second, and distant from us less than twenty years' journey of light, four are telescopic stars, to which attention was attracted by their large proper motion. Ten stars out of these twelve, it should therefore be added, are either in the Milky Way or within 15° of it. The exceptions are two minute stars in Ursa Major.

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* Professor Newcomb's account of these researches and speculations, in his 'Popular Astronomy,' pages 462–476, is full and interesting.

Will these facts enable us to decide what is the actual form of the immense cluster of stars in which our sun holds so humble a rank? We may conclude from them, with safety, that the strongly marked and surprising concentration of brightest and nearest stars in the galactic plane is irreconcilable with a generally prevailing uniform distribution, and agrees hardly better with Struve's theory of condensation in parallel planes. For this theory, it will be seen, requires a more decided concentration with a greater distance, the planes of equal density appearing to approach the galactic circle and each other as do the parallel lines of a perspective drawing. We do see some tendency of this kind in telescopic magnitude, so that we might suppose that Struve's theory began to express the facts at the distance of the faintest visible stars—unless it could be shown that the density of aggregation in the central plane also varies at different distances. In Mr. Peirce's opinion, photometric observations have proved that this density increases from the seventh to the ninth magnitude, and that therefore the idea of the Milky Way itself as a vast ring of closest aggregation, including a more sparsely filled region, thus giving to the whole cluster, could it be seen laterally from a sufficient distance, an appearance not unlike that of the annular nebula in Lyra, is well founded. He also finds reason for supposing a similar but independent arrangement of the brightest stars, in the peculiar localization of the nearest of them, and the sudden falling off in density at about 30° from the galactic circle, above remarked—other systems of condensation requiring gradual changes. The true figure, if his reasoning is to be trusted, would therefore be a small ring of maximum density near the centre of a very large one*. Such speculations are, however, it is hardly necessary to say, very uncertain.

It is not easy to make out, from the general distribution of the stars, that our sun is in one direction rather than another from the centre of the sidereal system; and there is even some doubt about the position which some astronomers give us, on the northern side of the plane of the Milky Way. Indeed, beyond the prevailing condensation toward this plane, it seems that no important general law governing star aggregation has yet been found. Mr. Proctor's services in calling attention to the grouping of certain portions of the heavens in subordinate systems having a common "star-drift," should not be overlooked; but his discovery that a large part of the southern hemisphere is particularly rich in stars * cannot be admitted for several reasons:—1. Behrmann's catalogue of southern stars, in which magnitudes were observed with particular care, shows nothing of the sort; 2. Mr. Proctor's own maps show nothing of the sort, for stars brighter than the sixth magnitude: and it is far less credible that an anomalous law of distribution holds over a wide area, affecting but this one order of brightness, than that those who observed this part of the heavens included more and fainter stars in their sixth magnitude than did northern observers; 3. Mr. Proctor's own maps show that the boundary of his "rich region" is the Tropic of Capricorn; and it is far less credible that an artificial circle should limit any law of distribution than that the whole difference is due to the fact that this tropic was also the northern boundary of La Caille's observations, the source, in all probability, whence the magnitudes of Mr. Proctor's stars were originally derived. Observers, in fact, are particularly likely to differ in estimating the extent of the sixth magnitude; for it seems to have been agreed by general consent that this magnitude shall include all stars to be seen with the unaided eye on the clearest nights, and differences of climate and of individual eye-sight affect this considerably. Argelander, and after him Heis, catalogued all the stars visible to their eyes; their numbers, for the whole northern hemisphere, were 2350 and 3936 respectively. Heis must therefore have seen stars at least four tenths of a magnitude fainter than Argelander's faintest. La Caille's eye must also have been sharper than the average; and, if Mr. Proctor had thought to apply the test of enumeration to the different magnitudes in different parts of the sky, this explanation would doubtless have occurred to him, and nothing have been heard of his remarkable "rich region." His observation is valuable, certainly; but only by showing the undeveloped state of the whole subject, and the precautions necessary before venturing conclusions on it.

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* 'Photometric Researches,' pages 175–178. The sun, it would seem, is to be considered as in a region of exceptional rarity as compared with other regions through which the galactic plane passes, and at the same time of exceptional density when the comparison includes stars remote from this plane.

The search for a common centre, about which the uncounted millions of stars composing the galactic cluster may revolve, has tempted many investigators, but it cannot be said as yet to have proved altogether successful. Mädler, by calculations from the proper motions of stars in different parts of the heavens, sought to locate it among the Pleiades; some later astronomers have preferred the Sword of Perseus; Mr. Maxwell Hall has just decided, and informed the Astronomical Society of England, that the universe turns about the South Elbow of Andromeda. The proof advanced is always incomplete, resting on assumptions not generally admitted; and when we remember that the gravitative force exerted by the fixed stars on one another is so small that to keep the nearest of them from falling to the sun, supposing no counterbalancing attractions, an angular velocity of but one second of arc in eighty years is needed; that the proper motions to be

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* Most positively stated in a lecture before the Royal Institution, May, 1870, also in the introduction to his 'Star Atlas.'

explained are often far larger than this; that the distance of the attracting centre must be many times that of the nearest fixed star; and that the heavens give no sign of any pre-eminent body or group of bodies to which we may ascribe the enormous attractive power necessary to control these motions—the scepticism of many astronomers as to the universal centre seems excusable.

It must be admitted, then, that but little of the true character of our sidereal system is known to us, and that all speculation upon it rests as yet on a very insecure foundation. But, as the sudden development of spectrum analysis has shown, matters of pure conjecture to-day may become entirely settled to-morrow; and it may reasonably be hoped that the secrets of this domain, if due interest be taken in them, will not much longer elude the search of scientific explorers.

HENRY FARQUHAR.

Double Stars for December.

In addition to the pairs already given (see 'Observatory,' Vol. ii. pp. 239, 269, and Vol. iii. p. 200) the few which follow claim occasional measurement.

Σ 249. R.A. $2^h\ 12^m{\cdot}7$, Dec. +44° 3′. Mag. 7, 9.
1873·9 190°± $2''{\cdot}0\pm$ Gl.
Probably binary.

Σ 254. R.A. $2^h\ 14^m{\cdot}8$, Dec. +23° 5′. Mag. 8·5, 10.
1874·0 343°·8 $12''{\cdot}9$ W.

Σ 278. R.A. $2^h\ 28^m{\cdot}3$, Dec. +68° 47′. Mag. 8·4, 8·7.
1872·3 73°·8 $0''{\cdot}64$ OΣ.
Probably binary.

Σ 288. R.A. $2^h\ 32^m{\cdot}3$, Dec. −11° 54′. Mag. 8, 11.
1867·2 213°·7 $11''{\cdot}67$ De.
Probably binary.

OΣ 43. R.A. $2^h\ 33^m{\cdot}6$, Dec. +26° 7′. Mag. 7·2, 8·8.
1871·7 66°·2 $0''{\cdot}70$ De.

β 521. R.A. $2^h\ 34^m{\cdot}9$, Dec. +47° 45′. Mag. 6·5, 11.
1878 153°·7 $5''{\cdot}86$ β.

Σ 314. R.A. $2^h\ 44^m{\cdot}3$, Dec. +52° 30′. Mag. 6·9, 7·1.
1876·2 300°·7 $1''{\cdot}6$ Gl.

β 524. R.A. 2^h 46^m·1, Dec. +37° 51′. Mag. 6·7.
1878·7 158°·7 0″·34 β.
A C is Σ 318.

Σ 326. R.A. 2^h 48^m·5, Dec. +26° 24′. Mag. 7·5, 9·7.
1868·7 216°·8 8″·59 OΣ.
A physical pair.

JOSEPH GLEDHILL.

Mr. Edward Crossley's Observatory,
Bermerside, Halifax.

Meteor Notes for December.*

THE return of the shower of quick short meteors from a radiant near α Geminorum should be observed on the nights of the 9th–12th. The point of departure is rather diffuse, as in the case of the Perseids and Orionids, but is apparently concentrated between the stars α and θ of Gemini. A shower of long streak-leaving meteors, seen by Corder from 108° +28° on Dec. 9, 1877 (and regarded by him as distinct from the true Geminids), should also be expected to recur, and on about the 6th an active radiant of slow brilliant meteors from near ζ Tauri may be seen. On Dec. 8, 1877, a very marked display of swift meteors was traced from an exact radiant at 145° +7° (south of α Leonis). Early in the month a shower from η Ursæ Majoris has been observed, and towards the end there is a conspicuous radiant near χ Ursæ Majoris (132° +47°). Heis gives three active streams this month from the points 74° +45° (2nd–13th), 66° +34° (3rd–11th), and 115° +55° (7th–23rd). Zezioli saw the latter shower both on the 9th and 21st, with an average radiant at 109° +62° from three observations. There are a large number of radiant-points at this epoch in Ursa Major, Leo, and the adjacent constellations. On the first few nights of January the Quadrantids should be looked for, though the moonlight will be too strong to give the observer a good prospect of success. This rich shower is seldom well seen, owing to the constant impediments of cloudy weather and moonlight; but during the last two years it has recurred with striking activity on the morning of Jan. 2, though it has been very slightly observed. Large meteors should be expected on the nights of the 8th–9th, 11th–12th, and 21st. The two former epochs correspond to the Geminids; but on the 21st no very marked radiant-point has hitherto been detected, though the number of fireballs seen on that night far exceeds (in the proportion of 3 to 1) the number recorded on other ordinary nights of the month. This predomi-

* See 'Observatory,' Vol. i. p. 259, and Vol. ii. p. 270, for further notes on December showers.

nance of brilliant meteors is possibly referable to the action of a special shower, which should be attentively looked for every year at this epoch.

Recent Observations.—Mr. H. Corder, at Writtle, Chelmsford, observed 54 Orionids, chiefly on the nights of Oct. 15 and 21, and gives two radiants at 88° +13° and 93° +19°. He says "the shower at μ–η Geminorum seen in 1878 seems not to be in action this year; but the one at ν Geminorum, scarcely noted last year, is very active this. Only three 1st mags. were seen; they were pale yellow with greenish streaks. There were probably adjacent showers near α Tauri, ζ Tauri, and in the N.E. part of Orion." Mr. Corder also noted two active radiants of slow meteors from the points 25° +8° (18) and 32° +11° (10), the former at the middle of the month, the latter at the end; and they are probably identical with a rich display observed by the writer this year at 31° +9° (37 meteors, Oct. 14–20), and by Major Tupman in 1869, Oct. 13, at 28° +10°. Amongst the other showers seen by Mr. Corder the most prominent were at 95° +34° (10 meteors, very swift, streaks brightening), 132° +22° (6), 286° +42° (6), 306° +64° (10). There were doubtful positions at 60° +38° (7), 22° +29° (6), and 40° +7° (4), and at the middle of the month 17 meteors were noted from an ill-defined scattered radiant in Cassiopeia (5° +62°). A stationary meteor and several others were registered from Lacerta (336° +43°).

292 shooting-stars were seen by the writer at Bristol in October, chiefly on the nights of the 14th, 15th, and 20th. 50 were recorded after midnight on the 14th, including a brilliant Orionid = ♀, at $16^h\ 45^m$, with a path of 12° in the fore part of Ursa Major. On the 15th a watch was sustained for 11 hours ($6^h\ 30^m$ to $17^h\ 30^m$), but meteors were by no means numerous; the aggregate number counted during the night was 127, of which 16 conformed to the radiant-point of the Orionids. The most active shower proceeded, however, from the west part of Aries at about 31° +9° (21 meteors); but the point of divergence was not very exactly defined. These meteors were slow, somewhat faint, and with occasional trains. The Gemellids (106° +23°) supplied 11 meteors of great velocity, and with a length of path considerably exceeding the average. Nearly all had streaks. Two showers from Auriga were observed during the night from 78° +57° and 76° +32° (15 meteors), and a position south of α Cassiopeiæ (7° +51°) was denoted by 7 slowish paths. After the 15th cloudy nights intervened until the 20th, when, during the 7 hours preceding $13^h\ 45^m$, 88 meteors were seen, including 21 Orionids with a radiant at 93° +17°. Of the remaining meteors, 8 indicated a good shower at 45° +6° (α Ceti), slightly seen also by Mr. Corder (40° +7°), and well determined by Major Tupman in 1869 at 43° +11°, Oct. 13, and 44° +4°, Oct. 14. In the early part of October this year a series of dense fogs prevented lengthy observations at Bristol; but on the 4th–6th about 15 shooting-stars were seen, chiefly from a

shower near *a* Cephei. The same radiant was very clearly manifested on several evenings later in the month, and with more favourable weather a large number of its meteors would presumably have been traced. The centre was at 316° +59° from 17 paths. Heis had determined it at 315° +59° for Oct. 3–Nov. 13 (B_9, 50 meteors). The recent observations for the period Oct. 14–20 also show a good centre at 95° +46° from 11 meteors, and another near *o* Ursæ (114° +62°) 7 meteors. Several of the swift meteors from these streams were very curious, appearing as transient gleams of dull greenish light, and utterly different from the sparkling star-like lustre of the slow meteors proceeding from Aries and the constellations yet further removed from the earth's apex. A remarkable 1st mag. meteor, evidently belonging to the Orionids, though the radiant had not nearly risen, was seen at 8^h 32^m on Oct. 20. It ascended from the N.E. by E. horizon to the star *β* Tauri, and there disappeared, leaving a brilliant streak in its slowly traversed path. The shower of Orionids appears to have been fairly well exhibited this year on the 15th, 20th, and 21st; but it is unfortunate that in the interval, when the maximum probably occurred, no observations could be obtained, owing to the cloudy weather.

Mr. Corder watched for the Leonids on the nights of Nov. 11, 12, and 13. He saw about 12 meteors from this shower on the 12th, and on the 13th there were a few other doubtful ones. The radiant was at the point 149° +23° exactly. A few of the meteors were 1st mag., pale yellow, with yellowish or greenish streaks. Mr. Corder also noted considerable numbers of Taurids. Altogether he mapped 40 of them with radiant at 58° +21°. They were greenish yellow, with short reddish trains, and sometimes a very narrow red streak. There were other radiants near the Taurids with similar slow meteors, notably in Musca.

The writer at Bristol observed 52 meteors on the night of Nov. 12 before 14.45, and on the 13th 100 meteors were seen in a watch extending over 11½ hours, from 5^h 30^m to 17^h. The sky was a little foggy before midnight and only 38 meteors were seen. There was a very decided outburst of fine Leonids on the morning of the 14th, chiefly between $2\frac{1}{2}^h$ and $3\frac{1}{2}^h$ A.M. The shower consisted of 18 meteors (2=♃, 2=♂, and 2=1st mag.), all very swift and with vividly bright lingering streaks, which enabled the directions of their paths to be very accurately recorded. Radiant-point at 148° +23° precisely. Max. Nov. 13 at 15^h. Two Leonids were also seen on the 12th, one of which, at about 10^h 17^m, was a very extraordinary meteor, fortunately seen both at Bristol and at Writtle. As observed at the former station, it had a very long path of 98° from 98° +22° to 4° −15°, and left a brilliant streak in its wake, though the nucleus was only 3rd mag. The duration of flight was estimated at 4 seconds. Mr. Corder says, "The commencement of the streak was at about 12° +21°, and the nucleus, evidently much diminished, was travelling very slowly towards Jupiter, stopping close to him at 338° −12°, or about 40° long; but the track was evidently much foreshortened, being close to the anti-radiant-point. I think it was the queerest meteor I ever saw. It was only about 4th mag. when I observed it, with a great streak like a meteor = Sirius at least." At Bristol, as at Writtle, large numbers of Taurids were observed. On the 12th the radiant seemed very exact at 62° +21½°, but on the evening of the 14th the best centre was at 58° +21°. On the 13th a number of meteors closely resembling the Taurids were registered from a point near ε Arietis; and on the morning of the 14th some very swift, streak-giving meteors indicated a radiant-point at 125° +55°. From these observations it is certain that the Leonids reappeared this year both on the mornings of the 13th and 14th, exhibiting some very fine meteors from the usual radiant-point, and that the Taurids offered an abundant display of meteors at the same epoch. The Leonids were increasing so fast at about 15^{h} on Nov. 13 that a brilliant return of these meteors seemed imminent; but, soon afterwards, the shower died away, and there were only occasional shots from the sickle of Leo.

A brilliant meteor, estimated 4 × ♃, was seen at Greenwich at $11^{h}\ 31^{m}$ P.M., Nov. 11. "Path nearly horizontal from ν Eridani, between γ and δ Eridani to 42½° S. 23°, leaving a bright emerald-green streak for 5 seconds, 5′ to 7′ broad in the centre, and tapering at both ends. Path fixed by the streak. It was seen through the window of a room illuminated by gas." Mr. Corder appears to have noted the streak of this meteor at $11^{h}\ 30^{m}$, Nov. 11, from 50° −12° to 28° −23°. He says it was evidently a Leonid; its path conforms to the radiant-point exactly.

Mr. E. F. Sawyer, at Cambridgeport, Mass., writes that "his observations in September were few in number, owing to cloudy weather." Three showers were seen as follows:—Sept. 9–11 at 2° +15° (9 meteors, max. on 11th), Sept. 15–20 at 10° +35° (10 meteors, max. on 17th), Sept. 7–17 at 346° +45° (8 meteors). On Oct. 19 Mr. Sawyer watched for 5 hours (8^{h} to 13^{h}) and recorded 46 meteors, of which 8 were Orionids from a radiant at 93° +14°. Referring to the August Perseids of this year, he says the shower was certainly a very meagre one and inferior to any previous display as observed by him. Yet M. Chapelas ('Observatory,' No. 31, p. 213) found the horary number greater than in 1848, which he regards as the year of maximum abundance. The latter observation is strangely at variance with other results.

A very brilliant meteor, illuminating the whole sky, was seen at Debenham, Suffolk, on Sept. 22, at $13^{h}\ 20^{m}$. It began at an altitude of about 70° in the Milky Way, and descended swiftly to a point several degrees below Jupiter, where it burst.

W. F. DENNING.

CORRESPONDENCE.

To the Editor of 'The Observatory.'

The Eclipse of 1879, *July* 18.

SIR,—

I find myself obliged, much against my will, to reply to the unjustifiable and gratuitous attack made on me by Señor Pujazon in the letter signed by him which appears in No. 30 of 'The Observatory.'

I do not consider myself an infallible observer, and the readers of 'The Observatory' will readily understand how easy it is to make a mistake in counting the seconds on the chronometer or in fixing the exact instant of any physical phenomenon whatever.

Señor Pujazon undertakes to defend the Spanish 'Nautical Almanac,' which no one has attacked; the comparison made by me between my observation and the data given in the 'Nautical Almanac' had for object to show the great difference between them.

If I had wished to call in question the accuracy of the Spanish 'Nautical Almanac' I should have confined myself to a revision of its data, since none of its astronomical tables are the production of Señor Pujazon, nor is he the author of a single formula.

If Señor Pujazon were a practical astronomer, he would know that many perturbing influences affect every astronomical observation—the quality of the instrument, the state of the atmosphere, the altitude of the celestial body, personal equation, &c.—all these should be taken into consideration before rejecting with disdain the results obtained by the most humble lover of astronomy.

I am, Sir, your obedient servant,

Cadiz, 1879, October 18. AUGUSTO T. ARCIMIS.

The Red Spot on Jupiter.

SIR,—

With regard to C. W. Pritchett's query in your last Number as to whether the red spot on Jupiter was visible last autumn, I have only two observations bearing on the point. One was on Sept. 9, 1878, at 9^h 53^m, long. of central meridian 248°, when I made a short note on Jupiter; there is no mention of the red mark, but the definition was bad. And I have a sketch drawn on Aug. 19, at 9^h 30^m, long. of central meridian 311°, when the spot should have been visible if in the same position as it has now; but no trace of it is shown. But when the long. of the central meridian was 82°, on Aug. 27, at 9^h 39^m, I have the following note:—"There is a long dusky mark a little south of the southern equatoreal belt, its *f* end being two-fifths of the way from the *p* to the *f* limb." I do not know whether this can have any bearing on the red spot; but at present its longitude is increasing about 5° per month.

These observations were made with a 4¼-inch refractor.

Prof. Pritchett appears to be in error in stating that the latitude of this spot is 40°; a sketch I made of it places it in lat. 22°.

I am, Sir, yours faithfully,
T. W. BACKHOUSE.

Sunderland,
1879, Oct. 29.

Large Meteor.

SIR,—

You may be interested to know that at $6^h\ 16^m$ G.M.T., on Nov. 19, I observed from the railway-station here a large meteor. It first appeared at a height of about 20° above the northern horizon, at a point about 5° east of north, and descended to a point slightly further east, about 10° above the horizon. Its path was within a few degrees of being vertical. The duration of its flight was, as far as I could guess, two seconds. Very faint at first, it increased rapidly in brightness until, at the close of its path, it showed an apparent breadth of about 6′ or 7′. It was about as bright as Jupiter would be if he showed the same area as it did. This, however, must be an underestimate, as it shone through a veil of clouds which hid all the stars in the neighbourhood.

I am, Sir, yours faithfully,
EDWARD B. KIRK.

Saltcoats, Ayrshire, N.B.,
1879, November 19.

Transits of Intra-Mercurial Planets.

SIR,—

Professor Oudemans, of Utrecht, has kindly called my attention to the fact that in the list of supposed observations of the Transits of Intra-Mercurial Planets or other Bodies across the Sun's disk, which you did me the honour of publishing in your impression of September last, no mention is made of Haase's discussion of the same subject in the 'Zeitschrift für Astronomie,' or of the list of such observations given by him.

May I be allowed to explain that I had Haase's articles before me while composing the Lecture from which the excerpt in the 'Observatory' is taken; but that I did not mention them, because I thought they would not be easy of access to my hearers? I also found that they mentioned many observations which I did not think of sufficient importance for insertion, even in what might well be termed as complete a list as I could give in a popular lecture.

I ought, however, before the list appeared in the 'Observatory,' to have seen that a reference to Haase's valuable articles was appended; and I shall be much obliged if you can insert this note, so that in the index of your current volume it may appear concurrently with the list in the September number.

Possibly in Haase's list some additional observations may be found which are really of importance; but I believe that most (if not all) of those which I have omitted were cases of Sun-spots of brief duration.

It may interest some of your readers to hear that Prof. Oudemans has suggested to me that some of the appearances referred to may be instances of large meteoric waves moving possibly in hyperbolic orbits, the transits of which would not recur.

I am, Sir, your obedient servant,
EDMUND LEDGER.

Gresham College,
1879, Nov. 22.

Mars and the Schiaparelli Canals.

SIR,—

These canals are still reported visible by Prof. Schiaparelli. In a letter to the writer, under date October 27, Schiaparelli states:—"It is as impossible to doubt their existence, as that of the Rhine on the surface of the earth." A careful search has been made for them; but the definition afforded by the St.-John's Wood atmosphere has barely sufficed to identify the details of the Madeira drawings. Mr. Burton, observing near Dublin, has obtained some very good views, in which there are traces of similar forms to the canals of Schiaparelli, though they do not occur in the same positions; and in some drawings by myself faint and diffused tones may be seen in places where Prof. Schiaparelli states that new canals have appeared during this opposition. Should any of your readers possess recent drawings, especially of the neighbourhood of Terby Sea, I should be much obliged by a sight of them.

I am, Sir, yours faithfully,
NATH. E. GREEN.

39 Circus Road,
St. John's Wood, London.

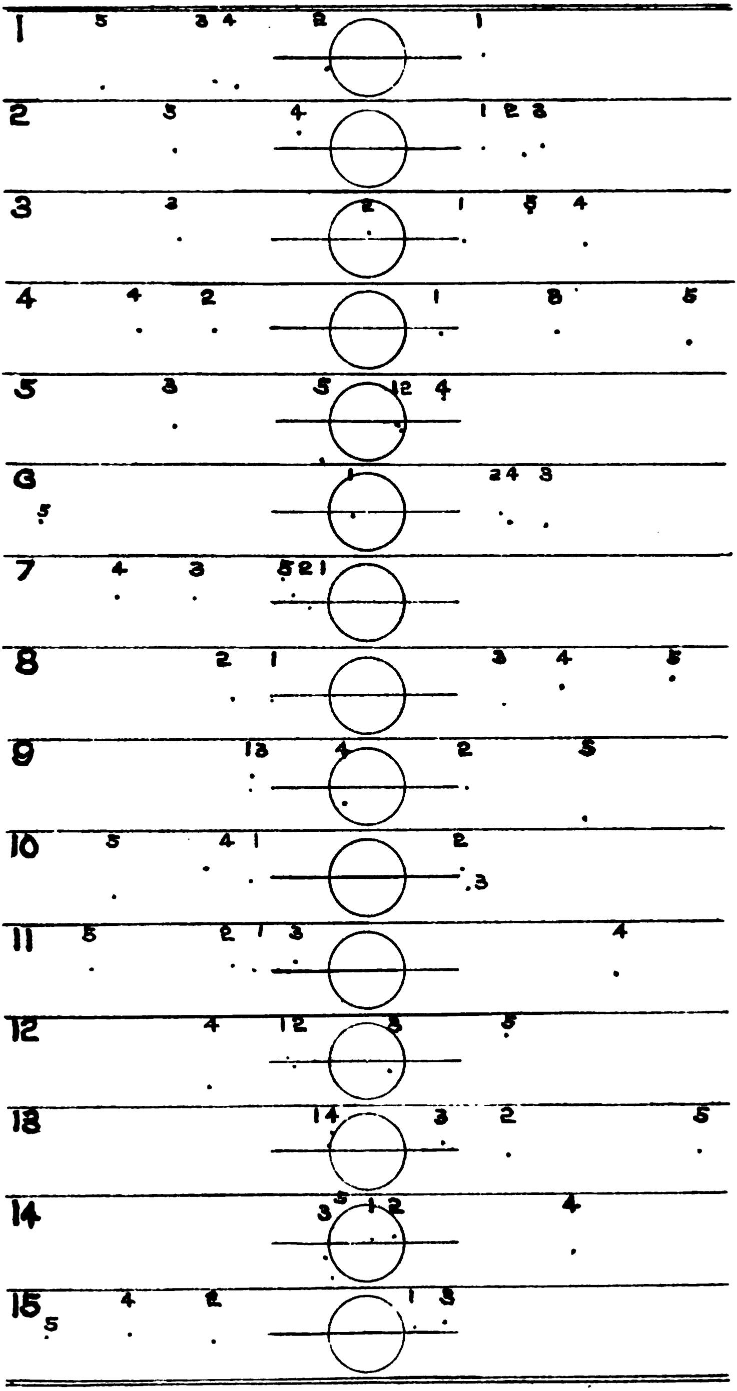
NOTES.

THE NOVEMBER LEONIDS.—The Leonids were watched for at the Royal Observatory, Greenwich, on the nights of November 12, 13, and 14, the circumstances on each occasion being very favourable. The particulars are as follows:—

Day	Duration of watch.	No. of Meteors seen.	From Leo radiant.	From other radiants.
Nov. 12	11ʰ to 14ʰ	13	6	7
13	11 to 15½	19	9	10
14	11½ to 15½	32	18	14
Total		64	33	31

The observations on these nights were made respectively by Messrs. Greengrass, Simmons, and Hugo.

APPROXIMATE PLACES OF THE FIVE INNER SATELLITES



Uranometria Argentina.—This important work, undertaken by Dr. Gould at Cordoba, in continuation of Argelander's and Heis's naked-eye surveys of the northern hemisphere, has just been published, and an interesting account of it is given in the Buenos Ayres 'Standard.' From this we take the following particulars:—The 'Uranometria Argentina' was intended to do for the southern heavens what Argelander and Heis in their Uranometries had already done for the northern, showing in a series of charts and accompanying catalogue all stars down to the seventh magnitude, the limit of visibility in the clear sky of Cordoba. Four assistants were engaged on the work, which was begun in 1870, and each portion of the heavens was independently mapped down by at least two of them, and the magnitude of every star carefully noted. Three years were spent in this survey, and since then Mr. Thome has twice reviewed the entire work, so that Dr. Gould considers it improbable that any star so bright as 7·0 magnitude has escaped detection, whilst the magnitudes are believed to be essentially correct to the tenth of a magnitude. The total number of stars is 10,649, and the total number of estimates of magnitude 44,510. All the stars have been observed at least four times each for accurate position with the Transit-circle. The charts are 13 in number, consisting of a polar chart extending 30° in radius from the pole and two belts of six charts each, reaching to 12½° north of the equator, and have been reproduced by photo-lithography. There is also an index map giving the whole limit of the Uranometry at a glance and containing stars down to the 5th magnitude. The accompanying catalogue gives the approximate position, the magnitude (that of each component being given in the case of a double star), colour, and range of brightness with length of period if a variable star. In the notes a complete history of all variable and suspected variables is given.

The last chapter of the work is devoted to a discussion of the distribution of the stars according to their brightness. Dr. Gould finds that by collecting all stars to the 9th magnitude and distributing them according to classes, there is always an excess in the number of the bright stars observed over those which result from calculation, on the supposition that the stars increase regularly in number as they diminish in brightness, but that the accordance is much closer for the faint stars. It is not assumed that the stars are all of the same size, and that they vary in brightness according to their distance from the observer—in fact we know that if our sun were removed to the distance of an average first-magnitude star it would shine as one of the third or fourth magnitude; but it cannot be doubted that the average distance of all the 5th-magnitude stars, for instance, is nearer to us than the average of all the 6th magnitudes. Reasoning from this excess of the bright stars, and the existence of a belt of bright stars girdling the heavens, inclined to the galaxy at an angle of 20° at its greatest deviation, and intersecting in two nodes 12 hours apart, he concludes that we are situated in a cluster, numbering probably 400 stars, and ranging in magnitude from 1st to 7th. This belt being very marked in the south, and less so in the north, he considers, that our position in it is on that side, in the direction of Hercules, the point in the heavens toward which our solar system is also believed by astronomers to be moving. We cannot do better than give a synopsis of Dr. Gould's conclusions as stated in his own words:—

"There is in the sky a girdle of bright stars, the medial line of which differs but little from a circle inclined to the galactic circle by a little less than 20°.

"The grouping of the fixed stars brighter than 4·1 is more symmetric relatively to that medial line than to the galactic circle, and the abundance of bright stars in any region of the sky is greater as its distance therefrom is less.

"The known tendency to aggregation of faint stars towards the Milky Way is according to a ratio which increases rapidly as their magnitudes decrease, and the law of which is such that the corresponding aggregation would be scarcely, if at all, perceptible for the bright stars.

"These facts, together with others that have been stated, indicate the existence of a small cluster within which our system is excentrically situated, but which is itself not far from the middle plane of the galaxy. This cluster appears to be of a flattened shape, somewhat bifid, and to consist of somewhat more than four hundred stars, of magnitudes from the first to the seventh, their average magnitude being about 3·6 or 3·7."

To determine our position with reference to the Milky Way an elaborate and systematic series of measures have been made along its entire course, as given on the charts and Heis's atlas. The circle resulting from these measures is so nearly coincident with a great one that our position may be assumed as nearly in its central plane. A close study of the measures for width of the main stream reveals the fact that there are two places of maximum breadth on nearly opposite sides of the sky, and two where the breadth is a minimum, also about 12 hours apart. At one of these regions of greatest width two branches are plainly and easily recognizable, the rift beginning near *α Centauri* and extending to *Cygnus* in the far north. The circles representing the course of these branches cross at the points of minimum width, seeming to show that there are really two galaxies going to make up the beautiful bright arch of the Milky Way. One of these branches presents a more widely diffused and fainter appearance than the other, which is very bright and comparatively narrow with sudden changes of brilliancy. This would seem to indicate that they are also at unequal distances from us.

PROFESSOR VAUGHAN ON THE ORIGIN OF THE ASTEROIDS.—It is to the general features of the numerous small planets beyond

Mars that we must look for a record of their past history. Though most liable to elude the search of the observers, asteroids of great orbital inclination to the ecliptic show already such numbers as to prove a stumbling-block to the usual methods of inquiry respecting the primitive condition of the planetary group. If, as Laplace supposes, they are parts of a ruptured solar ring, they could never deviate far from the plane of Jupiter's orbit by the attractive influence of other planets, and the greatness of the deviation as well as its independence of size excludes the idea of its arising from any mutual action exerted among themselves. The hypothesis of Olbers fails also to meet the difficulty. In the explosion of a single planet moving in the plane of the ecliptic, such an impulse as produced the great inclinations of the paths of Pallas, Euphrosyne, Gallia, Electra, or Artemis would give cometary orbits to masses launched forth in opposite tangential directions, and cause many asteroids of such an origin to wander far beyond the zone between Mars and Jupiter. Yet the peculiarities in question are precisely such as may be expected in a group of bodies owing their birth to a collision of two planets not very unequal in size or mass. Such an event would be possible when by long disturbance the orbits of both become too eccentric for safety, and when the aphelion of one was in conjunction with the perihelion of the other and near the intersection of their planes. Supposing both orbits to touch at the colliding point, the resulting fragments would be driven forth in a plane perpendicular to the course of their common centre of gravity. If the velocity from the impulse were thirty-two per cent. of that required for describing a circle, they would be constrained to move in ellipses varying in eccentricity between ·32 and ·1024; and about seven-eighths of the known asteroids conform to this limitation in the form of their orbits. Had the motion of both planets been merely progressive and coincident with the ecliptic, the orbital inclination for the fragmentary group would vary from 0° to about 19°, yet the range would pass many degrees beyond this limit through the influence of rotation in the great spheres in causing their matter to fly more rapidly into space in a polar direction in the early stage of the collision. Yet this rotational movement would prevent the eccentricities of the numerous orbits from assuming the proportions which might be expected if the line of motion of the colliding orbs did not pass through their centres as in the supposed case.

There is another clue to the cosmical history of the region between Mars and Jupiter. The orbits of Saturn's moons show a near conformity to geometrical progression; and, taking the common rates at 1·30756, the following table gives the empirical as compared with the actual distances in equatoreal semi-diameters of the primary:—

Mimas	3·3607	3·3607
Enceladus	4·3933	4·3125
Tethys	5·7483	5·3396

Dione	7·5182	6·8398
Rhea	9·8325	9·5528
	12·8600	
	16·8190	
Titan	21·9972	22·1450
Hyperion	28·7690	26·7834
	37·6250	
	49·2150	
Iapetus	64·3590	64·3590

It appears from this table that two consecutive satellites are missing in each of the chasms in the Saturnian family; and this evidence of their transformation into asteroids seems more reasonable on considering the disturbing influences of Titan and Iapetus on their planetary neighbours. The array of the satellites near Saturn would also lead to the belief of more worlds than one near the Sun, and the vain search for Vulcan would render more probable the existence of an asteroidal group within the orbit of Mercury.—*Popular Science Monthly.*

SOUTHERN MULTIPLE STARS*.—M. Cruls, the Director of the Imperial Observatory of Rio Janeiro, has been engaged for some time on the formation of a catalogue of southern multiple stars between the pole and 50° S.P.D., in the course of which he has noted some facts worthy of record.

1. In the beautiful stellar cluster round κ Crucis a comparison with Sir. J. Herschel's drawings and observations shows some notable changes. Three double stars appear to be certainly in orbital motion, and the star next to the red one shows a rectilinear displacement. M. Cruls has also registered a star of 6·5 mag., $1^{m}\ 4^{s}{\cdot}26$ *f* and $4'\ 14''{\cdot}6$ *s* of B.A.C. 4308, which he has not been able to find in any of the catalogues at his disposal.

2. The central double star of α Crucis appears to have a proper motion of about 1″ a year towards the S.S.E. relatively to the four other stars of the group.

3. The double star *h* 5193, noted by Herschel as following α Pavonis by $36^{s}{\cdot}5$, appears to have an annual proper motion of $-1''{\cdot}04$ in R.A., whilst further observations are required to determine that in declination. M. Cruls urges the importance of registering differences of R.A. and N.P.D. between a binary star and a neighbouring star for future reference.

4. M. Cruls has noted new distant companions to β and γ Crucis and *h* 4341.

THE NAUTICAL ALMANAC for 1883 has just been published. The principal change introduced is the application of Newcomb's Corrections to the R.A. and Dec. of the Moon as derived from Hansen's Lunar Tables. The Longitude, Latitude, Parallax, and Semidiameter have been taken unaltered from Hansen's tables. Also a table is given of the Greenwich corrections to the R.A.'s of the Nautical-Almanac Standard Stars for 1879.

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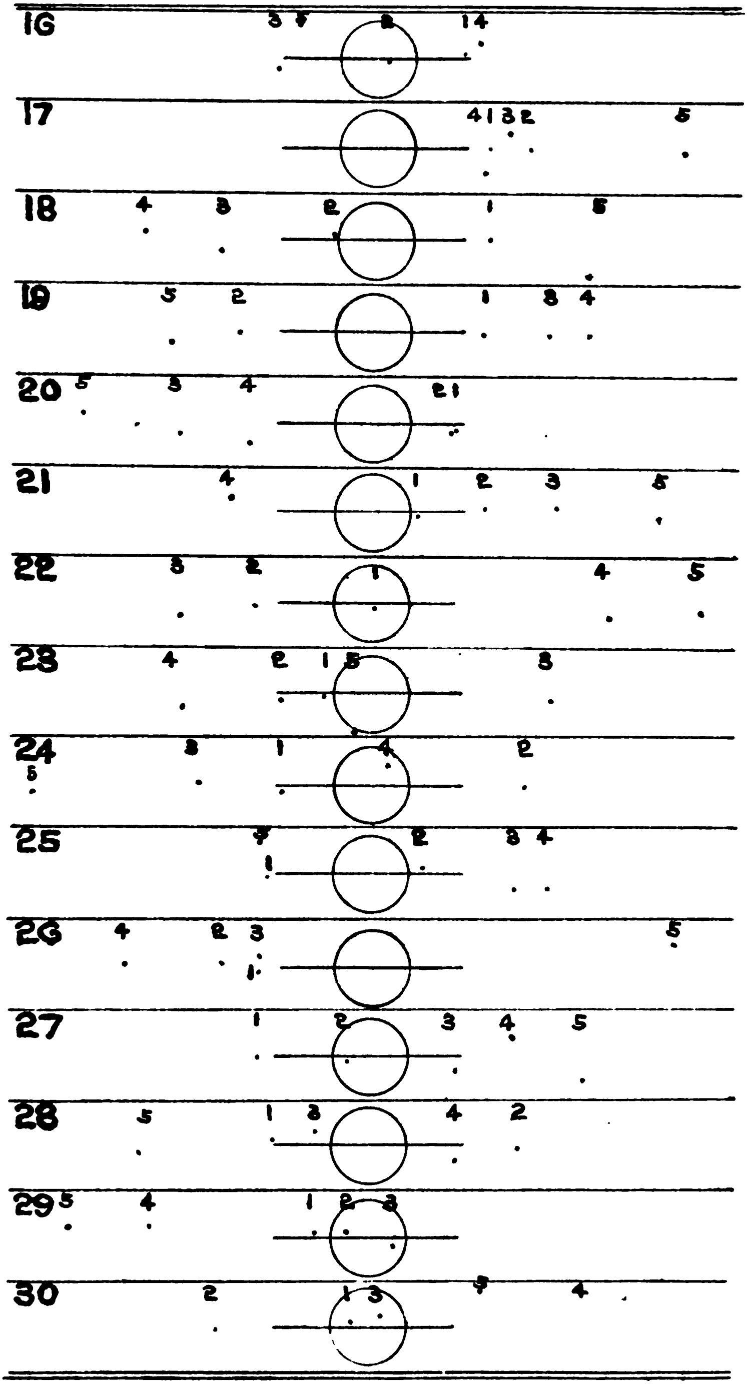
[*] Comptes Rendus, Vol. lxxxix. No. 8.

OF SATURN, FOR 8 HOURS G.M.T. DECEMBER. INVERTED



Nebulous Star.—On Nov. 14 the Rev. T. W. Webb noted a nebulous star, in Cygnus, 5″ in diameter, which had been recorded in the Bonn Durchmusterung as Zone +41°, No. 2004, Mag. 8·5. Mr. Knott examined the object on Nov. 25, and, with a Maclean star-spectroscope, found that its spectrum appears to consist of one bright line—very bright. At Dun Echt the spectrum was examined on Nov. 22 and 23, and was found to be approximately monochromatic. The position of the object is, for 1880, R.A. $21^h\ 2^m\ 31^s$, Dec. $+41°\ 45'·3$.

The Gresham Professor of Astronomy (Rev. E. Ledger) delivered a course of four lectures, at Gresham College, on the evenings of Nov. 11, 12, 13, and 14. In the first two lectures the Earth's rotation and its orbit were discussed with special reference to "Climate and Time." The last two were devoted to the planet Mars, and its satellites Deimos and Phobos. Mr. Green's views of Mars and his recently published map were exhibited on a screen by means of the limelight. The lectures were attended by an audience of between 300 and 350 persons on each evening, many of whom were teachers or pupil-teachers in schools in various districts of the metropolis.

Mr. B. Kidd writes to explain, with reference to his letter ('Observatory,' No. 31, p. 207), that what he saw was not a retrogression of the satellite, but merely a separation consequent on using a higher power, just as a double star may be seen separated with 180, that appears single with 100. His point was, "seeing the satellite *through* the disk of Jupiter; and that was as evident as any thing else one sees, whether an illusion or not."

Four new Minor Planets have been discovered since our last report, viz.:—No. 207 on Oct. 17, and No. 208 on Oct. 21, by Herr Palisa, at Pola; No. 209 (Dido) on Oct. 22, by Prof. Peters, at Clinton; and No. 210 on Nov. 12, by Herr Palisa. Editor.

The Satellites of Mars.

Greenwich Mean Times of Elongation: Pos.-angle 52° or 232°.

	Phobos.		Deimos.
Dec. 1	8·0*p*	11·8*f*	8·4*p*
2	7·0*p*	10·8*f*	14·7*p*
3	5·9*p*	9·7*f*	5·8*f*
4	8·7*f*	12·5*p*	12·1*f*
5	7·7*f*	11·5*p*	18·4*f*
6	6·6*f*	10·5*p*	9·5*p*
7	5·6*f*	9·4*p*	15·8*p*
8	8·4*p*	12·2*f*	7·0*f*
9	7·3*p*	11·1*f*	13·3*f*
10	6·3*p*	10·1*f*	4·4*p*
Dec. 11	5·3*p*	9·1*f*	10·7*p*
12	8·0*f*	11·9*p*	17·0*p*
13	7·0*f*	10 8*p*	8·1*f*
14	6·0*f*	9·8*p*	14·4*f*
15	8·7*p*	12·6*f*	5·6*p*
16	7·7*p*	11·5*f*	11·9*p*
17	6·7*p*	10·5*f*	18·2*p*
18	5·6*p*	9·4*f*	9·3*f*
19	8·4*f*	12·2*p*	15·6*f*
20	7·4*f*	11·2*p*	6·8*p*

Semi-axes of Orbits:—

	Phobos.		Deimos.	
Dec. 1	23″·6	7″·1	58″·8	17″·6
Dec. 20	19 ·5	6 ·1	48 ·7	15 ·3

The elongations *s p* and *n f* recur at intervals of $3^h{\cdot}8$ for Phobos and $15^h{\cdot}15$ for Deimos.

The times of elongation have been derived from Prof. Asaph Hall's data, applying a correction of -44^m in the case of Phobos, as found by his recent observations, and also a correction of $+4^m$ for aberration of light.

Prof. Asaph Hall has observed the satellites regularly since Oct. 12, and finds that Phobos comes to its elongations 44^m before the predicted times, the error in areocentric longitude of his ephemeris being +34°·3 (rather less than the estimated probable error), so that the periodic time ought to be diminished by $1^s{\cdot}074$, and would thus be $7^h\,39^m\,13^s{\cdot}996$. In the case of Deimos the error in pos.-angle appears to be only −0°·22, so that the satellite is almost exactly in its calculated place. These results must be regarded as highly satisfactory.

Mr. Common has accumulated a considerable number of observations of both satellites, and a series of micrometer measures of Deimos was obtained at Greenwich on one night (Nov. 12).

Satellites of Saturn.

Mr. W. E. Cooper, The Mount, Worcester, has prepared, from Mr. Marth's ephemeris, a sheet of diagrams showing the positions of the five inner satellites at 8^h G.M.T., for the month of December. The positions at any other hour may easily be inferred by help of the hourly motion in the orbit.

Semi-axes of orbits, expressed in radii of Saturn:—

	Mi.	En.	Te.	Di.	Rh.	Ti.	Iap.
Major	3·14	4·03	4·99	6·40	8·93	20·71	60·34
Minor	0·41	0·52	0·65	0·83	1·16	2·69	7·84
Daily motion.	382°	263°	191°	132°	80°	22°·6	4°·5
Hourly motion	15°·9	10°·9	7°·9	5°·5	3°·3	0°·9	0°·2

The reader is requested to correct the following slips in the lithographed diagrams:—Dec. 2, Sat. 5 should be *above* line of rings; Dec. 30, Sat. 4 should be *above* line of rings; Dec. 21, Sat. 5 should be *above* line of rings, and at a distance of 3½ radii.

Satellites are in front of the planet and move from right to left when below or north of the line of rings, and behind it when above.

Publications received:—J. Rand Capron, *Auroræ, their Characters and Spectra* (E. and F. N. Spon)—J. Rand Capron, *Photographs of the Moon taken at Guildown Observatory*, 1878—Piazzi Smyth, *The Solar Spectrum in* 1877 *and* 1878 (Trans. Roy. Soc. Edinburgh, vol. xxix.)—Annibale de Gasparis, *Sviluppo in Serie secondo le potenze del tempo del valore inverso del cubo della Distanza Variabili di due Pianeti* (Napoli, 1879). Editor.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

No. 33. JANUARY 1. 1880.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, 1879, December 12.

Lord Lindsay, M.P., F.R.S., &c., *President*, in the Chair.

Secretaries: Mr. J. W. L. Glaisher, M.A., F.R.S., and
Mr. A. Cowper Ranyard, M.A.

Mr. Glaisher reported that 58 presents had been received, including some volumes of the 'Journal' of the École Polytechnique.

The President read a letter from Mr. Bartlett, one of the churchwardens of Stone, appealing for help from the Royal Astronomical Society towards the restoration of the church, the cost of which is estimated at £3000.

This letter, with its enclosure, the President stated, was brought before the Council Meeting this afternoon; and it was there decided that as an Astronomical Society they had no funds whatever to lay out on such matters as these. If any Fellow of the Society is desirous of aiding in this matter, it must be purely as an individual; and therefore he was directed to read the letter this evening to the Fellows at large.

Mr. Stone placed upon the table the model of a micrometer; and he asked the Fellows to look at it by-and-by, and to offer suggestions upon it. It was simply a compromise between the German form and the English form.

The President. If I understand, there is only one web in it.

Mr. Stone. There may be cross wires. There are two parallel wires. It is an ordinary reading microscope.

Mr. Neison read a paper by Professor Newcomb on the variations in the apparent errors of the Lunar Tables due to the new term in the motion of the Moon arising from the action of Jupiter.

Mr. Neison also read a paper on the correction for personality required by the observations of the Moon made with the Greenwich

Transit-circle, the purpose of which was to show that a sort of fictitious increase in the Moon's errors, amounting to over 1″, had been created since 1870, simply by the change of observers at Greenwich. Mr. Dunkin, in a paper read to the Society some time back, showed that there was a personality in observing the limbs of the Moon; and Mr. Neison now pointed out that between 1870 and 1875 three observers (Messrs. Dunkin, Ellis, and Carpenter), who had, on the whole, a negative personality, had left off observing, and had been replaced by three other observers (Messrs. Lynn, Downing, and Thackeray), all of whom had a large positive personality. The effect of this was that the mean personality of the four observers instead of remaining sensibly constant had changed from a negative to a positive quantity, causing a fictitious increase in the Moon's errors. Prof. Newcomb in his paper points out that the term discovered by Mr. Neison would account for some of the variations in the Moon's errors; and Mr. Neison finds, from a discussion of the Greenwich observations, a coefficient 2″·31 for this term, which is almost identical with that found from theory.

Mr. Dunkin. I am sorry I was not in the room when Mr. Neison first brought forward this question; I should therefore like to know, in the first place, and for the information of the Meeting, at what date Mr. Neison commenced these comparisons.

Mr. Neison. In 1863.

Mr. Dunkin. Because, of course, it would be very important and valuable if he could have gone back a little further, and have also compared the personal equations of former observers, and I should have liked personally to have seen what results the old observations would have given. Supposing he had gone back to the commencement of 1847, when the altazimuth was first erected, then we should have seen distinctly the course of the error from the date of the construction of that instrument; or even further back than that, even to the old observers before my time, such as Mr. Henry, Mr. Ellis senior, and Mr. Rogerson; because Hansen's tables are founded upon the old observations at Greenwich; and I should like to know whether the large personalities which we have here exhibited existed with the old observers as well as with the modern observers. Has Mr. Neison tried the question with the observers from 1836?

Mr. Neison. I have not gone beyond 1863.

Mr. Marth. The Moon's errors for those old observations are calculated from Burckhardt's tables.

Mr. Dunkin. Still we might get some relative differences from Burckhardt's as well as from Hansen's tables. We all now know that there are very great personal equations in observations of the Moon. When the altazimuth was first erected in 1847, nobody could have believed that there would have been so much difference between the observations of two observers as was found between the alternate observations of the two first observers with the altazimuth. It was remarkable to see from day to day the up-and-down motion of the tabular errors as given by these two observers. It was then considered that he observed most correctly whose observations agreed with those of the old observers at Greenwich, especially with Mr. Henry.

Mr. Neison. As to what Mr. Dunkin has suggested about a comparison of the old observers, the only objection that I have is that the old observations are compared from 1847 with Hansen's tables upon the assumption that Burckhardt's tables and Hansen's tables might be fairly compared at midnight, and the observation might be trusted to hold good at the time of transit. Unfortunately it does not hold good. The period of several of the inequalities is very short, only two or three days, so that you cannot extend the observations beyond 1862 at the earliest. The result I have arrived at is that if we compare the observations of Mr. Ellis and Mr. Thackeray at the first limb we shall find a difference amounting to $0^{s} \cdot 12$, nearly 2″ of arc, in the place of the Moon. And if we take the second limb, Mr. Ellis and Mr. Thackeray would make a difference amounting to more than 3″ in the place of the Moon. This large error on the part of Mr. Thackeray rests upon very few observations, for he only commenced in 1875. It is possible when we get more observations this result will diminish. I am waiting till I get the result of the years 1877 and 1878. We are at present dependent on observations made at Washington and at Greenwich, which renders it more difficult to determine any variation due to personality on the part of the observers.

Mr. Dunkin. As I mentioned last month, 10 years ago I introduced a paper to the Society on the personality of observing the limbs of the Moon, which of course referred merely to the first four observers on the list, Mr. Dunkin, Mr. Ellis, Mr. Criswick, and Mr. Carpenter; and the amount of personality determined in that paper agrees very nearly with that given by Mr. Neison. At that time Mr. Lynn was not observing, and Mr. Downing and Mr. Thackeray were not at the Observatory. Mr. Neison's deductions are very interesting; but they are not new, because we have all known for years that those differences in the method of observing the Moon's limbs will occur between different observers. Anybody who has been observing the Moon from year to year, as we are accustomed to do at Greenwich, could not do so long without noticing that there are tremendous personalities in observing the limb of the Moon as it passes the fixed wires in the telescope.

Mr. Ranyard read a paper from the *Rev. T. W. Webb* on a new gaseous nebula*.

The President. I may add to the information contained in that paper that my attention was drawn to this nebula by Mr. Ranyard, who was kind enough to forward to me a portion of the telegram which he had received from Mr. Webb, thinking that it was a new comet; and on examining the object, I was able to identify it with the Star Bonn Catalogue Zone $+41^{\circ}$, no. 4004.

I sent out a circular to a good number of observers in this country and also on the continent about it. On the 23rd of November we examined its spectrum at the Observatory at Dun Echt, and it was seen to be approximately monochromatic.

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* This is the nebulous star to which attention was drawn in 'The Observatory,' No. 32, p. 257.

This observation was repeated on the following evening, and a short tail or wisp to the star was suspected.

The President read two letters from *Prof. Winnecke*, stating that the new nebula as seen with the 6-inch "orbit-sweeper," power 260, looked like a star of the 8th magnitude; was oblong in the direction of 136°, with a lucid point like a star of the 10th or 11th magnitude, the greatest diameter being $5''\cdot5$ and the least $4''\cdot9$. The nearest star which he could see in the bright moonlight was of the 11th magnitude, distant 135″, position-angle 152°. The spectrum was monochromatic, crossed by a long faint ray to be ascribed probably to the spectrum of a small nucleus.

Mr. Knott read a note on the same subject.

Mr. Ranyard. Do you think there is a change of magnitude?

Mr. Knott. Not at all. I gauged the magnitude as 8·5; that was the magnitude given by Argelander, and I should think there was no change.

I have also a paper which relates to a small new star in Canis Minor.

The President. I beg to supplement the remarks of Mr. Knott on this paper. Examined with the spectroscope the star showed no peculiarity, no particular part predominated. To the eye, the colour is sensibly a faint red or purple. A comparison of the brightness from Nov. 27 to Dec. 8 indicated a decrease of half a magnitude. On Dec. 10, when seen under very favourable circumstances, the star appeared about as bright as on Nov. 27, say 8·9 of Argelander's scale.

Mr. Sadler. Are we to understand that this star is not in Mr. Burnham's catalogue?

The President. It is supposed to be a new star. A message was sent us, so far as I recollect it, "Discovered last night a new star in Canis Minor." I have not been at home since so as to verify it.

Mr. Common read a paper on a series of observations of the satellites of Mars:—Deimos was seen on Sept. 21, 26, Oct. 15, 21, Nov. 2, 5, and 11, and Phobos on Nov. 2, 5, 11, 13, and 15. It has been a matter of surprise to me that no one else has seen the satellites of Mars, because when I had an excellent opportunity of comparing them with Saturn's satellites on Nov. 2, I considered that Deimos was about equal to Enceladus, and Phobos was a little brighter than Tethys. That is not taking into consideration the glare of the planet in either case. The light was quite different from that of the satellites of Saturn, it was sparkling and star-like. This may be due either to the absence of any apparent disk which one always fancies one sees in the satellites of Saturn, or to the contrast with the brightness of Mars. The colour of Deimos was slightly bluish; Phobos was quite white. On the 2nd of November the Moon was very bright, but it had no apparent ill effect upon the satellites; if any thing it seemed an advantage. I was very careful, with reference to the wish expressed by Mr. Webb, to look out for a third satellite, and I observed a great many stars that were near to the planet Mars; but they all moved in relation to the planet, or the planet moved in relation to them.

Mr. Christie in the 'Observatory' advised the use of a dark glass, covering half the field of the positive eyepiece; and I had before been using as a cover to the planet, while observing the satellite, a steel disk made out of the second's hand of a watch which I got from a watchmaker's. Mr. Christie's suggestion induced me to do away with the steel disk and substitute a very small piece of dark glass which I had prepared, and I found it a most useful thing when observing Saturn or Mars. If it is so placed on a movable arm that it can be taken away or brought over the intersections of the cross wires, an excellent means is obtained of getting a very good position-angle of a small satellite near a bright planet in the way described in this paper.

I have also a note on Mimas and Hyperion. Until I saw Mimas with a 36-in. telescope a few months ago, I had very strong doubts in my own mind whether it existed at all, for I had been trying after it with an 18-in. for about two years. Also I had a letter from an observer who was observing with about one half the aperture, and he gave me the times when he saw it, always at elongation, exactly at the time which Mr. Marth gives in his ephemeris. Now I was looking for it at the time of elongation, and not only myself, but a friend with me, but we never saw it. I happened to be observing Saturn on Sept. 25, at $9^h\ 15^m$, and to my surprise when I expected Mimas to be goingout and coming to elongation, it was found to be approaching, and at $10^h\ 50^m$ it appeared to be very much closer. I could not find it after $10^h\ 50^m$. I saw it again on many occasions and got four reliable observations. I have seen the satellite Mimas when according to the ephemeris it should not be visible; and I have no hesitation in saying that the ephemeris is from $1\frac{1}{2}^h$ to 2^h late. I had some observations possibly of Hyperion; but in the absence of an ephemeris or any information from any person, I must say "perhaps" yet. It appears that on Nov. 11 it was seen west, and on Nov. 13th and 15th it was approaching superior conjunction. On the 18th it was seen just above superior conjunction; and from the orbit I have made out I consider it very likely indeed to be Hyperion.

Mr. Marth. I should be extremely glad to get observations which could be relied on. If I have been misled, it is by a series of observations made by several observers. For the period I used Herschel's observations of 1790 and those of 1864 made by myself. I know that those two epochs are trustworthy, and at present I do not see how later observations can be reconciled with any thing like ordinary regular motion. There may be considerable perturbations which can only be found out if we get a series of observations, which I have begged for for years past, so that I really am quite shy of saying any thing more about it; but now, as Mr. Common has made some observations, perhaps December may be favourable to his obtaining a few more, and it may be possible to give next year predictions more trustworthy than those which I have made this year.

The President. We all wish to echo Mr. Marth's last words, that the weather may be favourable for Mr. Common to prosecute this valuable study.

Mr. Marth. I should wish this matter to be decided,—whether the satellites of Mars can be seen after December 16.

Mr. Sadler. On the 31st of November, 1877, I think Mimas was considerably under the 13th magnitude.

Mr. Neison. In 1878, while I was with Mr. Campbell, we observed the satellites of Saturn, and we marked it down as a satellite. Mr. Campbell and myself picked it up perfectly independently, and I came to the conclusion that it really was Mimas. On bringing the observations up to London, I met Mr. Marth, and told him I had seen Mimas; and he said, "It is very doubtful; your observation is an hour and a half out; and if so, it cannot possibly be the satellite Mimas." But we had looked at it repeatedly, and it looked like a satellite not far from the ring. Now Mr. Common finds that the satellite is really just as much out as the observations which were made a year and a half previously showed, and those observations indicate an error of exactly the same amount. I should come to the conclusion that Mimas is not such a difficult object to see if you observe it with a perfectly clear atmosphere, with a telescope of considerable excellence. If you get any false light, or any haze, there is not the slightest doubt that it will blot the satellite out entirely; but when we consider that it was seen with Sir W. Herschel's telescope, there is no doubt that any thing that could be seen with that telescope could be seen in a modern 9-in. or 10-in. instrument.

Mr. Common. Will you kindly mark upon the black-board a sketch of Saturn, and where you saw the satellite Mimas?

Mr. Neison. It is drawing much upon my recollection of a year and a half ago; but I will try it. (*Mr. Neison drew a sketch of Saturn and the satellite on the black-board.*)

Mr. Marth. This observation was made in 1878; and, as it happened, several other observations show that at that time my ephemeris is out only 8°, which amounts to less than half an hour, and now it is out more than 30°. This might be due to a mistake in the date; but unfortunately I cannot find any mistake. The motion of Mimas is something like 382° every day.

Mr. Sadler. It has been seen at Rome with the 6·3-in.; and it has been seen at Madras with a very inferior telescope of 6·4-in.

Mr. Marth. The statement that it has been seen must be received with very great caution. There has been a series of observations by perfectly trustworthy observers; but it is impossible to reconcile them with any kind of mean motion. I strongly recommend all observers to look very sharply to see whether there is not some other body in the neighbourhood which creates all this difficulty. But as regards the mere statement that it has been observed—well, I confess I question it; and until computation shows that those observations can be reconciled in some way or other, we ought to be very cautious in accepting the statement that it has been seen.

Mr. Campbell. I may say that on the occasion to which Mr. Neison alludes, we neither of us expected to see Mimas, and we both perfectly independently put it on paper, although not quite in the same position in which Mr. Neison has put it on the blackboard, as well as I recollect; but he himself saw it, as I did, a little higher and a little nearer to the ring. We both of us put a star or a body of some sort near the ring. I myself did not believe that the telescope could show it, and I am not at all convinced that it did.

Mr. Marth. Will observers of Mimas kindly try, under any circumstances, to give two diagrams, the second being made an hour later, in order to show clearly that this body has moved as it ought to move. I have regularly for the last three years given the change of relative position which a star ought to undergo.

Mr. Common. This is rather an important question touching the credibility of observers. I quite agree with a gentleman who wrote lately that astronomers are very loose in their description of astronomical phenomena; but what I was about to remark is this, that in 1878 the ring was not open nearly so wide as it now appears. I have observed Mimas several times and tried to follow it up to conjunction, but I have never been able to get beyond a certain point. When I tell you that with an 18-in. I never had a real observation of Mimas in my life, although I find undoubted observations of the satellites of Uranus, which I do not think any 6-in. telescope has yet touched, I have the strongest reason for agreeing with Mr. Marth, and for saying that no 6-in. telescope will show Mimas.

The President. I wish to add something to the paper on the satellites of Mars. I have a few observations here which we have been fortunate enough to get at Dun Echt, and amongst others two on Nov. 17th. The first two observations in the paper are probably vitiated from the fact that Deimos was passing very close to a star equal to itself in brightness.

I tried a dark disk in the eyepiece, but I found that it was not so satisfactory as making a very contracted aperture in the eyepiece. I found also that you could save yourself trouble at any time by merely looking up in the observatory with the shutter open and arranging yourself on the floor so that Mars was just behind the shutter. If with the naked eye there was the slightest trace by which you could recognize the presence of the planet in the sky when just behind the shutter-opening, it was of no use looking for the satellite.

Mr. Christie. May I explain that the suggestion as to the use of a dark glass to diminish the glare of the planet is not my own?

It was used in the observations of 1877 at Harvard College Observatory.

Mr. Common. If your lordship will allow me I will add a suggestion from my paper that three parallel webs should be used in the micrometer, the satellite being bisected by the middle one, and the two outer being made to cut off equal segments from the planet.

Mr. Christie. I may mention with regard to Mr. Common's suggestion of three wires, that that system has been in use at Greenwich for some little time, and I can speak from experience that it is very successful.

The President. I have it also in a micrometer made for me about eight or nine years ago.

The President then read a note on the spectrum of the red spot on Jupiter.

Mr. Christie. I may mention that Mr. Maunder examined this spot on Jupiter, but he was not able to detect any thing peculiar about it, beyond the obscuration of the whole spectrum.

Captain Noble. With reference to some of the discrepancies or disagreements which have appeared as to the colour which has been ascribed to this spot, I have myself noticed since our last meeting, on one or two occasions, that it is very much brighter and more scarlet in the twilight than it is as the evening gets on, when the spot very perceptibly darkens in character, and becomes more of a maroon colour.

The President. Perhaps Mr. Christie will some time try a spectrum of the third satellite of Jupiter and see if he gets any results.

Mr. Christie. I shall be happy to try it.

Mr. Ranyard read a letter from *Father Perry* dated Dec. 9th:—
"The egress of Jupiter's first satellite was observed last night. The first satellite is not so variable as the third and fourth; but last night, for more than three quarters of an hour between the time it left the disk and the setting of the planet, it was only visible by glimpses, although the other satellites were clearly visible."

Mr. Sadler. Spots had been seen on all the satellites of Jupiter by Father Secchi.

Mr. Common. It was pointed out to me by Mr. Knobel that the first satellite was very much fainter than the second, which is close by. Perhaps the photograph may throw a little light upon that, so far as regards measuring the actinic power of light, as it shows the first satellite is very much fainter than the second or the third.

Mr. Ranyard. As far as I remember, Zöllner showed that the reflecting power of different parts of the satellites varied very much, and at different parts of their orbit they varied greatly in brightness.

Mr. Downing stated that he had examined his comparison of the N.P.D.'s of the Cape and Greenwich Catalogues for 1860 to see whether he could detect any discordance in the Greenwich places depending on R.A. analogous to that which Mr. Stone had deduced from his comparison of the Cape Catalogue for 1880 with the 'Nautical Almanack.' For this purpose he had arranged in order of R.A. the stars used in the comparison, which are situated between N.P.D. 110° and the horizon of Greenwich, as it is reasonable to suppose that, for these stars, any effect that can be traced to refraction depends on the Greenwich rather than the Cape observations. The quantity $-0''\cdot18$ was applied to the Greenwich place of every star used, to allow for systematic difference between the catalogues.

The result then is :—

R.A.	0^h–6^h	6^h–12^h	12^h–18^h	18^h–24^h
Corr. to Greenwich	$-0''\cdot10$	$-0''\cdot04$	$+0''\cdot26$	$-0''\cdot11$

The only one of these corrections worth attention is probably that for 12^h–18^h; and if this may be assumed to be due to refraction, it appears to show that the refractions used at Greenwich are too small for the observations made during May, June, and July. This is, in fact, a partial corroboration of Mr. Stone's result, but makes it appear probable that part of the discordance which he has exhibited is due to the Greenwich observations. The correction for 12^h–18^h given above is confirmed by the comparison of the Cape and Melbourne places between the same limits of R.A. and N.P.D., as, when the systematic difference between the catalogues is allowed for, it is found that the correction to the Cape is only $-0''\cdot01$.

Mr. Stone. That appears to be very much the same kind of result that I got from a comparison of the others. It is a misconception to suppose I stated that those errors arose entirely from the Cape. All I stated was that there was a slight difference between the Cape and the 'Nautical Almanack;' and it proved that the refraction tables do not correct the effect of change of seasons. I am not going to say any thing against the Melbourne observations; but there appears to have been a doubt whether the comparison that I made some years ago is trustworthy or not. Without saying whether it is trustworthy or not, I will say that if not trustworthy, you cannot trust the Melbourne results, because those observations are corrected from that very paper. The results would not agree at all with those of the Greenwich observations unless that correction had been applied which had been got from my paper. So that the Melbourne observations have been compared with the Greenwich ones. A correction was got by me to render them generally in harmony; and that correction was applied, after consideration, by the Melbourne people, to their general results; and the results that appeared in the catalogue are those corrected results. So that when you come to nice points of comparison, it is to be remembered that they have been, to a very large extent indeed, already made to agree with the results of Greenwich and the northern observatories. As soon as the Cape Catalogue was made, I compared it with the Greenwich 1860 Catalogue; and if anyone will refer to the Introduction to the Catalogue, he will see that I have there discussed the refraction tables, but did not correct them from the Cape observations; because, on the whole, comparing them with Greenwich, it appears to me that they do not require any very large correction, and I feared to trust the result above and below pole.

Mr. Downing. In my comparison of the Cape and the Melbourne Catalogues, I have shown that, near the horizon of Melbourne, the Melbourne N.P.D.'s are considerably in error, so that, as a matter of fact, it appears that the refractions used at Melbourne are not correct, and do not make the Catalogue places agree with northern observations. With regard to this comparison which I have brought forward to-night I only used the stars within 15° of the zenith of the Cape and within 20°, I suppose, of the zenith of Melbourne, so that an error in the refraction used in the reduction of the observations at Melbourne will not materially affect the result.

Mr. Marth. I should like to make an appeal to Mr. Stone. Would he be kind enough to publish the computations which are the foundation of the whole investigations, in order that the differences may be reconciled, so as to give something of the details by which he has arrived at his general results?

Mr. Stone. So far as I am concerned, I can explain what was the state of the case. At that time the refractions were computed down to Z.D. 82° with one set of refraction tables. From 82° to 85° the refractions were reduced in a certain proportion, and below 85° they were again reduced. I must confess that I consider such a proceeding most unsatisfactory. I did not care about very small quantities; but we had got here rather large quantities. Bessel, in his refraction tables, has got two disposable constants, and he uses up one of these by making the refractions near the horizon agree pretty well with the observations; and the other one is afterwards used up by making the mean refractions agree. Now if it turns out that his refractions agree pretty well up to about Z.D. 82°, and that they then require an alteration in the mean refraction, and another alteration beyond Z.D. 85°, then I say that his assumption is altogether illegitimate, and he has not found a continuous chain throughout. I know the difficulties in the matter; but I am satisfied of this, that if the Greenwich circle will not give you observations sufficiently accurate to determine the refractions from, then you cannot expect to get better results by comparing observations with that circle with those made by another. If the comparisons above and below the Pole will not give you satisfactory results, I hardly know what to say. Bessel's refraction tables themselves were based upon observations of the same kind; and I am not prepared to admit that the observations I used are worse than those he used. I have compared them since, and they give that refraction again; and Mr. Christie has also compared them.

Again the latitude is mixed up with the results, and it depends largely upon the refraction tables used. Then there are a certain number of small systematic errors in every circle that I know of. You may look through the results, and whenever you get the means of fairly checking them, you will find them out. The

Washington observations are supposed to be good, but certainly there are distinct indications of systematic errors in them. The Greenwich observations I believe to be thoroughly good, but there are indications of systematic errors in them; and although I did every thing I could to avoid systematic errors at the Cape, I know that I have got them. If you confine your attention, in trying to get corrections to the refraction tables, to an arc, throughout which the refractions do not become very large, any systematic error cropping out at the important points is likely to give rise to very unsatisfactory results. If you compare the observations above and below the pole for some years you will find that they are not to be reconciled with each other, except by something like a diminution of refraction. With regard to my Cape observations, I think when my catalogue is published it will be seen that the Cape observations give a basis for determining the refractions. Mr. Downing compared them with the Washington observations, and he deduced a diminution of refractions. I certainly do not think it necessary to republish the paper I have spoken of, because anybody can verify it in a very few hours.

Mr. Christie. The same desire occurred to me as to Mr. Marth to see the work of Mr. Stone's paper; but I have since gone through it, and treating it in a slightly different way I have got substantially the same results. So that I think there can be no doubt that from the observations of these particular years above and below the pole you must get the same result. Of course the question arises whether the certainty of the observations is sufficient to justify us in assuming a change in the refractions; that is quite another point; and on that point I think I am rather at issue with Mr. Stone, because during these years I find that the observations of the Sun which give, in fact, the position of the equator show that the change of the refractions ought to be in the opposite direction: and if you diminish the refractions and diminish the co-latitude you will throw the position of the equator very much out, as much as a second of arc. We have not only to reconcile the observations north of the zenith, but the observations south. These things get mixed up with the latitude and various things of that kind; and it is very difficult to settle any thing positively when you are dealing with small quantities, because there are systematic errors coming in, as Mr. Stone has just pointed out. I have reason to think that the systematic errors of large circles are very much larger than most observers suppose.

Mr. Marth. I am very glad that this subject will be considered. When Bessel undertook the reduction of the Greenwich observations he had such boundless admiration for Bradley that he took them all for granted. He had at that time never made an observation himself. It appeared to him that the refractions near the horizon were too large; and when he came to make an observation himself he came to the conclusion that any thing beyond 85° would not be a strict investigation. If Mr. Stone will republish his paper and show us what is the foundation of his investigation I shall take the liberty of discussing it.

Mr. Stone. You have not quite caught my point. I quite admit that Bessel gives up his refraction tables below 85°, but he should not have done that after he had used up one of his constants in reconciling the observations near the horizon. If his theory will not carry him to Z.D. 85° it will not carry him to the horizon.

On the motion of Admiral Sir Erasmus Ommaney, seconded by Mr. Stone, Mr. Lecky, Mr. J. Campbell, and Mr. Sidney Waters were elected Auditors.

The following papers were announced:—

R. L. J. Ellery. Occultation of 64 Aquarii by Jupiter observed at the Melbourne Observatory.

R. H. M. Bosanquet and *Prof. A. H. Sayce.* The Babylonian Astronomy.—No. 2.

S. W. Burnham. New Double Stars.

A. de Gasparis. Sur la variation du demi grand'axe des orbites planétaires.

Rev. S. J. Perry. Occultation of Stars by the Moon and phenomena of Jupiter's Satellites observed at the Stonyhurst Observatory.

Rev. S. J. Perry. The November Meteors.

W. T. Lynn. On recent changes in the mean Error of Longitude of Hansen's Lunar Tables.

Lord Lindsay. Note on General Catalogue (supplement) No. 6000.

A. Marth. Ephemeris for finding the positions of the Satellites of Uranus, 1880.

The Meeting adjourned at ten o'clock.

Observations of the Satellites of Mars at Princeton, U. S.

It may be of interest to note that both satellites of Mars have been repeatedly seen here during the late opposition with our 9½-inch Yanssian objective (by Clark).

I have never myself been able to see either of them steadily except by hiding the planet behind an occulting-bar in the field of the eyepiece; but my colleague, Professor Brackett, whose eyes are better than mine, on Nov. 10th saw them both perfectly in the presence of the planet.

On Nov. 15 I succeeded, more as a matter of curiosity than any thing else, in getting a small series of micrometer measures of Deimos, both in position and distance. This was done by setting one wire on the planet, then moving the eyepiece (which is carried by a slide) until a little piece of paper gummed to the field-lens hid the disk, and finally setting the other wire upon the satellite, making the wire visible by occasional faint flashes of light into the field. Prof. Hall informs me that the measures agree with those made at Washington the same night, within 1″ or 2″ in distance and about 1° in position.

The record of observations stands as follows, the distances and positions being mere estimates, except on Nov. 15, when they were measured, as stated above.

Oct. 20, 10^h to 12^h Washington M. T. Deimos near western elongation; watched long enough to identify it as a satellite by its motion with reference to neighbouring stars. Seen both by myself and assistant, Mr. M'Neill: power 400.

Oct. 25,	10^h	50^m	W. M. T.	Deimos,	pos. 240°,	d. 50″
31,	9	0	"	"	40	45
Nov. 7,	8	0	"	"	220	35
7,	8	50	"	Phobos,	55	25
10,	9	20	"	Deimos,	40	50
				Phobos,	230	25
15,	9	32	"	Deimos,	48	
15,	9	45	"	"		60·5
15,	10	17	"	"		56·8
15,	10	27	"	"	45	
15,	12	5	"	"	41	
15,	11	50	"	Phobos,	235	25
16,	13	0	"	Deimos,	60	60
21,	9	25	"	Phobos,	60	22 (seen only

by glimpses, but unquestionably. Moonlight troublesome).

Deimos has always been found very near the position given by Marth's ephemeris based on Hall's elements of 1877.

Phobos is some three quarters of an hour ahead of the ephemeris place.

Both are decidedly more easy to see than Mimas, which is about the *minimum visible* for my eye with this instrument. I have seen Mimas only twice this season, once on Oct. 25 at 9.40 W. M. T., when it was near its eastern elongation, nearly in line with Tethys and Dione, Enceladus at the same time being at western elongation.

The other time was on Nov. 10 at 9.30, when it was at eastern elongation again. On this occasion Professor Brackett saw it steadily and distinctly. I saw it myself on both occasions only by glimpses. C. A. Young.

Princeton, New Jersey, U. S. A.
Nov. 22, 1879.

Note on the b *Lines in the Solar Spectrum.*

On examining this group on October 3rd with the extreme dispersion produced by a Rutherfurd grating of 17280 lines to the inch in the spectrum of the 4th order, I found that both b_3 and b_4 are *double* lines, the distance between the components being about $\frac{1}{8}$ of one of the divisions of Angström's scale in each case, though b_3 is a little easier to divide than b_4. The line at 5207·4 on Angström's map is also double. The interest of the observation lies in the fact that all three of these lines are "basic," *i. e.*, are shown upon the map as common to the spectra of two or more substances. b_4 is given as common to the spectra of Fe and Mg, b_3 to Fe and Ni, and 5207·4 to Fe and Cr. The solar spectrum needs to be remapped with higher dispersive power than ever used hitherto.

Princeton, N. J. C. A. Young.
Nov. 20, 1879.

Utilization of Tidal Action.*

There is good reason for the hope that the day is still far distant when the supplies of coal, upon which the commercial supremacy of England has so largely depended, will begin to fail. Such a contingency is, however, one the possibility of which must be contemplated, while its ultimate certainty may be affirmed. At present we see no signs of any steady increase in the price of coal owing to any failure of its supply as compared with the demand for it; but long before the exhaustion of our coal-beds shall take place we may look forward to a greatly increased cost in its production, from its having to be raised from greater and greater depths below the surface of the earth; and under such circumstances a very moderate increase in the demand for it would have a very great effect in augmenting its selling price. We have only to look back to the price at which coal was sold some 4 or 5 years since (a price nearly double that at present existing) in order to see how soon any scarcity in its production, or the exhaustion of the seams at present most available, might raise its value.

Nor is it likely (as in the case of wheat and other edible products of the earth, which are far less bulky in proportion to their value) that imported coal could (except to a limited extent in domestic consumption) take the place of that which is at present obtained in our manufacturing districts. In fact, even within the limits of our own country, the carriage of coal so much enhances the value of the products manufactured by its agency, that it is found that the manufactures must go to the coal, and not the coal to the manufactures.

It therefore becomes important to consider what moving-power may, in course of time, be substituted for that which we at present obtain from coal; and it appears to be the duty of men of science, as far as possible, to bring down to the grasp of the practical mechanician those forces which are at present unused or very little used, but which Nature has provided with almost lavish profusion.

It may probably be the case that before very long electricity may be largely used as a prime mover. It seems that this use of it may very likely be much more extensive than its use for purposes of illumination, however successful the various forms of electric lighting may ultimately prove to be.

But it also appears that great motive power will be required for the generation of the electricity in question.

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* From a Lecture delivered at Gresham College in November 1878.

Once generated, its conduction to any particular point of application may be a much easier matter.

The utilization of the energy of the Falls of Niagara has been suggested as a means for the generation of electricity, which may thence be conducted so as to work every machine in the United States and Canada.

So in like manner the machinery of South America may be supplied from the great falls (little known, but well deserving to be considered rivals of Niagara) on the Potaro in British Guiana, which are said to be 822 feet in height and 369 in breadth, with a depth of water varying from 15 feet in the dry season, to 20 feet in the wet season.

In England we have none of these vast supplies of energy; but we have, in common with other countries, a supply the importance of which can hardly be over-estimated. It may well form a part of the office of the astronomer, and of the subject-matter of an astronomical lecture, to urge upon the public the value of that tidal energy which we owe partly to the Sun, but in more than double as great a degree to the Moon. Apart from the tidal movements of our rivers, we have an amount of tidal energy along the whole extent of our sea-coasts in which we may, as has been the case in connection with very different matters in the past, once again find that an insular position is superior to a continental one.

The adaptation of tidal energy to the production of mechanical power may well be left to the practical engineer. Astronomical theory will, however, take its part in enabling the hour and minute of high water, and the occurrence of exceptionally high and low tides, so far as they depend on the places of the Moon and of the Sun, and the positions of their perigee and apogee, and the inclination of the lunar orbit, to be accurately predicted. If mechanical arrangements be made for the utilization of tidal energy, the accuracy of such prediction may become increasingly important.

It will very likely be found by the employment of suitable machinery, capable of a simple reversal of action, that both the ebb and flow and the rise and fall of the tides may be continuously employed.

Electricity may doubtless be in this way generated to an almost unlimited extent, and distributed by conductors, as has been suggested in the case of such sources of power as the Falls of Niagara.

But it appears to me that a simpler and probably more practical application of the power which the Sun and Moon bring to our doors may be found in its employment for the compression of air into large reservoirs, from which it may then be distributed to great distances at very little expense beyond the first laying of the necessary pipes.

It is well known that the machines used in the great St. Gothard Tunnel are worked by air compressed by water power outside the ends of the tunnel. I have myself witnessed the working of exactly similar boring-machines in a mine in Yorkshire, by means of air compressed by a small waterfall near to the entrance of the mine.

A great advantage connected with such an employment of compressed air is that it affords a constant escape of pure fresh air from the cylinders of the machines which are worked by it. This may be of no small moment not only in mines but in many manufactories. Again, compressed air has the further advantage that it may be easily transported in suitable receptacles. We can hardly conceive that electricity generated by a waterfall, if transmitted to a railway terminus, could be carried on a locomotive from one end of a journey to another; but we can easily see that in place of the boiler of the locomotive a cylinder, or cylinders, full of compressed air sufficient for a moderate length of journey may be carried by it. It may, however, be the case that air compressed by tidal action will never supersede steam generated by the consumption of coal on our *railways*; but the day may not be far distant when it will be employed to drive the machinery of our large factories and for many domestic purposes. We may find that the Moon is a blessing to the Earth in saving from slow and painful destruction (a process which I believe is generally of about four years' duration) the large number of horses which work our tramways and omnibuses. At intervals of a few miles on every town and country road we may have posts supplying air compressed by the tidal action of the Sun and Moon, from which, by the simple turning of a tap, the cylinder of every private carriage may be replenished and the use of horses for traction be entirely dispensed with. In fact it is to be hoped that such a tap may be ere long established in every house, and the progress and support of astronomy be vastly increased, by the new interest in the Sun and Moon which will thus be generated throughout the length and breadth of our land. E. Ledger.

The Meteors of November 13–14.

The Leonids were seen in considerable numbers at several points in Indiana on the mornings of November 13th and 14th. On the 13th, at Spiceland, Henry County, Mr. William Dawson saw about 140 from midnight to daylight. At Washington, Daviess County, Prof. D. E. Hunter, commencing at $3^h 30^m$, counted 190 in two hours. On the 14th Mr. George W. Henley at Bloomington, Monroe County, saw nine in 25 minutes, commencing at 1 o'clock. These were all the meteors seen during the times specified, and the whole number, of course, could not have been Leonids; the shower, however, was distinctly marked.

It may be worthy of note that numerous meteors were seen at this epoch in the years 1846–1849*, the interval from the first date to 1879 being 33 years. It seems probable that besides the principal cluster of the stream, which passed its perihelion in 1833 and 1866–7, at least two other groups, less dense in their constitution, move in nearly the same orbit. The first of these rarer swarms, as was shown in 'Nature' for June 3, 1875, probably passed its perihelion about 1787, 1820, and 1853, and may be expected to return about 1887. The existence of the other is indicated by the showers of 1846, 1847, and 1849, together with that of 1879.

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* Quetelet's Physique du Globe, p. 310; alsc Humboldt's Cosmos, vol. iv.

Bloomington, Indiana, U. S. A. Daniel Kirkwood.
Nov. 20, 1879.

Double Stars for January.

The principal pairs have already been given (see 'Observatory, Vol. ii. pp. 241, and 304). Of the following objects those discovered by Σ and OΣ should not be neglected, while special interest attaches to those recently discovered and measured by Mr. Burnham (β).

β 526=β Persei. R.A. 3^{h} 0^{m}·3, Dec. +40° 30′.

Mag. A 2½ to 4, B 13, C 13, D 10, E 12·5.

A is the celebrated variable star Algol. Smyth observed

AB	155°·8	58″·21	1878·6	β.
AC	145 ·2	67 ·62	·6	,,
AD	192 ·4	81 ·86	·6	,,
DE	115 ·6	10 ·77	·6	,,

β 536. R.A. 3^{h} 39^{m}·1, Dec. +23° 49′.

Mag. 8, AB 9, C 8, D 12.

Very close and difficult; in the Pleiades AC is So. 437.

AB	336°·4	0″·44	1878·7	β.
AC	302 ·4	36 ·72	·7	,,
CD	11 ·2	18 ·17	·6	,,

β 547=47 Tauri. R.A. 4^{h} 7^{m}·4, Dec. +8° 58′.

Mag. A 5, B 8, C 12.

AB	359°·7	0″·89	1877·8	β.
	·7	·82	·9	De.
AB and C	223 ·1	32 ·2	·9	β.

OΣ 78. R.A. 4^{h} 8^{m}·5, Dec. +29° 42′. Mag. 7·9.
247 ·7 2 ·72 1866·9 De.

Probably binary.

Σ 520. R.A. 4^{h} 11^{m}·1, Dec. +22° 31′. Mag. 8, 8.
106°·3 0″·9 1868·1 De.

Probably binary.

OΣ 79. R.A. $4^{h}\ 13^{m}$, Dec. +16° 14′. Mag. 6·5, 8.

50°·3	0″·78	1872·2	De.

Binary.

OΣ 80. R.A. $4^{h}\ 15^{m}\cdot2$, Dec. +42° 9′. Mag. 6·5, 7.

183°·0	0″·65	1873·2	De.

Binary.

β 550=Aldebaran. R.A. $4^{h}\ 29^{m}$, Dec. 16° 16′.
Mag. 13–14.

109°·0	30″·45	1877·9	β.
110 ·5	31 ·26	8·0	Hall.

β saw this companion "very fairly with the aperture contracted to 12 inches."

For the old companion we have

35°·2	113″·94	1877·9	β.

JOSEPH GLEDHILL.

Mr. Edward Crossley's Observatory,
Bermerside, Halifax.

Meteor Notes for January *.

OBSERVATIONS may be made in the evenings during the first 18 days and in the mornings from the 7th to the 21st. During the first half of the month a shower of Cygnids should be looked for. Heis gives the position at 296° +53°, Dec. 8–Jan. 25, and the writer found it at 295° +53° on Jan. 4–17, 1877. A series of radiant-points near η Ursæ at 201° +54° was derived by Schiaparelli from Zezioli's observations in 1868–69. On Jan. 28, 1869, a very large number of meteors were seen by the latter; but they exhibited considerable confusion in their directions, and the best radiant-point was that of an ill-defined position in Corona at 236° +25° (25 meteors). Showers at 10° +57° (Jan. 10, S. and Z.), 181° +35°, 150° +67° (Jan. 6, G. and H.), 170° +8° (Jan. 5–11, T.), 132° +67° (Jan. 27, S. and Z.), 145° +5° (Jan. 4–11, T.), and 260° +45° (Jan. 14–17, D.) should also be expected this month. Fireballs are more numerous than usual on the 2nd, 21st, and 31st.

Recent Observations.—Mr. Corder observed about 350 meteors between Nov. 4–17, and a brief abstract of his results for Nov. 11–13 was given in our last month's notes. In all he noted 29 Leonids (max. Nov. 13 A.M.), of which 8 were 1st mag. and 18 left streaks. The hourly number for one observer was not higher than 4. The aggregate number of Taurids seen during the month was 53; but if several outlying radiants of similar meteors are included the total is swelled to 85. Mr. Corder remarks that, considering the length of his watches, the number seen was not large; the horary rate never exceeded 6 for one observer. The best of the minor showers of this epoch as determined by him were at 47° +23° (14), 80° +68° (12), 60° +40° (8), 134° +50° (9), 335° +68° (8), 50° +15° (8), 42° +16° (7), &c.

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* See 'Observatory,' Vol. i. p. 301, and Vol. ii. p. 305, for further notes on January showers.

At Bristol the principal shower west of the Taurids on Nov. 12–14 was at 46° +21° (19). There was also a well-defined radiant in the fore part of Ursa at 133° +70° and at ζ–β Tauri, 80° +24°. Other suspected positions were derived at 62° +34°, 59° +48°, 48° +43°, 5° +52°, 76½° +33°. The brightest meteor seen during the month was at about $7^h\ 20^m$ on Nov. 10, falling slowly from 213° +56° to 207 +32° (=25°). It was nearly as bright as Venus.

Mr. E. F. Sawyer reports from Cambridgeport, Mass.:—"The sky continued persistently cloudy from Nov. 11th to the 15th, and no opportunity was afforded of watching the Leonids until the latter date, when an observation of two hours' duration was obtained in a perfectly clear sky, from 13^h to 15^h, and 16 meteors recorded, of which 7 were undoubtedly Leonids. They were of medium brightness, short, rapid, and accompanied by streaks. The brightest meteor observed was a member of the Leonids. It was of the 1st mag. and appeared near the radiant-point at $13^h\ 50^m$, leaving a greenish streak. The other Leonids where of the 2nd and 3rd mag., while the majority of the unconformable meteors were of the 4th and 5th mag. The radiant-centre in Leo was accurately determined, from three very short and exactly noted paths near the focus, as at 151° +22°. Three other meteors, rather bright, short, and of an orange-colour, were well observed, and indicated a shower from the point 146° +1°. This agrees with a position observed by Denning at 148° +2°, Nov. 25–Dec. 21, 1876, and = comet I. 1813?, although my observation would seem to fix the appearance of this shower at an earlier date. The remaining meteors seen on Nov. 15 diverged from Ursa Major and Gemini." Mr. Sawyer adds that an Indiana newspaper received by him from Prof. Kirkwood contains the following notes on the November Leonids:—"Mr. G. W. Henley, of the junior class in the State University, counted 9 in 25^m on the morning of Nov. 14; while Prof. D. E. Hunter, assisted by several students (number not stated), at Washington, Indiana, on the morning of the 13th, from 3.30 A.M. to 5.30 A.M. (2 hours), counted 190! He remarked that the display was of an intermittent character. The meteors would appear in considerable numbers for 8 or 10 minutes and then cease entirely for 5 or 6 minutes, then appear again as before. This was frequently repeated." Though the numerous showers of this period must have contributed to the large number of meteors seen in this instance, the inference is that the Leonids returned with considerable intensity; but, in the absence of precise information with regard to the number of observers and the arrangement and results of the observations, it is impossible to understand to what extent this was the case.

A large meteor was seen on Nov. 17, $9^h\ 40^m$ G. M. T., by Dr.

Copeland, at the Dun-Echt Observatory, Aberdeen. It was brighter than Jupiter, of a bluish-white colour: and observed to explode at about the point 301° +6°.

None of the Andromedes or meteors of Biela's comet appear to have been seen at the end of November. At Writtle, Mr. Corder reports cloudy weather on the 26th and 28th. On the 27th fine from 8^h to 8^h 30^m, when during about an hour's watch only one meteor (a Taurid) was seen. At Bristol the sky was clear on the early evening of Nov. 25th, but no shooting-stars were visible in the bright moonlight; 26th cloudy; 27th occasionally fine during the night. Watched between 5^h 30^m and 6^h, and for similar intervals at about midnight and 16^h, when the sky was very clear, but there were no meteors. The evening of the 28th was overcast; but at midnight the clouds dispersed and allowed a brief observation, with, however, the same negative result as before. The last two or three nights of the month were pretty clear, without any manifestation of the expected display.

A fireball was seen at Bristol by Mr. C. P. Denning on Dec. 10, 11^h 2^m P.M. Path from $32° +18°$ to $349° +26° = 41°$. The track crosses β Arietis and nearly reaches a point slightly below β Pegasi. Though not very brilliant at first, the meteor burst out with great lustre just before extinction and left a bright train lasting about 15 seconds, from which the direction of the path was carefully noted. Mr. J. L. Stothert, writing from Bath, says:—"Dec. 10th, 11^h 2^m G. M. T. Large meteor appeared near Mars and passed about 2° south, in front of α Andromedæ, and shortly burst with fine flame, leaving a long track behind. Brightness perhaps = ♀ × 6." This description of the path almost coincides with the meteor's course as noted at Bristol.

On Dec. 12 the shower of Geminids was evidently very active; but at Bristol the sky was more or less enveloped in fog all the evening. At about 10 P.M. 5 rather bright meteors were seen in a few minutes.

W. F. Denning.

CORRESPONDENCE.

To the Editor of 'The Observatory.'

Appearances of Comets in 1879.

Sir,—

It may be worth while to call attention to what I believe to have been a well-marked peculiarity in the appearance of Palisa's comet (*d* 1879). For a few days both before and after perihelion passage, viz. from about Sept. 25 to Oct. 10, according to my own observations, the nucleus of this comet became much more diffused and fainter than the very trifling theoretical diminution of its brilliancy will account for; and later on, or about Oct. 15, it had recovered at once its lustre and definition. This is quite similar to the behaviour of Encke's comet when near perihelion, but, so far as I am aware, of none other. It is not to be accounted for by atmospheric circumstances, as upon Oct. 5, 10, and 15 the most satisfactory state of the sky prevailed, particularly on the first-mentioned dates. I feel confident that the phenomenon was so marked that it cannot fail to have been noticed by other observers of this comet, whose confirmation I shall be glad to have. I need scarcely add that during the whole interval the distance of the comet from the Sun varied so little that it is impracticable to explain the fact as an effect of the increased heat to which it had been subjected; and the suggested explanation of M. Valz in the case of Encke's comet is for various reasons unsatisfactory. In this state of the case it seems desirable to inquire if the spectroscopic results present any peculiar or very close analogy to those obtained by observation of Encke's comet in 1871–2.

I would further wish to take this opportunity of commenting on the very inconspicuous character of the tail of Brorsen's comet at its apparition this year. Unless my memory is greatly at fault, this feature was fairly observable in 1868; but I find that on the last occasion it is not even mentioned by any astronomer except Dr. Schmidt (Astr. Nachr. No. 2266). With the fine refractor at Orwell Park, the tail was seen very faintly on two evenings only, namely on April 11 and 24, and merely a trace of such an appendage was detected on intermediate days. As periodical comets generally tend to become fainter at each return, it may be interesting to note the stages by which their loss takes place.

I am, Sir, yours faithfully,
JOHN I. PLUMMER.

Jupiter in 1869 *and* 1879.—*The "Ellipse" and the "Red Spot."*

SIR,—

The "red spot" which has for some time been visible on the disk of Jupiter is attracting so much attention that it has occurred to me to extract from the note-books some account of both it and the "ellipse" or "elliptic ring" which was so much observed in the southern hemisphere of the planet in 1869. The drawings which accompany these descriptions will, I hope, add somewhat to their interest and usefulness.

The "Ellipse" of 1869.—All through the autumn and winter of this year the physical features of the planet Jupiter were carefully watched and sketched. A great variety of cloudlets, dark spots, bright spots, "breaks" or "gaps" in dark belts &c. were observed; but by far the most interesting feature was the "elliptic ring." This was first seen on the 14th of Nov., and it was central about $10^h\ 50^m$ G.M.T. "It lies parallel to the belts and touches a belt to the south: it is darkest at the E. end." In the drawing (p. 281) the ellipse is too short.

On the 5th of January, 1870, it was seen by Prof. Mayer of Lehigh University. It was carefully observed by me till March 1870, and in July of the same year it had disappeared.

The following transits of the centre across the polar diameter of Jupiter were made here; they were merely eye estimations, the micrometer not being used:—

	h.	m.*		h.	m.	
1869, Nov. 14..	10	50	1869, Dec. 28..	7±		
16..	12	0	1870, Jan. 4..	8	0	E. end in centre.
19..	9	45	9..	7	0	" "
28..	12	15	23..	8	20	
30..	13	45	25..	10	0	
Dec. 1..	9	40	26..	5	45	
4..	7	15	Feb. 9..	7	30	
13..	9	30	26..	6	20	
27..	11	0				

Although this beautiful form was examined so often no change was detected, with the exception of a slight thickening of the line in Feb. 1870. On Dec. 1st, 1871, a similar elliptic ring was seen resting on the S. equatorial dark band.

The "Red Spot" of 1879.—The drawing was made about 11 P.M. on Sept. 29th. The definition was not good, and the position, form, and colour of the small spots preceding the "red spot" are consequently uncertain. From measures made on Aug. 31st, Nov. 7th, 12th, and 14th, the length would seem to have diminished; but much uncertainty attaches to this, owing to the bad state of the air when the measures were made.

The following transits of the "red spot" over the polar diameter of Jupiter have been observed. The method was as follows:—The equatoreal diameter was measured and the micrometer-webs set half that distance apart: one web was then kept on the eastern limb of the planet until the preceding end, centre, and following end of the "red spot" had come up to the other web.

	Preceding end.		Centre.		Following end.	
	h.	m.*	h.	m.	h.	m.
1879, Nov. 7	7	13±	7	44±	8	13±
12	6	20±	6	50±	7	20±
14	7	59±	8	25±	9	6±
29	5	15±	5	50±	6	30±
Dec. 1	7	3±	7	35±		
3	8	45±	9	10±		
6	6	5±	6	45±	7	15±
8	7	41±	8	21±	8	46±

I am, Sir, yours truly,
JOSEPH GLEDHILL.

Mr. Edward Crossley's Observatory,
Bermerside, Halifax.

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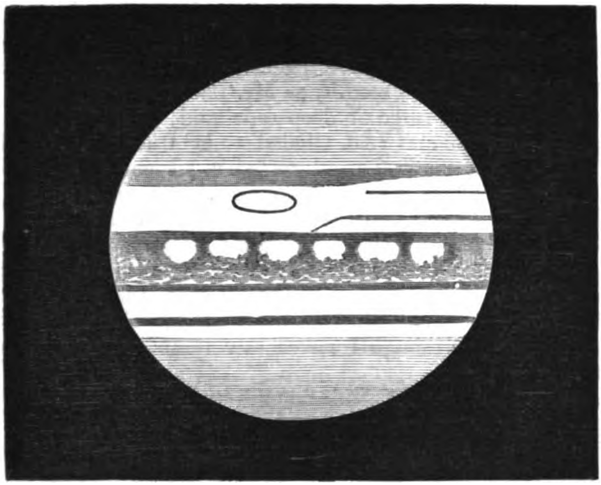
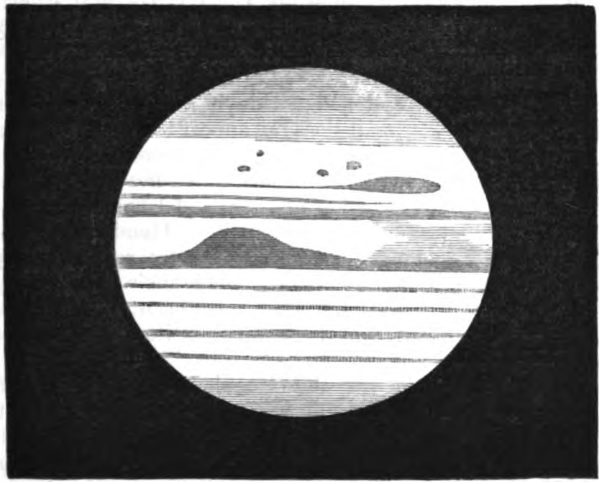
* G. M. T.

JUPITER.



1870, January 23, about 8ʰ 20ᵐ G. M. T.



1879, September 29, about 11ʰ 30ᵐ G. M. T.

The great Patch on Jupiter.

SIR,—

The valuable article by Mr. Brett in your last Number comes at an opportune moment. The great patch on Jupiter, which your correspondent rightly describes as "a most extraordinary phenomenon," deserves not only to be well observed, but, what is more important, correctly described. In all my observations of this object I have always seen it, as Mr. Backhouse has, as a "long dusky mark," with a tinge of deep red; and the non-visibility of the bright "rose colours" &c. alluded to by other observers I set down as due to the limited aperture (4½ in.) I employed. It appears now, however, that the object was never so bright as it was said to be by some, and that the "flames" we were so eagerly looking for had no real existence. Mr. Brett only expresses the feeling of humbler observers when he depreciates such extravagant vagaries in observational science. At the same time it is only fair to say that early last autumn the object was certainly more highly coloured than now, though I never saw it at any time brighter than a dull brick-red.

Mr. Brett alludes to Cassini's famous spot of 1665, and inquires "whether any other correspondences can be shown." There have been, during recent oppositions, some phenomena of a character allied to the present which deserve notice. Mr. Denning in 1868 records on the S. belt a dark spot of elliptical form. This may have been the precursor of the great southern ellipse first observed by Mr. Gledhill on Nov. 14th, 1869. This object was stated to be as bright in the interior as the zone on which it lay, and showed no change in contour. The proportions of this ellipse were 1 to 1·51 according to measures by Prof. Mayer. In the succeeding opposition of Dec. 1871 Gledhill again observed this object. It was then much less regular in outline and had within it a short slightly curved dark line. From this time it does not appear to have been observed, though Birmingham three months later records a large dark marking hereabouts. The present dark patch lies in the same latitude as the great ellipse of 1869 to 1872, and I have little doubt is allied to it. Both present the same shape and tendency to elongate in the direction of the longer axis. A still more remarkable parallelism lies in the circumstance that both made their appearance about the time when the sun-spot minimum was terminating. Whether the present marking will, like the last, retain its depth and position till the time of maximum can only be decided by time.

The belt system as a whole shows a singular alternation of activity. From 1860 to 1863 there was a general tendency of the S. belts to increase in size and activity, the N. continuing smaller and much less given to change. From 1863 to 1867 the S. belts lost size and tendency to alteration in form, whereas the N. became more evident and active. In 1868 the S. region resumed its activity (N. E. Green). In 1869–70 the S. region was still active, but in 1871–72 the N. region was the seat of change (Gledhill). In 1874–75 I found the N. belts still disturbed; but since then activity has been transferred to the S., where it still remains. These facts may be worth noting at the present time.

I am, Sir, yours faithfully,

Hoxton Street, N., Dec. 8, 1879. **Albert P. Holden.**

Spot on Jupiter in 1792.

Sir,—

In connexion with the much-talked-of spot that recently showed itself on Jupiter, attention may be called to an observation of something very similar observed by Schroeter in the last century. This was on the occasion of the occultation of the planet by the Moon on April 7, 1792. The equatoreal belt is represented as being very distinct, consisting of two zones of a brownish-grey colour separated by a more luminous interval. Two well-defined stripes, which Schroeter had noticed for years, crossed the disk. The most remarkable phenomena, however, were two nebulous undefined spots, perceptibly darker than the principal belt; and, above all, a more remarkable spot is represented in the southern hemisphere of the planet, circular, bright in the centre and with a dusky shading all round. A similar spot seems to have been observed in 1786 and 1787. I am, Sir, faithfully yours,

Samuel J. Johnson.

Abbenhall Rectory, Gloucester,
December 8, 1879.

The Moon's Photographic Semidiameter.

Sir,—

Mr. Neison, in the Monthly Notices' of November, has been so good as to call attention to a paper of mine on the measures of the Moon's photographic semidiameter, inserted in the same publication of June last. His paper contains several items; these shall be first stated, and then briefly discussed.

I. An empirical formula for the Moon's mean semidiameter, as viewed through a telescope of any given aperture.

This formula will be valuable and interesting if shown to be correct; but I fear reasons exist why any such general formula must be received with great hesitation.

II. An application of this formula to the De la Rue reflector of 13 inches aperture.

This will be shown to be a probable misapprehension.

III. A remark that Bessel's method of measuring the Moon, and adopted by myself for a specific purpose, is inapplicable on account of irregularities of the Moon's limb.

This I believe to be groundless.

IV. A remark that if the measures of the Lunar photographs had been reduced by Burckhardt's tables, the discordances would be very seriously increased.

I here venture to observe that Mr. Neison cannot possibly be in possession of data whereon to found any computations justifying this remark. I have never yet stated the dates of the photographs; and these dates form a very essential element in the computation of the parallaxes.

V. A remark that Wichmann's heliometer measures would be as accordant, *inter se*, as are the photographic measures, provided Burckhardt's lunar parallaxes were corrected.

Mr. Neison, no doubt, has founded this assertion on computations which he has already made. Pending their production, I have grave reasons for doubting the removal of the discordances in question by this process.

VI. A reference to certain experiments whereby he has determined the amount of "*Photographic Irradiation*" in the case of the Oxford Lunar Photographs to be about 0″·3.

A detail of these experiments would be both valuable and instructive. Assuredly they must have been very difficult.

Further, as to the empirical formula for the Moon's semidiameter obtained from telescopes of specific apertures. In the 'Monthly Notices' this formula is evidently printed with errors. Correcting to the best of my power, it would stand thus:—

15′ 33″·37 + 4″·1 ÷ (1 + 0·7 aperture in inches).

Now on examination of the Greenwich and Oxford (Radcliffe) Observations, I find that, in a single year, such discordances as 6″, 8″, and even 10″ occur in the resulting values of the Moon's observed diameter. This indicates the existence of serious "*personality*," and very great difficulty in placing a wire on the Moon's limb. I have myself observed the same difficulty in regard to lunar photography, and I at once abandoned the method for the more secure one suggested by Bessel. In obtaining an empirical formula, representing the results with all telescopic apertures, these personalities ought to be eliminated. It may be that Mr. Neison has done so; still the great discordances of single observations must always throw a doubt upon the concluded measures.

Again, the Astronomer Royal, in discussing the Moon's semidiameter as resulting from twelve years' observations, arrives at 15′ 33″·65 (Introduction, 1863), differing by 0″·18 from Hansen's adopted value; this of itself shows the very precarious character of all such determinations. Further still, in the 'Astronomische Nachrichten,' No. 2287, for Nov. 29 of the current year, Prof. Winnecke stated that, from observations made at Strassburg with a 5-inch transit-circle, Hansen's value of the Moon's semidiameter ought to be reduced by 1″·09. If this be so, then either Mr. Neison's formula becomes invalidated, or the whole question is at present insoluble.

II. Mr. Neison, in applying his formula to the De la Rue *reflector* of 13 inches aperture, has omitted to notice the well-known diffraction effect of the small mirror in all such instruments.

III. In measuring the Oxford (University) photographs, I adopted Bessel's method, devised expressly for the avoidance of irregularities in the Moon's serrated limb. I select places where these irregularities are either non-existent or very small. The effect of this peculiarity of the method is seen, I believe, in the remarkable accordances of the photographic measures. If, notwithstanding this precaution, the measures are vitiated, as Mr. Neison suggests, what shall we say of the results obtained from transit-circle observations, adopted by himself, where no such power of selection exists, and where the discrepancies are so unavoidedly great?

I will only add that I inadvertently deduced the 'Nautical Almanack' value of the Moon's mean semidiameter by the employment of Professor Adams's mean parallax of the Moon. I should, with greater consistency, have used Hansen's, though probably the less correct of the two. But this has nothing whatever to do with the questions raised in this paper. These are real questions, possessing an interest of their own; and this is the only excuse I can plead for occupying so much space in the valuable pages of the 'Observatory.' I feel obliged to Mr. Neison for calling attention to the subject.

I am, Sir, yours faithfully,
C. PRITCHARD.

Oxford University Observatory,
Dec. 18, 1879.

The Royal Astronomical Society.

SIR,—

The Fellows of the Astronomical Society who were present at the November Meeting, and others at a distance, who, like myself, heard only of what took place on that occasion, must have felt a deep regret at the incipient re-occurrence of an outbreak of irregularities, the like of which recently brought the Society to the very verge of disaster.

If Captain Noble made use of the language imputed to him, viz. "*pot-house abuse,*" at a scientific meeting of the Astronomical Society, no matter what provocation he had met with (and he seems, from the report in the 'Observatory,' to have met with a considerable amount), then, no doubt, Captain Noble will offer an apology to the society for the unbecoming expressions which he is reported to have adopted. On the other hand, it may be that the gentleman who is reported to have provoked him by disrespectful expressions will gladly withdraw language which he may now regret.

But the astronomer who occupies the chair at these meetings has the power of repressing all such improper ebullitions of temper or of personality; and perhaps it may not be too much to hope that even the very amiable nobleman who now presides over these astronomical debates will so far overcome his well-known kindliness of disposition as to put a stop to language which may lead to consequences far from advantageous to the Astronomical Society.

The Astronomical Society is established for the furtherance of astronomical research, and not for the indulgence of personal spleen, nor for the gratification of the appetite for notoriety.

I am, yours faithfully,

Oxford, Dec. 15, 1879. C. PRITCHARD.

NOTES.

THE LICK OBSERVATORY.—We learn from an article in the 'San Francisco Chronicle' that Mount Hamilton, the site selected by Mr. Lick in 1875 for his proposed Observatory, has been recently visited by Mr. Burnham, who, after spending a month there, pronounces it one of the best astronomical sites in the United States, a judgment in which he is confirmed by Prof. Newcomb. Mr. Burnham erected his six-inch equatoreal in a temporary observatory on the summit of the mountain, and during his stay there discovered about twenty new double stars. After fixing on the site, which is some 50 miles south-east of San Francisco, Mr. Lick forthwith constructed a road 20 miles long to the top of the mountain and obtained some 1500 acres of land for the observatory; and there the matter has rested until further information could be obtained as to the largest size of refractor practicable. The Lick Trustees seem disposed to wait till the success of the 30-inch object-glass ordered by Mr. Struve for the Pulkowa Observatory has shown that the limit of size has not yet been reached. But meanwhile, in order to properly observe the transit of Venus in 1882, they propose to secure at once a twelve-inch refractor, which will become a permanent fixture of the observatory. As regards the buildings, we are told that, besides the observatory and residences for the observers, they will include a large hotel for the accommodation of the general public. Mr. Lick, who died two years ago, has left 700,000 dollars for the erection and endowment of the proposed observatory as an adjunct of the University of California, so that ample funds are provided not only for purchasing the largest instruments which can be made, but also for securing a permanent establishment.

PARALLAX OF THE STAR OELTZEN'S ARGELANDER 11677†.[*]—The large proper motion of this star ($-0^{\mathrm{s}}{\cdot}507$ in R.A. and $+0''{\cdot}21$ in Dec.) has led M. Geelmuyden to undertake at Christiania, with a seven-inch Merz refractor, a series of micrometer measures of its distance from a neighbouring star of 9·5 mag. $91^{\mathrm{s}}\,p$ and $7''\,n$, with the object of determining its parallax. The observations were continued from 1878, Sept. 4, to 1879, Oct 14. Putting μ for the relative proper motion and π for the relative parallax, 222 observations of difference of R.A. give $\mu=-0^{\mathrm{s}}{\cdot}424\pm0^{\mathrm{s}}{\cdot}022$, $\pi=0''{\cdot}423\pm0''{\cdot}078$; and 206 observations of difference of Declination give

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[*] Astronomische Nachrichten, No 2287.

$\mu = -0''\cdot137 \pm 0''\cdot087$, $\pi = 0''\cdot272 \pm 0''\cdot038$. The circumstances that the relative proper motion enters as an unknown into the equations, greatly diminishes the accuracy of the determination of the parallax, and M. Geelmuyden has therefore tried the effect of solving the equations on the assumption that the relative proper motion is the same as the absolute proper motion given above. On this hypothesis the observations in R.A. give $\pi = 0''\cdot270 \pm 0''\cdot075$, and those in declination $\pi = 0''\cdot242 \pm 0''\cdot043$. It would seem from this that the star, which is of the ninth magnitude, has certainly a sensible parallax; and it is to be desired that other astronomers should observe it for parallax, especially after the lapse of some years, so that the relative proper motion may be determined independently of the equations for parallax.

We have received the prospectus of a 'Bibliographie Générale de l'Astronomie,' by J. C. Houzeau, Director, and A. Lancaster, Librarian of the Brussels Observatory. This valuable work gives a methodical catalogue of all astronomical works from the invention of printing to the year 1880. These are divided into—1. Books, 2. Memoirs, 3. Observations; and in each section the works are classified according to their subjects, an alphabetical list of authors and subjects being prefixed. Specimens are given of each of the sections, in which we are glad to observe that the authors have not contented themselves with giving merely the bare title, but have added a brief abstract of the contents, so that a very good idea of each work may be formed from a perusal of this Catalogue. Not only have the contents of all the astronomical periodicals been analyzed and classified, but also those of such encyclopædic works as Lalande's, Laplace's, Bessel's, Encke's, &c., which have been classified article by article. The entire work will consist of three thick octavo volumes in double column, which will be published in parts of 300 or 400 pages each. Persons who wish to subscribe for the work are requested to send their name and address without delay to MM. Houzeau and Lancaster, Observatoire Royal à Bruxelles. The subscription is fixed at the rate of three pence for each sheet of 16 pages, exclusive of postage.

A new minor planet of the tenth magnitude (No. 211) was discovered by Herr Palisa at Pola on Dec. 10. Editor.

Astronomical Memoranda, 1880, *January.*

Sun. Jan. 1, sets $3^h\ 59^m$, rises $20^h\ 8^m$; Jan. 31, sets $4^h\ 44^m$, rises $19^h\ 42^m$.

A total eclipse of the Sun, visible only in California and the North Pacific, occurs on Jan. 11.

Equation of time:—Sun *after* Clock, Jan. 1, $3^m\ 38^s$; Jan. 11, $8^m\ 2^s$; Jan. 21, $11^m\ 29^s$; Jan. 31, $13^m\ 39^s$.

Sidereal Time at Mean Noon:—Jan. 1, $18^h\ 42^m{\cdot}1$; Jan. 11, $19^h\ 21^m{\cdot}5$; Jan. 21, $20^h\ 1^m{\cdot}0$; Jan. 31, $20^h\ 40^m{\cdot}4$.

Moon.	*rises.* h	m		*sets.* h	m		*sets.* h	m
Jan. 1..	8	33	Jan. 11..	3	43	Jan. 21..	16	12
2..	9	48	12..	5	11	22..	17	9
3..	11	5	13..	6	35	23..	18	0
4..	12	25	14..	7	57	24..	18	39
5..	13	46	15 .	9	14	25..	19	9
6..	15	9	16..	10	28	26	*rises*	
7..	16	35	17..	11	40	27..	5	6
8..	17	52	18..	12	50	28..	6	21
9..	18	57	19..	13	59	29..	7	37
10..	19	46	20..	15	9	30..	8	54
						31..	10	13

Last Quarter, Jan. 4, $18^h\ 49^m$; New, Jan. 11, $10^h\ 40^m$; First Quarter, Jan. 18, $18^h\ 40^m$; Full, Jan. 26, $22^h\ 12^m$.

Mercury is a morning star, having passed his greatest elongation W. on Dec. 30. He rises $1\frac{3}{4}^h$ before the Sun at the beginning of January.

Venus is a morning star. The illuminated portion of the disk is 0·684 on Jan. 15, and 0·780 on Feb. 14, and the corresponding diameters 16″·7 and 13″·9.

	R.A.	Dec.	tr.	rises
Jan. 1,	$15^h\ 36^m{\cdot}3$,	16° 32′ S.,	$20^h\ 55^m$,	$16^h\ 19^m$
Jan. 31,	18 4 ·1,	21 50 S.,	21 25	17 20

Mars is advancing from Aries to Taurus. His diameter is 14″·6 on Jan. 1 and 10″·8 on Jan. 31, and the illuminated portion of the disk 0·908 on Jan. 15. Period of rotation $1^d{\cdot}025 = 1^d\ 0^h{\cdot}60$; Hourly rotation 14°·62; Pos.-angle of axis 142°; Lat. of centre of disk 17° S.; Spring equinox of Mars's Northern hemisphere on Jan. 21 [from Mr. Marth's ephemeris].

	R.A.	Dec.	tr.	sets
Jan. 1,	$2^h\ 46^m{\cdot}5$	17° 55′ N.,	$8^h\ 3^m$,	$15^h\ 44^m$
Jan. 31,	3 24 ·1	20 38 N.,	6 43	14 41

Jupiter is an evening star, in Aquarius.

	R.A.	Dec.	tr.	sets
Jan. 1,	$22^h\ 44^m{\cdot}7$,	9° 12′ S.,	$4^h\ 2^m$,	9 19^m
Jan. 31,	23 7 ·2,	6 50 S.,	2 27	7 56

Saturn is an evening star, in Pisces, advancing slowly.

	R.A.	Dec.	tr.	sets
Jan. 1,	$0^h\ 38^m{\cdot}2$,	1° 25′ N.,	$5^h\ 55^m$,	$12^h\ 6^m$
Jan. 31,	0 44 ·7,	2 15 N.,	4 4	10 19

	Outer Ring. Maj. Axis.	Outer Ring. Min. Axis.	Inner Ring. Maj. Axis.	Inner Ring. Min. Axis.	Ball. Diam.
Jan. 15...	39″·08	5″·48	25″·99	3″·64	15″·7
Feb. 4...	37 ·90	5 ·78	25 ·20	3 ·84	15 ·1

The south side of the rings is visible, the elevation of the Earth above their plane being 8° 4′ S. on Jan. 15 and 8° 47′ S. on Feb. 4, and of the Sun 10° 32′ S. and 10° 49′ S.

Uranus is in Leo, about 16^m *f* and 50′ *s* of ρ Leonis.

Jan. 1, R.A. $10^h\ 43^m{\cdot}1$, Dec. 8° 59′ N., tr. $15^h\ 58^m$
Jan. 31, 10 40 ·0, 9 19 N., 13 57

Neptune is stationary in Aries, in quadrature on Jan. 29.

Jan. 1, R.A. $2^h\ 30^m{\cdot}2$, Dec. 12° 54′ N., tr. $7^h\ 47^m$
Jan. 31, 2 29 ·9, 12 55 N., 5 49

Phenomena.

	G. M. T. h	m	
Jan. 1	7	47	J. i Ec. R.
3	13	49	[4006] Oc. R. 294°.
6	7	38	J. ii Ec. R.
7	7	31	J. iii Tr. E.
	20	15	[5197] Oc. R. 228°.
8	6	22	J. i Oc. D.
9	5	51	J. i Tr. E.
13	5	25	J. ii Oc. D.
16	5	10	19 Piscium Oc. D.
	5	33	J. i Tr. I.
17	5	12	J. iv Ec. D.
	6	6	J. i Ec. R.
18	5	40	J. iii Ec. R.
Jan. 20	7	4	*e* Arietis Oc. D.
	8	25	—— Oc. R. 272°.
22	6	14	J. ii Tr. E.
	15	5	[1518] Oc. D.
23	14	48	132 Tauri Oc. D.
25	6	28	J. iii Oc. R.
	6	33	J. iii Ec. D.
26	14	49	d^2 Cancri Oc. D.
28	11	15	[3407] Oc. R. 221°.
	11	29	π Leonis Oc. D.
	12	37	—— Oc. R. 212°.
29	6	11	J. ii Tr. I.
31	6	56	J. i Oc. D.

Abbreviations:—J. i, Jupiter's 1st Satellite; [4006], B. A. C. 4006.

Oc. Occultation. D. Disappearance. Tr. I. Transit Ingress.
Ec. Eclipse. R. Reappearance. Tr. E. Transit Egress.

The angles are reckoned from the *apparent* N. point towards the right for the inverted image.

Editor.

Satellites of Saturn.

Mr. W. E. Cooper, The Mount, Worcester, has prepared a sheet of diagrams, based on Mr. Marth's ephemeris, showing the positions of the five inner satellites at 8^h G. M. T. for the month of January. According to Mr. Common's observations ['Observatory,' No. 33, p. 263] Mimas is from $1\frac{1}{2}^h$ to 2^h in advance of the positions here given.

Publications received:—Agnes Giberne, *Sun, Moon, and Stars*, with a preface by Prof. Pritchard (Seeley, Jackson, and Halliday, price 5*s*.)—A. d'Abbadie, *Discours prononcé au nom du Bureau des Longitudes* (Firmin-Didot, Paris)—Otto Struve, *Tabulæ Quantitatum Besselianarum* 1880–1884 (St. Petersburg, 1879)—O. Backlund, *Erscheinung des Encke'schen Cometen in* 1878 (Acad. des Sciences St. Petersburg, 1879, April).

Editor.

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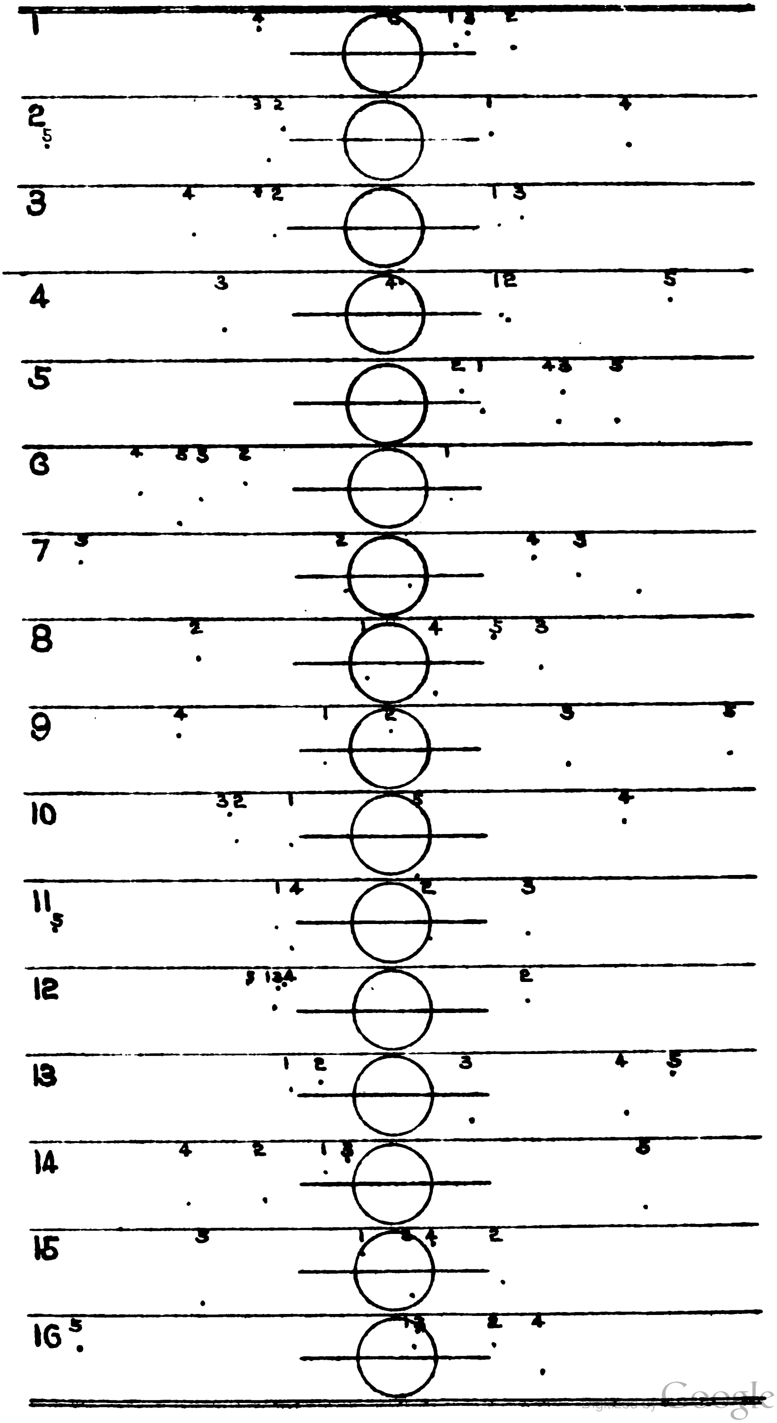
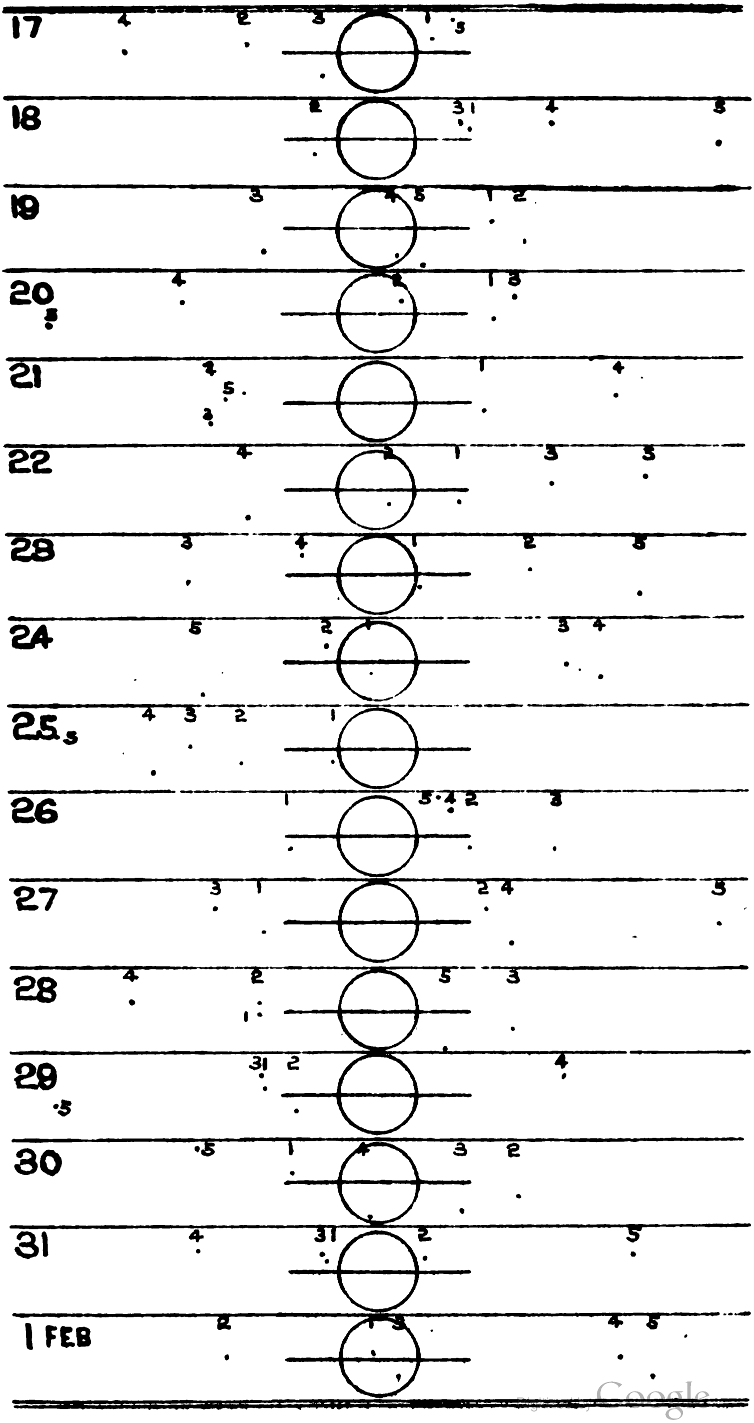


SATURN. INVERTED. JANUARY 8 HOURS G.M.T



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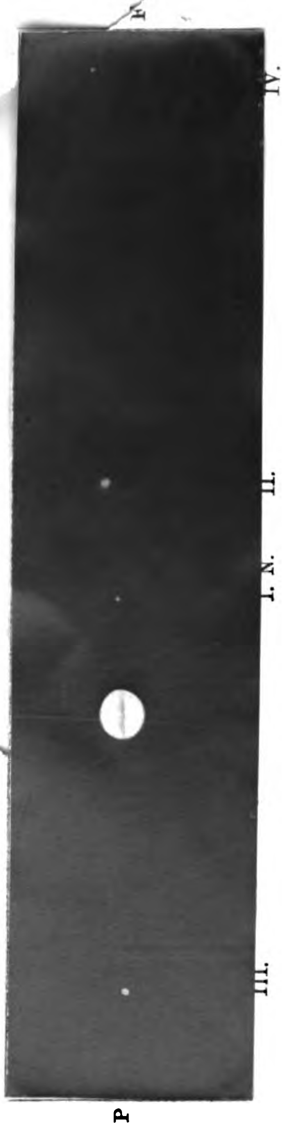

Jupiter and four Satellites, 1879, Sept. 8, 11^h. Exposure for Jupiter $1\frac{1}{2}^s$, for Satellites 17^s. Enlarged about $5\frac{1}{2}$ diams.

The Red Spot on Jupiter, 1879, Sept. 3, 9^h. Exposure $1\frac{1}{2}^s$. Enlarged about 4 diams.

Taken with a 36-inch Reflector.

A. A. Common.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

No. 34. FEBRUARY 1. 1880.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, 1880, January 9.

LORD LINDSAY, M.P., F.R.S., &c., *President*, in the Chair.

Secretaries: Mr. J. W. L. GLAISHER, M.A., F.R.S., and
Mr. A. COWPER RANYARD, M.A.

MR. GLAISHER reported that 42 presents had been received, including a copy of the 'Uranometria Argentina' from Professor Gould.

Capt. Noble. I am extremely unwilling to intrude upon the time of the Society with a purely personal matter; but my excuse for doing so must be found in the fact that it is a matter which, to a certain extent, affects the conduct of the Society itself. I am afraid some of those present at the Meeting in November will remember that in a moment of irritation I was betrayed into the use of the phrase "pot-house abuse," with reference to a certain interruption. The discussions of this Society are reported in two or three Astronomical journals, and, among others, in a publication called the 'Observatory.' Mr. Christie sent me a proof of my remarks at the November meeting for revision in this paper, and through this most objectionable phrase I ran my pen, with a marginal request or note that it was to be struck out; but for some reason (which, no doubt, Mr. Christie will be perfectly able to explain*) I suppose he printed it. I do not know the paper; I never see it; and therefore whether it was printed or not I should not have known, but for a circumstance that has since occurred. It must have been printed, because some one's notice was directed to my objectionable form of speech, and in a very roundabout way I have heard of this. Now that I have heard of it, and now that my attention has been drawn to it, I have no hesitation whatever in saying that it was a form of expression which no amount of provocation would justify. (Applause.) And believing that, I most unhesitatingly withdraw it, and express regret that I should have been irritated into using so very unparliamentary a form of expression. (Hear, hear.)

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* [We think it desirable that a correct report of the scenes which have more than once taken place at our Meetings should be given to the Fellows generally, in order that public opinion may operate as a check on such unseemly language. We are glad to see that our action has called forth a tardy apology; and this must be our justification to the Astronomical world for "washing our dirty linen in public."—ED.]

Mr. Ranyard read a paper by *Mr. Pratt* on the "Rotation period of Jupiter."

The President. I think the Society will pardon me if I tell them that I have taken the liberty of asking Dr. Huggins to communicate to us this evening some of the results which he has already, unfortunately, sent to another Society; and therefore it can only be by the invitation of the members present that he can bring them forward to us this evening. (Hear, hear.) I feel certain you will be interested.

Dr. Huggins. My Lord President, I have great pleasure in giving a short account of some results which I have obtained during the last four years on the ultra-violet portion of the spectra of stars. I will state first in a few words the general form of apparatus which has been employed. It will be obvious that it is not very easy to obtain photographs of the spectra of stars, because the light of a star when spread out by dispersion to form a spectrum becomes extremely faint, especially if a narrow slit is used for the purpose of accurately noting the position of the lines afterwards, by comparison with the solar or a terrestrial spectrum. Another circumstance (as even those who are in the habit of using the best clocks will admit) increases this difficulty, namely, that it is not an easy matter to keep the image of a star upon a slit so narrow as the 1-350th part of an inch for one hour together. It was therefore of great importance to determine the form of spectrum-apparatus which should give a minimum spreading out of the light of the star compatible with a very detailed spectrum, a spectrum showing distinctly the finer lines. After trying several different arrangements, I adopted a form of spectrum-apparatus which consists of one prism of Iceland spar (I need not say it is desirable not to employ any glass) which is very transparent to the ultra-violet rays, very nearly as much so as quartz, but possessing a much greater dispersive power, so that a prism of Iceland spar of 60° is equal to a prism of dense flint glass of the same angle. The lenses were of quartz; and the spectrum-apparatus was provided with a slit of the width of 1-350th of an inch. With this apparatus the length of the portion of the spectrum between G (the commencement of this map) and O in the ultra-violet is half an inch; yet in this small spectrum, by means of a magnifying-glass, at least seven lines can be seen between the lines H and K. I mention this to show that the strong lines in the photographs do not arise from any want of defining-power of the apparatus. This spectroscope was placed at the principal focus of the 18-inch mirror of the Cassegrain telescope belonging to the Royal Society, at present in my hands. The small mirror was removed and the spectroscope placed so that the slit should be at the principal focus of the speculum. There was an arrangement by which the slit should be brought precisely to the focal point, which was found to be essential to good definition, and also by which the axis of the collimator could be placed in the optical axis of the mirror. Then came the difficulty of bringing the image of the star precisely upon any part of the slit, and also of maintaining it there, and not allowing it to go off on one side or on the other during the whole time of exposure. This difficulty I overcame by a simple but efficient device, which consists essentially in fixing in front of the slit a silver plate highly polished, with a small longitudinal opening rather larger than the slit itself, and also placing in the opening of the large mirror a small Galilean telescope. Artificial light, after passing through yellow glass, was thrown upon the silver plate. In looking through the small Galilean telescope you see the silver plate and the slit distinctly, and also the image of the star as a bright point; and therefore by means of motions of the equatoreal this bright point can be brought exactly on any part of the slit, and by supervision during the whole time of the exposure can be accurately kept there. Different photographic methods were employed, but ultimately the extreme sensitiveness of gelatine plates led to the adoption of that form of preparation. The photographs were afterwards examined under a low-power microscope to which a micrometer was attached, giving the means of measuring with accuracy to the 100th part of the distance between H and K. The wave-lengths of the lines were determined by the graphic method, making use of Cornu's map of the ultra-violet part of the solar spectrum, and Mascart's wave-lengths of the lines of cadmium.

[By the courtesy of the proprietors of 'Nature' we are enabled to give on Plate IV. Dr. Huggins's diagram showing the solar spectrum and spectra of stars from his photographs, and below Cornu's map of the Ultra-violet.]

When you look at the spectra of the stars which may be considered as the most typical of white stars, you see at a glance the peculiar rhythmical arrangement in the grouping of the 12 strong lines which essentially make up this part of their spectrum. The distance between the lines decreases as the refrangibility increases, suggesting that this group of lines indicates some one substance. The first of these lines coincides with the line of hydrogen near G, the second line with the line *h* of the solar spectrum—this line is also a line of the hydrogen spectrum—and the third line with H. The line K, which is equally broad in the solar spectrum, is either absent or represented by a fine line, which can only just be perceived in the photograph. Then come nine strong lines, which do not coincide with any strong lines in the solar spectrum. The spectrum does not end with those lines, but may be traced on the plates, even beyond S. But there are no more lines; they seem to stop suddenly. These lines appear to belong to one substance; and two of them at least belong to hydrogen. There is a line in the photographic spectrum of hydrogen at H. Mr. Lockyer has pointed out its coincidence with H, and it is in the maps of Mr. Capron; and I have it also in my photographs of the hydrogen spectrum. I have been able to trace in some photographs of the hydrogen spectrum fine lines near the positions of some of the other lines; and I think, therefore, there is a very considerable probability that these lines do represent either hydrogen as we know it or in some other condition.

The two lines H and K coincide with two strong lines in the spectrum of calcium. It was therefore extremely interesting to see if a second pair of calcium lines, which are more refrangible, coincide in position with any of the strong lines in this class of stars. They are near two lines, but do not coincide with the stellar lines. This circumstance diminishes the probability that the whole group of lines belongs to the spectrum of calcium.

The other stars, also of the white class—η Ursæ Majoris, α Virginis, α Aquilæ, and α Cygni—I have arranged in the order in which they appear to differ from what one may regard as the typical spectrum. You will see that though the same lines are present in all the stars of this class, they become narrower and more defined at the edge—at the same time other lines come in. The second line, corresponding to K, becomes stronger than in the typical stars, showing an approach towards the solar type. In the spectrum below we have that of Arcturus, which resembles the solar type; but if respect is had to the relative thicknesses of the lines H and K, this spectrum may be considered to be on the other side of the solar type in the order of change from this typical form, the line K being broader than it is in the solar spectrum relatively to the thickness of H. It seems by no means impossible that these various changes, which are presented in this map, may indeed represent the order of the changes in time through which any sun passes. It is by no means an impossible case that Vega and Sirius, being two of the whitest stars, may be in the earliest condition; while Arcturus, being yellower than the Sun, may probably be on the other side of our Sun in the order of departure from the type of Vega. I have here a small photograph, taken directly by superposition from one of the negatives (which can be handed round), of the two stars η Ursæ Majoris and Vega.

I omitted to state that the slit has two shutters; so that after the spectrum of a star has been photographed on the plate, one shutter can be closed and the other opened, and a second spectrum taken on the same plate, so as to serve as a comparison-spectrum. This may be either the solar spectrum or a spectrum taken from the Moon or the spectrum of a known star, or the apparatus can be removed and a terrestrial spectrum can be taken. With the same apparatus spectra of the planets were taken. The plan adopted was that which I employed in 1864 with the naked eye, viz. to observe the spectrum before it was dark; so that with a long slit upon the photographic plate you have a broad spectrum giving the spectrum of the sky, and a narrow and more intense spectrum which is due to the light of the planet. In this way any difference between the light of the planet and that of the sky would be at once perceived. I have obtained in this way the spectra of Venus, Mars, and Jupiter; but there is no indication whatever of any difference or of any modification of the solar spectrum in the region from G to O.

With my apparatus I have also taken spectra of small portions of the Lunar surface, but hitherto without any indication of a lunar atmosphere. This apparatus, by enabling me to take a very small area or restricted portion of a heavenly body, is specially suitable for taking photographs of the spectra of different portions of sun-spots. I hope shortly to apply my method in this direction. I think it may also be possible to get lines in the ultra-violet portion of the spectrum of gaseous nebulæ. (Applause.)

Mr. Christie. I think there cannot be two opinions as to the importance of Dr. Huggins's work on the ultra-violet part of the spectra of stars. I must confess myself, when he first mentioned to me that he was taking up the question of photographing the spectra of stars, I hardly expected that he would get much in the way of important results, on account of the difficulties in obtaining any thing by photography. But now I have great pleasure in recognizing how much he has done. One important part of his work is, of course, that he has extended the spectrum to a part which cannot be examined with the eye; and that is a matter of very great importance. There are other points, too. He has also got permanent records of the relative intensities of the lines; and that is a matter much neglected by observers, and in which there is a great difficulty in obtaining any certainty except in a photograph, which nobody can gainsay. It is a matter of great importance thus to arrange the spectra of stars in order of merit, so to speak, and certainly a very great point gained, because, although Secchi had classed the stars in types, he did not go to the extent that Dr. Huggins has done, and did not differentiate between stars of the same type—that is, stars of the white class. Further, the very important point is brought out that there is a difference between the characteristic behaviour of the two lines supposed to be due to calcium, H and K. That is an important scientific fact that Dr. Huggins has established. There is one other point which has struck me, though Dr. Huggins has not alluded to it, and that is that in the visible part of the spectrum of α Aquilæ the F line in particular is very broad; broader than in Vega or Sirius—at any rate it is a star we have had the greatest difficulty in measuring in our attempts to determine the proper motions of the star in the line of sight, in continuation of Dr. Huggins's work—yet in this photograph of the ultra-violet it appears that the lines of this star are sharp. That is a very important point, it seems to me; and it is a matter that deserves some further discussion, that we should have the spectrum of a star belonging apparently to one type in the visible part and to another type in the ultra-violet. I should like to ask one question of Dr. Huggins as regards his diagram; and that is, whether the breadth of the H line is represented throughout in its proper proportions, and whether the H line is broader in the Sun, and in Vega, and Sirius, and Arcturus than in the other stars, particularly α Aquilæ and α Cygni.

Mr. Ranyard. I should like to ask Dr. Huggins one or two questions, in addition to the information he has given to us. I think I understood him to say he found no difference in the spectrum of different parts of Jupiter. There is such a great difference in the photographic action of the different parts of the disk of Jupiter—the belts, and so on—that one would have expected there would have been some difference. Do I understand that in the whole breadth of the spectrum of the planet there is no difference? I should like to know also how his former observations with regard to Uranus are borne out, and whether he has been able to obtain any photograph of the spectrum of Uranus bearing out the existence of bright lines. I should like further to know whether he has arrived at any new facts with regard to the H lines being connected with the groups of lines which fall within them in the Solar spectrum, or whether it appears to him that the solar H line must be considered as groups of small lines with a shading over them, and of quite a different origin from the smaller lines.

Mr. Neison. I should like to ask Dr. Huggins a question; and I should not have risen to have said any thing if Dr. Huggins had not touched upon a pet subject of mine in regard to the Moon and its atmosphere. Now Dr. Huggins said he observed the Moon, and is unable to find any indications of an atmosphere. I should like to ask whether it is not true that, supposing the Moon's atmosphere was found to be of a similar nature to our own, we should not expect to see any thing, because the effect which that atmosphere would produce on the spectrum would be masked by the similar effect produced by the much greater amount of our own atmosphere through which it must pass.

In reply to the invitation of *The President*,

Mr. Rand Capron said: I have listened with much pleasure to Dr. Huggins's remarks. I have had no experience in photographing any of the spectra of the celestial bodies; but I have had some experience, as possibly you know, in photographing the spectra of the metals and the gases. With reference to one of the questions put to Dr. Huggins—that is, as to the breadth of the H line in the spectrum—I would venture to express an opinion that it is a question of the pressure exercised upon the gas at the time. I have not, of course, at present heard Dr. Huggins's answer as to the width of the H line as shown on the diagram; but I rather assume, from what Dr. Huggins has told us, that these lines are represented on the diagram approximately as they appear upon the photographs—that is to say, they are wider in the Sun and Vega and Sirius than they are in the other stars. There is nothing else occurs to me at present, except to notice the extremely ingenious method that seems to have been adopted to keep the star-spectrum upon the slit; and also, what Dr. Huggins has rather modestly overlooked, the fact that I can speak to from experience with much more manageable objects than stars—the extreme difficulty of getting in these photographs the minutiæ of detail, which are something wonderful. I think it ought to be a matter of congratulation that Dr. Huggins has gone to the great extent that he has in his researches.

Mr. Bidder. May I suggest that the diagram, to my mind, brings out very prominently the fact that lines which have a common origin may behave very differently under different conditions? which, I suppose, however, is not a novelty. Assuming that the diagram is correct, no one can doubt for a moment that the 12 lines which are shown in the spectrum of Vega, and the corresponding lines which appear in the other spectra, are attributable to one common origin; but it is prominently brought forward by the diagram how differently certain of these lines behave under certain conditions that presumably exist in the other stars, particularly as regards the three at the most refrangible end, which entirely disappear in Sirius and almost disappear in two others and in the remainder are comparatively insignificant. I have only to express, further, a hope that the diagram may be reproduced in the 'Monthly Notices,' as I think it is quite worthy of it.

Mr. Common. Before Dr. Huggins replies, I should like to ask one question, if too many have not been already put; that is, as regards getting a certain definite width in the spectrum. Is it done by a cylindrical lens, or how? because an ordinary star's image would only give the spectrum as a line.

Dr. Huggins. The method of giving width to the slit is not by means of a cylindrical lens, which, for several reasons, I wished to avoid; but after the star has been allowed to remain upon one portion of the slit for a sufficient time to form a spectrum, it is then moved a little in declination, thus joining to the former one a second spectrum. In this way you can get any breadth you wish.

With regard to Mr. Christie's question, my intention has been to represent relatively the character of these lines; but this diagram does so in rather an exaggerated degree, being intended to be seen from a distance; but in the diagram I have sent in to the Royal Society these differences of character of the lines are given with great accuracy, the principal points being that in Vega and Sirius the first line "H" is about the same, I should say, as in the solar spectrum. When you come to η Ursæ Majoris it will be seen a little narrower and defined at the edges and still more so in α Virginis.

I think the next questions were those of Mr. Ranyard. The spectrum of Jupiter was taken when the red spot was on Jupiter, but the spot was near the limb. I have not yet been able to get a photograph of the spectrum of Uranus. With reference to Mr. Neison's question, I do not see the force of his reasoning. It is well known the difference in the effect of the earth's atmosphere on the spectrum is great between looking at the Sun at a high altitude and looking at the Sun when he is low. If you look at the Moon at high altitude, you have a very small effect from the earth's atmosphere; but if the Moon has a similar atmosphere, then the Sun's light going to the Moon's surface and back again passes twice through the Moon's atmosphere and would certainly add greatly to the effect of the earth's atmosphere, and, even though it might produce the same modifications, would, I think, certainly be perceived under these circumstances. I do not wish for a moment to say that these observations are antagonistic to a lunar atmosphere; they are simply negative as to its existence.

Mr. Ranyard reminded Dr. Huggins of his question whether the H lines in the solar spectrum were a group or not.

Dr. Huggins. I cannot form a certain opinion; but the probability would be, I think, that the line H in the solar spectrum has a number of fine lines in it accidentally, in consequence of the lines falling there, but probably not physically connected with it.

The President. I am sure the Fellows of the Society will accord their warm thanks to Dr. Huggins for his most interesting paper. (Applause.)

Mr. Christie read a paper "On the systematic errors of the Greenwich North Polar Distances from 1836 to 1878," in which the determination of the co-latitude from circumpolar stars, and the corrections to the position of the ecliptic, were discussed. The points to which attention was directed were the flexure, the refractions, and the latitude. He had come to the conclusion that a determination of refractions could only be satisfactorily made by comparison with southern observatories; and that there was no reason to assume any change in Bessel's refractions, which satisfied the Greenwich observations with as much accuracy as could be expected. Taking the observations of circumpolar stars for 1836 to 1878, divided into four periods, there was very little evidence in favour of a diminution of the refractions; and such as there was depended largely on the adoption of the new law $a+b \sin z \cos^2 z$ for the R—D correction. It appeared, however, that the evidence tended slightly in favour of the old law, $a+b \sin z$, which made the R—D correction neutralize the flexure. But for the particular years on which Mr. Stone had based his diminution of the refractions, substantially the same results were found as in his paper, though they did not appear to be entitled to any great weight. Again, the latitude deduced from observations of the Sun gave an independent determination of the refraction; and dividing the results (corrected for error of the thermometer) into three periods, there was a majority of 2 to 1 in favour of Bessel's refractions. And, finally, the comparisons of the Greenwich observations with the observations at the Cape and at Melbourne showed such decided discordances when the diminished refractions were used that it seemed almost impossible to admit them.

Another question that was discussed was the obliquity of the ecliptic. Two values of the secular variation in the obliquity of the ecliptic had been used in the 'Nautical Almanac'—Bessel's up to 1863 and Le Verrier's since. A comparison of the Greenwich series of observations from 1836 to 1878 seemed to show that Le Verrier's theoretical value was supported by the Greenwich results.

Another point was that this comparison afforded evidence as to the law for R—D, and it went in favour of the old law. A further point was that, if Le Verrier's value were adopted, Bradley's and Piazzi's observations of the obliquity must have large errors of 1″·8 and 1″·5 respectively. These were large quantities; but still the Greenwich observations seemed to show that they must be accepted as inevitable in this case.

Mr. Marth. I will only put three or four questions. The last group on the Table extends to 45° N.P.D. Do you not go beyond?

Mr. Christie. I was not discussing discrepancies beyond 80° Z.D.

Mr. Marth. At the last meeting Mr. Stone referred to refractions beyond 85° Z.D.; but as his last star has less than 84° Z.D., his investigation bears in no way upon the refractions beyond 85° Z.D. The second question:—There was a change made in the thermometer in 1858; I said that the instrument had been shifted 0°·8, and you have corrected it by 0°·6. What evidence is there for the correction?

Mr. Christie. The correction was 0°·7 in 1858, by comparisons with the standard thermometer; and I have applied 0°·6 for the period 1854–57, assuming that the error is progressive roughly from 1842.

Mr. Marth. Had the errors of the thermometer been investigated in 1842?

Mr. Christie. Not as far as I know; but the thermometer was then new, and, having been made with care, the index-error would probably then be very small.

Mr. Marth. This question was raised more than half a century ago. To get it settled, the then Board of Longitude, under Dr. Thomas Young, presented meteorological instruments to the chief observatories—Greenwich, Dublin, Kœnigsberg, and others. Whether the instruments were compared among themselves is not known. I should like to know what is the history of the Greenwich barometers and thermometers used in the circle-room from that time (1825) to 1858. Could you not supply it? You see it is a very serious question, because it affects the Greenwich observations of that time. I mentioned last year that the difference between the refractions from Bradley's observations and those which Bessel deduced from his own observations was reduced, by taking the errors of their meteorological instruments into account, to 0°·4

Fahr. Will you be so kind as to make inquiries? My third question I will ask Mr. Stone and Mr. Christie. Have you read Bessel's own investigation, on which the refractions in the 'Tabulæ' are founded?

Mr. Stone. Oh yes.

Mr. Marth. You have? Then will Mr. Stone be so good as to enlighten me upon a single figure? His corrections are given for five groups. I should like to know how one of them—say the last, which is $+2''\cdot4$ or something of that kind—has been derived from the observations. We were told at the last meeting that the verification of the whole investigation can be done in a few hours. But who is clever enough to do it? for you must take nine volumes of the Greenwich observations, and try to find out what has been done.

Mr. Stone. I said the other day that the observations were collected from the investigations for co-latitude in the several volumes of the Greenwich observations. I had applied small corrections, depending upon the division errors that were adopted.

Mr. Marth. I beg your pardon. In your paper on nutation, in which you come to the conclusion that the co-latitude may depend on the Moon's node, you make corrections for the division-errors for Polaris, though not sufficiently, and also in the paper on refraction. And will you be kind enough to inform me what other corrections beyond those for division-error you have applied?

Mr. Stone. I think I have given you the observations you asked for, and where the results were taken.

Mr. Marth. Mr. Christie, will you be so good as to mention at Greenwich that the observations of Polaris are still corrected for the division-errors of the same lines as twenty years ago? I should have been extremely glad to have pointed this out long ago; and six years ago I made an appeal—

The President. Possibly we are getting rather out of the record.

Mr. Ranyard. Perhaps it will be more easy to follow the paper when these investigations have been printed.

Mr. Stone (to Mr. Marth). The question you asked me was, where I got my results from. From the introductions of the several volumes of the Greenwich observations. With respect to the small question about $0^\circ\cdot2$ of the thermometer, I do not care any thing about it—$0^\circ\cdot3$ or $0^\circ\cdot4$: you may take it or leave it; that has nothing whatever to do with it. We are considering a far more important question than an index-error of $0^\circ\cdot3$ or $0^\circ\cdot4$.

Mr. Marth. It is a question of $0^\circ\cdot8$.

Mr. Barrow. I rise to order. This is hardly the way in which our discussions ought to be conducted. We meet to discuss matters of general interest. If Mr. Marth wishes for any information from Mr. Stone, I have no doubt he can get it by making application by letter or otherwise; but I think that the time of the Meeting should not be taken up by matters of controversy, and that, to the great majority of the members, this discussion is a little uninteresting.

Mr. Stone. Perhaps I may be allowed to say one or two words. Curiously enough, I had in preparation a paper on this subject for this evening, but it is unfinished. As far as I understand the question, we are not very much concerned with questions of even half a degree in our thermometer. A much larger question has been broached with regard to these refractions; that is, whether Bessel's refractions will hold over a large arc of zenith-distance or not. I rather think at present that this part of the question has been a little shirked. Whether you take observations above and below the pole, or take them towards the south, when you get to 85° Z.D., Bessel's refractions with the constant of the 'Tabulæ Regiomontanæ' will not represent the observations, which must be reduced. But I am by no means certain that his theory does not satisfy the observations. So far as my result of 1867 went, the general result was to confirm Bessel's refractions. It was known that below 85° the smaller refractions were required. The general result of my investigation was that the circumpolar stars, above, as well as below, 85° Z.D. could not be well represented, unless the refraction of the 'Tabulæ' were diminished by about 0·005 × Refraction; and the consequence was that the whole of Bessel's refractions were chained moderately well together. Provided that my result were true, we should have a moderately sound foundation in Bessel's theory; and we might then attempt to determine the constant of refraction with the utmost accuracy and with all the refinement that Mr. Marth desires. My result is certainly correct so far as the materials used will allow; and it was substantially the same as that which Mr. Christie would get from the residuals he has exhibited in this room, viz., a diminution of about 0·004 or 0·005 in the refraction. I have recently examined the circumpolar observations printed in the Nine-Year Catalogue. It is stated in that Catalogue that the circumpolar observations are not represented by these reduced refractions. But in the three classes given, there is no indication of any error of the refraction. Now I have taken the trouble to go through the annual volumes of observations of 1868–1876, and formed the groups not included in the introduction. The residuals are $-0''\cdot14$, $+0''\cdot23$, $-0''\cdot04$, $+0''\cdot32$, $-0''\cdot21$. The last two groups extend to 46° 13′ and 48° 56′ respectively. So far as these observations are concerned, therefore, they are reconciled together by the diminished refractions, and this is using Bessel's refractions with simply the diminished mean refractions without any other alteration from the zenith down to the lowest star observed. So far, therefore, as the observations from 1857 to 1865 and from 1868 to 1876 are concerned, I see no reason whatever for assuming that these diminished refractions do not thoroughly represent the circumpolar observations; and if they do they are entitled to much confidence. I cannot follow the legitimacy of the method Mr. Christie has adopted of using the law $b \sin z$ for R—D in preference to the new one $b \cos^2 z \sin z$. The flexure should be and is determined very carefully by means of the opposite collimators; and having determined it, there is an end of it, and you cannot after that introduce another correction dependent on the sine. When my paper of 1867 was written, the only southern stars available were those observed by Mr. Ellery and his staff in 1863, 1864, and 1865. I compared these with the Greenwich observations. They required apparently the refraction for Greenwich to be diminished by 0·0033 × refr., not quite to the same extent as that found from the observations above and below the pole, still in the same direction. It was thought then that the Greenwich instrument was more free from systematic errors than the Melbourne one, and that the result obtained solely from the Greenwich observations should be preferred. As far as my own observations at the Cape are concerned, they do not agree so well with the recent Greenwich observations with the diminished refraction as with the larger value, and would be inconsistent with the smaller value, unless there are considerable systematic errors in one or other instrument.

The President. Is it your pleasure to record your thanks to Mr. Christie for his paper? (Applause.)

Mr. Dunkin read a paper on the effects of personality in the tabular errors of the Moon, in reply to two points in Mr. Neison's paper on the same subject, published in the last No. of the 'Monthly Notices.' The first related to an evident misconception by Mr. Neison of the meaning intended to be conveyed in a paragraph contained in a former paper by Mr. Dunkin, and the second to the probable insufficiency of the data employed in Mr. Neison's comparison of the relative personal equations of the Greenwich observers, with regard to the tabular errors of the Moon as exhibited in the volumes of Greenwich observations. As a test of the accuracy of Mr. Neison's values, Mr. Dunkin has compared the errors of longitude determined from observations by Mr. Lynn, Mr. Downing, and Mr. Thackeray, made in 1877 and 1878, with corresponding errors determined from Mr. Criswick's observations. The result, given in a table, scarcely bears out Mr. Neison's theory that the positive personal equations of the new observers have had a considerable influence in producing the comparatively large positive errors of the Moon noticed in recent years; and Mr. Dunkin therefore considers it unsafe to found any particular theory, when based on such small quantities subject to considerable fluctuations from year to year.

Mr. Neison said he would furnish a short paper at a future meeting in refutation of Mr. Dunkin's views.

Mr. Marth made an appeal to the Fellows to observe the brightness of Mars towards the end of February.

Capt. Noble complained that a paper by Mr. Ellery announced at the last meeting had been burked, and that the evening had been wasted over one or two papers and in personal squabbles as to Observatory details. He remarked that mathematical papers were better read than heard.

Mr. Ranyard reminded Capt. Noble that the discussion had been a very important one, and must be interesting at least to the gentlemen at the Observatories.

The following papers were announced:—

C. Todd. Observations of the Phenomena of Jupiter's Satellites &c. at the Observatory, Adelaide.

W. F. Denning. Meteor-showers.

Astronomer Royal. Observations of Occultations of Stars by the Moon and Phenomena of Jupiter's Satellites made at the Royal Observatory, Greenwich.

H. Corder. Meteor-showers 1870–79.

C. Pritchard. Note on the re-measurement of a Lunar Photograph, in reply to Mr. Neison's criticism.

C. E. Burton. Changes of Relative Brightness of Jupiter's Satellites.

T. W. Backhouse. Estimations of the time of transits of the red mark on Jupiter across the Central Meridian.

Astronomer Royal. Observations of the Outer Satellite of Mars made at the Royal Observatory, Greenwich.

A. M. W. Downing. The Greenwich Standard Right Ascensions.

N. E. Green. On some changes in the Markings of Mars.

The following were duly elected Fellows:—E. A. J. Crossley, W. S. Franks, Arthur Riches, J. S. Sloman, Rev. William Smith, Don Rafael Roig y Torres.

The Meeting adjourned at ten o'clock.

The Great Detonating Meteor of 1879, *February* 24*.
Part I.

On 1879, February 24, a magnificent meteor startled the few people abroad about 3^h A.M. in East Yorkshire and Durham. The exact time, as given by several independent observers at York, was 2.53 A.M. by the Minster, which keeps, pretty approximately, Greenwich time. The appearance was most startling, and not less so at Stockton. There a blinding light, intensified by the glare caused by the heavy snow, was accompanied by shaking of shutters and rumbling as of waggons underground. This should not be forgotten in determining the meteor's real course, which all other reports indicate to have been some distance away. The easiest explanation will probably be found in the length of the last and middle parts of the course, presented laterally to that town.

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* Paragraphs and notes in brackets are by Prof. A. S. Herschel, the chief additions being the final deductions, which the writer had omitted, not considering that his deductions, if uncorroborated, warranted the extra labour. The calculations have all been verified by Prof. A. S. Herschel, whilst one or two passages are slightly modified and occasional additions made. Hence, as it now stands, the paper to a certain extent is a joint production.

In all, there were five good observations at York of the meteor itself, three being by policemen, whilst valuable particulars of the time-interval and of the peculiar character of the sound of the explosion have been received besides.

The *duration* or whole time of visibility of the glare of light which proceeded from the meteor is spoken of as "perhaps 2 sec." (James Backhouse); as "giving time to look up and down the street for the cause and then seeing it overhead, descending thence quite low" (Messrs. Savage and Hughes, engaged in hauling manure); as "lasting ½ min. to a min." (Serg. Todd), 3 or 4 secs. (Mr. Wade); "I looked all around, then across a field, then saw it overhead" (Serg. Ridley). Perhaps the entire space of time during which the light of the meteor must be considered to have been visible, from its earliest outbreak to the moment of extinction, may be taken from these accounts to have been about six or eight seconds. [At a full allowance this would give ample leisure time for all the actions said to have been executed by observers at York in looking for and following the light. The brief intervals of 1–2 and 3–4 seconds, said by two other observers there (who were indoors) to have been occupied by the light, confirm this. Captain Heselton's observation that it was visible "one second behind clouds, like the rising moon, and then 10 seconds moving across the sky," at Whitby, relates pretty certainly, as will be noticed further on, to a much earlier view of the meteor than what the York and Hull observers, who were attracted by the light *round* them, connected with the positions (used in the following projections) which they gave of the meteor's apparent "point of first appearance."]

The *brilliancy* is spoken of by all as intense, "as bright in a second as a summer's day," all objects near and far being rendered visible. Serg. Todd, at York, described how the light "first lit up the housetops, then rushed down the sides, filled the street, and in a second enveloped him in a blaze of light." The street lamps seemed to disappear and it "more than rivalled the electric light at the cattle-show." The effect indoors was the same.

In *appearance* one or two accounts mention rapid variations, perhaps caused by scintillations noticed by another observer, and some a sudden burst (apparently just over York) of intensity of its light, as at Brighton, Liverpool (?), and overhead at York; but the ball is said, at York, not to have broken into fragments at last, but to have "disappeared in the air"; and it left no permanent light-streak on its track. It was "a primrose-coloured flame," at Hull, "lengthened out behind, where it was prismatic, the length never four times the breadth, and this was scarcely so much as half of the moon's apparent width."

The *colour*, at first clear and intensely white, all describe as becoming light blue or pale blue-green towards the end of its flight (white changing to purple at Dundee, or to a faint magenta colour at Shoreham).

In *shape* it was pear-like "like a jargonelle," the diameters in a sketch being prolonged into a tail two or three times the length of the head.

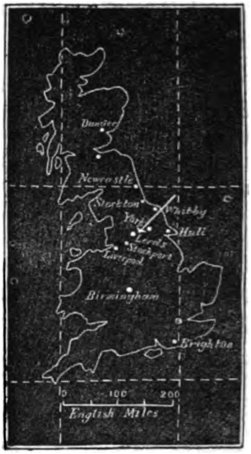
The *size* is spoken of at York as "less than the moon." Several observers kindly made circles to compare the two, giving diameters ⅛ to ½ that of the moon. The latter was given by Serg. Simpson, who at first said that it was "as big as my helmet." But the clouds then rapidly banking up for a heavy storm, whose first flakes immediately afterwards began to fall, gave it a glory four times the diameter of the moon. The observer at Stockport, with a more end-on view of the meteor, described it very similarly to this, as "a whitish ball surrounded by a kind of mist of pale colour, forming a globe of light about 18 in. in diameter."

As regards the *train*, Serg. Todd said he saw none, though looking out for it. [He thought that it "burst overhead into thousands of fragments," and that the ball, "seen just before *disappearing in the air*, did not go behind the houses."]

An *interval* then elapsed, which James Backhouse, Esq., who had been long lying awake, considered to be fully 2 or 3 minutes, or "twice as long as the longest interval between the most distant flash and thunder." In the 'Natural History Journal,' 1878, p. 127, Henry Corder speaks of hearing thunder after an interval of 100 seconds, under very exceptional circumstances. But 50 seconds, the limit given by Flammarion, and very rarely exceeded, may more properly be considered as the "longest interval." This is Prof. Herschel's decided opinion. I myself only once remember a storm giving intervals of 1½ minute. Therefore the interval according to Mr. Backhouse's report may safely be reckoned at about 1¾ minute, corresponding to 20 or 21 miles. Other reports are less definite, some speaking as if it followed almost immediately. It gave time for a good deal of movement and conversation to pass; Serg. Ridley, repeating his movements and conversation, occupied 1 to 1¼ minute; Serg. Simpson, doing the same, needed 1¼ to 1½; Serg. Todd thought ½ min. elapsed, others even less.

Then came a tremendous *explosion* and prolonged rumbling lasting "some seconds," probably as much as half a minute. At York it was sufficiently startling to awaken sleepers and cause great alarm. A publican in Goodramgate could not rest till the police had tried his doors and windows. A lady was reminded of the Regent's-Park explosion, which she heard at eight miles distance. There was "one single grand burst, followed by a dull prolonged, gradually diminishing roar, as if at an immense distance" (J. B.). Serg. Simpson's description was striking:—Like a large cannon without any premonition (as some seemed to think), followed by a continuous roll, "which appeared exactly to retrace and roll back along the path of the light, decreasing as regularly as could be and dying off as long as you could hear it." Serg. Ridley spoke of the bang as louder than any cannon at a mile distance, the sound dying away like a wave on a rocky shore. The tremendous concussion, caused by an explosion surpassing a salvo of 500 cannon, though it must have taken place in a medium not a tenth part as dense as common air, shook shutters, doors, and windows everywhere, one policeman saying that the vibration seemed to affect the whole street.

When we attempt to make out the *apparent position* of the meteor, the difficulty, as usual, becomes very great. [On the annexed outline map the positions of the various places at which observations were made are roughly indicated.] Three of the York accounts tally very fairly. As the particulars were taken down separately and afterwards projected on a large plan, the results are perfectly independent. York observers all agree in making the meteor pass directly overhead; nor could any of them indicate to the slightest degree whether it passed a little on one or the other side of the zenith. Only one thought he saw the beginning well; and in most cases houses prevented good observations of the end. Some newspaper paragraphs spoke of the meteor's apparent position as "in the north."



Observer (York).	*Objects used.*	*Positions obtained.*
Messrs. Savage and Hughes.	The Minster, 530 yards off.	Right overhead to 41° W. of S.; alt. 10°, where it disappeared near a black cloud.
Serg. Todd.	Street direction and Monk Bar (he standing about the middle of Goodramgate).	Overhead (possibly a little S.E. of the zenith) towards 43° W. of S. (altitude undetermined).
Serg. Simpson.	From over Clifford's Tower, York Castle, to over Mount Villas on South Road.	N. 76°E.; altitude estimated 58°, *i.e.* 36° W. of S. (no altitude recorded).

The line joining Serg. Simpson's two positions passes perceptibly south-eastwards of the zenith. The direction obtained from a 4th report (S. 84° W. for the end) is probably not very reliable, as the observer, Serg. Ridley, was fresh to the district and hardly knew the ground. Of the others, the first observation, confirmed by both observers, was obtained (by myself upon the spot) by reference to the lofty top of a most conspicuous distant edifice. The final observation, also given by a prominent architectural object, seems very exact, and it agrees with a general impression of the reports of different observers of the meteor at York that it actually passed a little south-eastwards of their zenith. The *altitude* at the end being given, in the first observation, with reference to the Minster tower, is probably fairly reliable. Serg. Ridley gave lines of apparent altitudes indicating nearly 30°; but this more elevated position is no doubt to be attributed, in great measure, to the well-known tendency of estimated altitudes, especially near the horizon, towards exaggeration. For the same reason, Serg. Simpson's estimated altitude of the point of first appearance may be only 35° or 45° instead of 58°, as he sketched it on paper from estimation.

Considering the alarm and terror generally caused by the "fiery portent," the above results for York are eminently satisfactory. Many confessed that they thought it the end of the world, their actions being strictly in accordance with their belief. One constable went straight to headquarters "for further orders." None of them, unfortunately, succeeded in arresting the disturber of the peace!

Turning to other localities, at Pocklington (12 miles E.S.E. from York) a newspaper letter gives an account of the writer's being aroused by a terrible peal and shaking of the house, the light lasting long enough to let him get up and see every thing outside as if by daylight. At Gunby, 12 miles S.E. by S. of York, the explosion, roll, and tremor of the house were very apparent. At Filey, near Scarborough, about 40 miles N.E. from York, it sounded like a cannon at a distance, booming up apparently from the N.E. Even reports from Liverpool, Stockport, &c. speak of a large ball and a brightness approaching that of day. At Stockport, near Manchester, it flared forth in the sky on a person's right hand as he was walking N.N.E. The town of Stockton, in which the inhabitants were so terrified by the force of the explosion, lies almost due N.N.E. from Stockport; and had the meteor passed straight over Stockton and then over York it must have appeared to the observer at Stockport to have begun its course on his left hand, and thence to have crossed over to his right. Passing still further from the scene, the Dundee papers speak of it as "a brilliant and startling meteor, seen to fall in a westerly direction, apparently over the rising ground west of Newport." Taking the *centre* of Dundee and a point halfway from Newport to Woodhouse as the assigned direction, we have a line 17° E. of S., which passes just E. of Leeds and Sheffield. But the two base-points of the line are at once so near together and so ill-defined, that the exactness of places in its prolongation cannot be very much relied on. A line of 190 miles in length may very well deviate considerably when dependent on a rough bearing distance of 3 miles. Even further off was an observer, Mr. Nathaniel Short, Harbour Master of Shoreham, 6 miles W. of Brighton, distant 213 miles from York. Attracted by the light, he saw the meteor "with a long tail like a kite," of a slightly magenta colour, making it for a moment as light as day all round, moving slowly low down, from E. to W., between two hills, which may have cut off the beginning and end, from N. by W. to N.W. by W. He thought it went in a considerable curve, taking 30 secs. Clouds and not the hills, he adds, may have obscured it. He gives the "top" [or starting point] as rather less than ⅙ of the way up the sky, which agrees well w th a statement that he was ¼ mile from hills 250 to 300 feet high. The altitude obtained is 11° N. by W., which, taken as the line of sight of first appearance, passes just E. of York. The terminal line of sight towards N.W. by W. passes not far off from Gloucester! These positions evidently require to be "twisted round" to the E. (especially the last one), in order to bring them nearer to the north and to each other; for it is quite impossible that the meteor travelled further south than Yorkshire.

[But the observation is yet a most valuable and important one, as showing, from the slight inclination of the meteor's descending path from right to left along the north horizon at this distant station in the south, that the meteor's real slope of path was not itself *very* much inclined to the horizon, and that its real radiant-point (whose true bearing must certainly have been about N.E. ½ N.) was therefore also *at no very* great altitude above the horizon. Taking due north or nearly so as the direction in which the meteor was first seen at Shoreham, some way off the Yorkshire coast near Whitby, its real height there, which corresponds to an altitude of 11° at Brighton, allowing nine miles for the earth's curvature, was about 60 miles. It is not likely the meteor was seen at Shoreham much earlier, or eastward from the Yorkshire coast, than this; and as there can be little doubt that it descended below the level of the Brighton horizon soon after passing over York, or a point between Leeds and Selby, scarcely 12° or 15° W. of N. from Shoreham, the description of its termination, "N.W. by W." at Shoreham, and also the extent along the horizon of its visible path there, must manifestly stand in need of a very large correction. Taking this last extent as 15° (instead of 45° as described), and the first altitude, due N., as 11° (as represented), the apparent slope of path at Brighton, then arrived at, prolonged backwards as far as the bearing N.E. ½ N. of the radiant-point, gives for that point's position, at its assumed azimuth, an altitude of about 35°, which agrees very nearly with what the collective descriptions of its course, in the immediate neighbourhood of the meteor's track itself, generally indicate as its position. It may be added that the words used at Dundee, "falling *in a westerly direction*," reproduce as nearly as possible, *as they should do*, the observer's words at Shoreham, to describe the slope of path, since the meteor's "earth-point" (or the point of encounter with the earth of its line of flight prolonged) lies almost directly on the straight line between Shoreham and Dundee. By moving "in a considerable curve," it seems indeed to be pretty evident that the Shoreham observer intended to imply that the meteor, in the short course in which he saw it falling towards the west, descended "with a decided slope"—such a slope, we may perhaps assume, as has just been recognised, of about 37°, as corresponding at the Shoreham point of view to the adopted real path.]

Of the two remaining observations, one was by Captain E. Heselton, of the 'Margery,' from Seaham, Durham, to Scarborough. When two miles N.W. of Whitby * the light kindled for a second in the N.E., and then spread all around, while the meteor passed through the zenith from altitude 45° at N. by E. ¾ E. (20° E. of N.) to alt. 45° at S. by W. ¾ W. (20° W. of S.). The clouds were thick at the time. The light was as intense as at York, lasted ten seconds, and on vanishing a slight noise like distant thunder was heard.

The last observer, Mr. S. Walliker of Hull, had a good view of the meteor, and in answer to my request sends an admirable sketch plan of directions &c., referred to local objects traced from a map. He missed the beginning, having his back to it, and first caught sight of it 4° W. of N. (he thought it began 5° to 10° E. of N.) at an altitude of 60°, ending W. by N., altitude 20° †. (The altitudes, it should be noticed, in the last two accounts are only estimated ones.) J. Edmund Clark.

[To be continued.]

Handbook of Double Stars. ‡

Of the different classes of astronomical observation, we know of none more interesting—we might almost say fascinating—to an amateur astronomer than the measurement, or estimation, of the angular positions and distances of double stars. That we are not alone in this opinion is clearly proved by the many valuable series of observations contributed to this branch of stellar astronomy by some of our best observers, beginning with Sir W. Herschel. The duplicity of a few of the larger stars was known many years before the publication of Herschel's memoir in vol. lxxii. of the 'Philosophical Transactions;' but the birth of systematic double-star observing certainly dates from these early researches of the great amateur astronomer. Before his discovery of Uranus in 1781, Herschel resolved to make a systematic examination of all the principal stars, using a high power, in order that he might be able to select a sufficient number of those most favourable for his purpose. How he succeeded we have been fully informed in his different memoirs on the subject, and in the more recent synopsis of his labours, prepared by his illustrious son.

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* A newspaper paragraph had given it as 12 miles off land at Huntcliffe foot; but probably this is incorrect, as Captain Heselton in his note speaks of "being then two miles N.W. from Whitby."

† The positions are as deduced from the sketch.

‡ 'A Handbook of Double Stars, &c. For the use of Amateurs.' By Edward Crossley, F.R.A.S., Joseph Gledhill, F.R.A.S., and James M. Wilson, M.A., F.R.A.S. London: Macmillan & Co., 1879.

In the excellent work before us, 'A Handbook of Double Stars,' for which we are indebted to Mr. E. Crossley, Mr. J. Gledhill, and Mr. J. M. Wilson, we have a most valuable collection of observations of double stars made by different observers, including the earliest measures in the last century, together with detailed explanations of the methods adopted in the observations and in the determination of the orbits of binary stars. It is, as it professes to be, a true handbook for the amateur astronomer, to whom it will doubtless be of essential service; and we shall be much surprised if it does not find a place in every astronomical library.

The work is divided into four sections; the first being historical and descriptive of instruments and methods, contributed by Mr. Crossley and Mr. Gledhill; the second is mathematical, and contains a detailed account, by Mr. Wilson, of the calculation of the orbit of a binary star, with fully worked out examples, which cannot fail to be of great value to those interested in this kind of calculation. Mr. Wilson also contributes three chapters on relative rectilinear motion, on the effect of proper motion and parallax on the observed position-angles and distance of an optically double star, and on the errors of observation and the combination of observations. Each of these subjects is treated in that masterly manner which might be expected from so experienced a mathematician as Mr. Wilson. As an example of an orbit worked by Sir J. Herschel's graphical method, he has selected the star Castor as a double star of great historical interest, and sufficiently brilliant and widely separated to be within the reach of most telescopes possessed by amateurs. The explanation of the method is given by him with great clearness; and in it we have a discussion of all the observations, beginning with those of Bradley and Pound's measures in 1719, from which a series of elements are deduced, agreeing, within small limits, with those determined by Doberck and Thiele. This investigation is illustrated by diagrams. In chapter iii. of this section of the book, the authors have been fortunate in obtaining a most valuable contribution from Dr. Doberck, whose experience in the manipulation of binary orbits is second to none, as is well known to most readers of the 'Observatory.' This chapter is well worth a careful reading, as in it Dr. Doberck gives a detailed explanation of the working out of an orbit by analytical methods, taking for his purpose the binary star σ Coronæ; to which he has added an extensive table, containing a comparison of the final elements with 148 series of observations made at various epochs, commencing with Sir W. Herschel's measures in 1781.

The third section, compiled by Mr. Gledhill, contains a catalogue consisting of a selection of binary and other double stars deserving of attention. The catalogue is followed by lists of measures, with historical notes &c., of 786 stars, selected chiefly from the great works of W. Struve, O. Struve, and the two Herschels. It was the original intention of the authors to include a large number of the recent discoveries of Mr. Burnham. Owing, however, to the extreme faintness of most of his objects, some difficulty was found in selecting stars which might prove interesting to general observers; and considering that, as a rule, well-known stars only are suitable for the amateur, they have been omitted. The authors, however, remark that, "For the convenience of those observers who, having sufficient skill, patience, and instrumental power, may desire to assist Mr. Burnham in obtaining thoroughly reliable measures of his recently discovered pairs, we have appended to our Catalogue a selection from his published lists."

A mere glance at the synoptical history gives us an excellent idea of what has been done in the way of measurement of each of the 786 stars; and the historical notes are extremely interesting and valuable. The whole of the measures by each observer are grouped together, and the individual results are placed in chronological order. The authors state that this arrangement was found convenient in the compilation, as it exhibits at sight the observations of each observer in different years. We quite agree with them that this classification of the measures in observer's groups has its advantages; but, so far as our experience goes, we are rather inclined to prefer that adopted by M. Flammarion in his useful work, 'Catalogue des étoiles doubles et multiples en mouvement relatif certain,' in which the separate measures are arranged in chronological order without regard to the observer. By the latter method we are able to detect at a glance all the periodic changes of angular position and distance from the earliest to the latest observation. This facility of examination is particularly noticeable in a long and continuous table of measures, as in the case of γ Virginis, Castor, ξ Ursæ Majoris, and other stars, measured over a long series of years. Double-star observers, however, may find it advantageous to have the power of consulting the corresponding measures under both arrangements.

The total number of double stars catalogued, including those in the supplementary lists, is 1200. For these are given the approximate R.A. and declination, magnitudes, the numbers in the catalogues of the two Struves, and in Sir J. Herschel's general catalogue in vol. xl. of the 'Memoirs' of the R.A.S., besides other information.

The fourth section is devoted to a list of the titles of some of the most important works and papers on double stars, on the micrometer, and on the colours of double stars.

Though we have already briefly alluded to the contents of the first section in the 'Handbook,' we cannot close this notice without referring to Mr. Crossley's interesting contribution to the volume on the construction and adjustments of the equatoreal, on an astronomical clock, on the best form of observing-chair, and on the parallel-wire micrometer, which he considers to be *par excellence* the instrument of the double-star observer. These introductory chapters contain some well-executed woodcuts of Mr. Crossley's residence at Bermerside, near Halifax, including the

Observatory; the Temple Observatory at Rugby; the Bermerside Equatoreal; and specimens of observing-chairs, two of which are contrivances of the late Mr. Dawes. The book is well and clearly printed; and we heartily recommend it to our readers as a most valuable contribution to the literature of double-star astronomy.

E. Dunkin.

*Rand Capron's Auroræ and their Spectra**.

In the pursuit of any special line of inquiry, the most advantageous point from which to start is only to be found in a complete acquaintance with the previous labours of others upon the same subject. When these are all to be found well represented within the covers of a single book, although it be a capacious one, much of this preliminary labour is altogether saved. And future students of the subject of "Auroræ" may well be grateful for Mr. Capron's magnificent volume, which renders a very essential service to science by the completeness of its presentation of the subject. The history of early and recent observations of Auroræ is given with ample details; and the arrangement of them as illustrations of the different views which have arisen respecting the various characteristics of these phenomena is a striking proof of the impartiality of the author. All sides are fairly stated, without an endeavour to enforce any special theory; and the accounts of the various lines of inquiry which have been pursued in investigations respecting its cause, origin, and mode of development are all that could be desired.

The first five chapters deal with the general appearances and the special characters of Auroræ: their geographical distribution; the colours, height, and noises attributed to them; phosphorescence, and the production of ozone as attendant phenomena; and the polarization of auroral light—are some of the subjects treated of. The most curious part af Chap. V. is the account of Donati's investigation of what is called the travelling of Auroræ, which shows that on the occasion of Feb. 4, 1872, it was not observed in different parts of the Earth at the same instant of universal time, but appears to have become visible in the various localities *at the same local time*; and thereby indicating that cosmical causes combine with meteorological conditions to produce this wonderful and enigmatical light. However that may prove to be, *the successive propagation of Auroræ* is very curious, and deserves to be more fully worked out.

Chap. VI. treats of the possible connexion of Auroræ with other phenomena; as Clouds, Thunderstorms, Magnetic Disturbances, Atmospheric Electricity, Meteoric dust, and the Planets.

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* Auroræ: their Characters and Spectra. By J. Rand Capron, F.R.A.S. (Spon, London.)

Chap. VII. gives some account of singular aurora-like patches on the eclipsed Moon. There is evidence of very abnormal appearances seen on the lunar disk in eclipse, on different occasions, which are very difficult to account for. They have as yet scarcely attracted so much attention as they seem to deserve. Our author suggests analogies between them and polar auroræ; and were the evidence of that strengthened by future observations, it would of itself indicate the presence on the Moon of something of the nature of an atmosphere. If such an envelope surrounds our satellite, we know, for various reasons, it can only be in an extremely attenuated condition; but if Auroræ are developed in the higher regions of our own atmosphere, it may very possibly be the case that an excessively rarefied state is one of the special conditions necessary for the production of these phenomena. The curious subject of a lunar atmosphere has been sometimes debated with more vehemence than should be imported into matters of which the data are far from being absolute in character; and the most assiduous of lunar observers are generally agreed that many outstanding facts are most difficult to reconcile with the theory that the Moon is utterly and entirely destitute of any thing analogous to a lunar atmosphere. For instance, it is hard to suppose that the materials of the lunar crust can sustain the continuous and excessive heating which we know they receive during the prolonged lunar day (more than 500° Fahr., according to Lord Rosse), without giving off something of the nature of a gas. That the attraction of gravity is much less on the Moon than on the Earth, and the general absence of atmospheric pressure there, do not diminish, but rather increase the probability that some process of that nature is passed through, only, however, to be again reversed during the equally long lunar night. However future observation may decide the question, it is evident that no suitable lunar eclipse should be allowed to pass without close and careful scrutiny, so that the little-known appearances which Mr. Capron describes may be still better understood.

The supposed causes of Auroræ are detailed in Chap. IX., which completes the first part.

Chaps. X. to XIII. are devoted to a thorough investigation of the spectrum of the Aurora. Those who are familiar with Mr. Capron's previous work, 'Photographed Spectra,' will know that in this part of the subject he is completely at home, and was one of the earliest observers of this variable and difficult spectrum. Many observations of it are described at length, a thorough comparison of the whole is made, and a most interesting map of the spectrum, combining the whole results, is appended; and what will be of great service to future observers is that the discrepancies and difficulties of the subject are clearly pointed out. This is followed by a lengthy comparison of various tube-spectra with that of the Aurora.

An exhaustive examination of the behaviour of the electric discharge in various gases under the influence of magnetism, showing its bearing on the subject of the production and development of Auroræ, is contained in Chaps. XIV. to XX.

It is difficult to find fault with such a Galaxy of Lights; for Mr. Capron's book is as thorough and complete as possible, and will form a very valuable addition to any scientific library. The large number of excellent full-page plates adds very greatly to its importance and interest. The magnificent style in which the whole is produced, suggests that this splendid quarto is a rich gift to science, from the intensity of the author's own love for it more than any thing else; and it comes at a most seasonable time, allowing all intending observers of the next Aurora time to prepare and mature their plans, after taking advantage of the service Mr. Capron has rendered. HENRY PRATT.

Photographs of Jupiter.

WE are indebted to Mr. Common for the enlarged prints from photographs of Jupiter (Plate III.), taken with his magnificent 3-feet silver-on-glass reflector. Small though these photographs are, they give us much valuable information; and they have this great merit, as compared with the drawings of most observers, that they can be relied upon as accurate. It is little to the credit of those who attempt to make astronomical drawings, that a photograph less than $\frac{1}{20}$th of an inch in diameter should be sufficient to expose the inaccuracies of drawings on 300 or 400 times the scale. In this connexion we would invite our readers to compare Capt. Noble's representation of Jupiter in the 'Monthly Notices' for January with Mr. Common's photograph of Jupiter and the Red Spot. The photograph of Jupiter and his four satellites is important, as showing the relative brightness of the satellites. EDITOR.

Double Stars for February.

FOR the principal pairs best seen in this month see 'Observatory,' Vol. ii. The following objects will at once command the attention of the amateur.

α Aurigæ. R.A. $5^h\ 7^m{\cdot}8$, Dec. +45° 53′. Mag. A 1, B 12–13.

AB 183°·2 126″·2 1878·03

This small companion was discovered by β. A still closer object was detected by the same observer, 317°·5, 78″·17, 1878·9.

For the old companion we have

151°·4	169″·10	1783·6	H_1.
146 ·2	158 ·6	1878·0	β.

τ Orionis. R.A. $5^h\ 11^m{\cdot}8$, Dec. −6° 58′.

Mag. A 4, B 14, C 12, *b* very faint.
This is β 188.

AB	249°·1	36″·20	1877·9	β.
	250 ·1	35 ·98	6·2	Hall.
AC	60 ·0	36 ·09	7·9	β.
	59 ·8	35 ·97	6·2	Hall.
B*b*	53 ·5	....	7·8	β.
	49 ·3	3 ·77	6·2	Hall.

β Leporis. R.A. $5^h\ 23^m{\cdot}1$, Dec. −20° 51′. Mag. A 3, B. 11.

This is β 320. Rapid change in angle.

268°·4	2″·89	1875·1	De.
279 ·7	3 ·12	6·1	Hall.
287 ·2	2 ·67	7·1	O. Stone.
286 ·9	2 ·68	8·1	β.

θ′ Orionis. R.A. $5^h\ 29^m{\cdot}4$, Dec. −5° 28′.

Mag. 5th star 11·3; 6th star 11·3.

5th star	353°·6	3″·86	1832	Σ.
	354 ·3	4 ·02	78	β
6th star	125 ·9	3 ·62	44	Dawes.
	126 ·0	·96	67	De.
	119 ·8	·74	77	β.
	121 ·4	·98	78	Hall.

With regard to the other faint stars which some observers think they have seen in the trapezium, β writes, "There was not the slightest suspicion of any additional stars." This assertion rests on a very careful scrutiny on several occasions under very favourable conditions. The same observer strongly doubts the suspected variability of the 5th and 6th stars. He says, "They have always been seen with my 6-inch glass in the last six years, when the atmospheric conditions were suitable."

α Orionis. R.A. $5^h\ 48^m{\cdot}6$, Dec. +7° 22′·9.

The old companion (mag. 11):

152°·3	161″·77	1786	H_1.
·3	174 ·7	1878	β.

β's nearer companion (mag. 12–13):

348°·0	77″·59	1878	β.

Correction. In "Double Stars for January," p. 275, line 8, *for* Smyth observed *read* Smyth observed AD.

JOSEPH GLEDHILL.

Mr. Edward Crossley's Observatory,
Bermerside, Halifax.

Meteor Notes for February [*].

MORNING observations may be made between the 4th and 20th, and in the evenings during the first half of the month. The chief showers given in Mr. Greg's catalogue (1876) are from 180° +56°, 158° +29°, 175° +14°, and 75° +44°, and both Zezioli and Tupman give many positions of radiants, visible in the mornings of February, which it is desirable to reobserve.

Fireballs should be looked for on the 3rd, 7th, and 10th.

Recent Observations.—Mr. Corder at Writtle observed a fine fireball = ♀ on Dec. 6 at $9^h 38^m$. Path from 80° +45° to 105° +50°, but the beginning was not seen. A similar one was seen at 7^h the same evening, falling almost due N., probably through the tail of Ursa Major. On Dec. 10, Mr. Corder counted 69 meteors, chiefly between $10^h 15^m$ and $13^h 45^m$. Several fine meteors were noted at about 11 P.M., or slightly before that time—one in the E., then one S.W., and a third in the N.W. That in the S.W. was nearly = ♃, of a pale green colour, followed by flakes of fire and leaving a streak. The path was from about 2° +8° to 345° +5°, and it appears to have been the same meteor as that observed at Bristol and Bath ('Observatory,' Vol. iii. p. 278). Of the 69 meteors seen by Mr. Corder on Dec. 10, 36 were Geminids, with a radiant at about 106° +34°. The periodical shower of Jan. 2 was also observed by Mr. Corder on the evening of that date, between 6 and 10 P.M. Watching for about 3¼ hours he recorded 66 meteors, including 48 from Quadrans. The radiant-point seemed double, as follows:—232° +55° (36 meteors) and 230° +48° (12 meteors). Some of the meteors left streaks, others were trained. Those from the major radiant appeared to be swifter than those of the more southerly position.

At Bristol the sky was partly cloudy, with occasional storms, on the evening of Jan. 2, so that only 25 meteors were observed between 6^h and $9^h 30^m$ P.M.; 19 of this number were Quadrantids, with a good radiant at 228° +54°. The meteors were rather slow, with spark-trains; average length of path = 15½°. Considering the low altitude of the radiant-point and the frequently cloudy state of the sky at Bristol, the shower was decidedly a good one. On the evening of January 3 the sky was very clear, and the stars of Draco and Ursa were watched continuously between 6^h and 8^h, but not a single meteor was seen from Quadrans. The duration of this shower is evidently confined within very narrow limits.

Mr. Sawyer, at Cambridgeport, Mass., made a series of observations for the Andromedes at the end of Nov. and early in Dec., but obtained no evidence of the shower's appearance. Mr. Cornish at Notting Hill, London, W., also reports that no meteors were visible there in short watches on the nights of Nov. 27 and 28.

W. F. DENNING.

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[*] See 'Observatory,' Vol. i. p. 337, and Vol. ii. p. 345–6, for further notes on February showers.

CORRESPONDENCE.

To the Editor of 'The Observatory.'

Occultations of Bright Stars.

Sir,—

A perusal of the Rev. S. J. Johnson's paper on Occultations of Antares in your Number for July last induces me to send you the following statement of occultations of bright stars observed here by me during the past few years.

			Windsor M. T.			
			h	m	s	
1875, Sept. 7 ……	Antares.	R	7	11	17·7	A considerable fraction of a second late.
1876, June 2 ……	Spica.	D	12	34	7·2	Small fraction of a second late.
1876, June 2 ……	Spica.	R	13	39	5·2	Obs. not so good as that of the disappearance.
1876, July 3 ……	Antares.	D	12	14	41·0	Good observation.
1876, July 3 ……	Antares.	R	13	7	32·9	Good observation.
1877, April 2 ……	Antares.	D	11	4	53·9	Excellent observation.
1878, Aug. 7 ……	Antares.	D	6	43	12·9	Good observation.

The occultation of August 7, 1878, was observed with a telescope of 1⅝-inch aperture, and the other occultations with the 4½-inch equatoreal. I do not find, however, that I have made any remarks concerning the companion of Antares at the times of occultation, although at other times and under favourable atmospheric conditions I have frequently seen this small star with the last-named instrument.

I am, Sir, yours faithfully,

John Tebbutt.

Observatory, Windsor, N. S. Wales.
October 24, 1879.

Motion of the Spots on Jupiter.

Sir,—

The note of Mr. Backhouse in the December Number of the 'Observatory' may, to *some*, convey a wrong impression. I did not design to raise any query as to the visibility of the "red spot" during the autumn of 1878. On the *contrary*, I felt assured of its visibility from the observations of Messrs. Dennett, Trouvelot, Niesten, and others. I did, however, give some reasons for doubting the *constancy* of its visibility; and my reasons seem to be strengthened by the observations of Mr. Backhouse.

As to the error in the latitude of the spot, I only designed to give the *approximate* latitude as it was early in August. The spot has no doubt had *some motion* toward the equator since August. On Oct. 20 it was so far northward, that the shadow of Sat. III. was in apparent contact with its following end for 45 minutes. A similar motion, northward, was noted by Mr. Dennett last year. I do not pretend, however, that this motion amounts to 15° of latitude, for I have made no measures.

But to the apparent *change in longitude* I have given *close attention.* Having noted with great care, and by approved methods, the times of transit of its centre across the meridian *bisecting the disc*, during nearly five months, and having compared the meridians due at those times (according to Mr. Marth's ephemeris), I can say with all confidence that between Aug. 6 and Dec. 13 there has been an apparent increase of 21° of longitude. This may result either from a *real motion of translation* in a direction contrary to the planet's rotation, or from an erroneous value of the *time of rotation*, on which the ephemeris was built. This motion has been uniformly going on through the time, and has suggested to me the importance of directing observations to ascertain, if possible, whether the motion is *real* or *apparent.* I compared this spot for many weeks with certain grey notched and triangular patches in the *s. p.* quadrant. A month or two past these were very plain on good nights; and even yet they can be faintly seen on rare nights. Relatively to *these*, the red spot had no appreciable motion. I also compared it with various spots, some *white*, some *grey*, and some *brown*, between the two great equatoreal belts. These *latter* were found to pass the red spot with a rapid motion in the *direction of rotation*, sometimes moving its whole length in 3 or 4 days. Reserving discussion of these facts for the future, I barely mention them, hoping that something can yet be done to determine the reality of the retrograde motion of this spot. In this point of view I was specially glad to see the communication of Mr. Corder in the December Number of the 'Observatory.' The motion of the "white spots" has a bearing on this question.

I am, Sir, yours faithfully,

C. W. Pritchett.

Morrison Observatory,
Glasgow, Mo., Dec. 16, 1879.

Spots on Jupiter.

Sir,—

Two sorts of spots on Jupiter have claimed my attention during the past two or three years,—one the great coloured spot in the S. temperate zone, which I have had the good fortune to see on no less than 42 evenings; the other the white spots in the equatoreal zone. So far these are the only bright spots I have ever seen, for the tiny satellite-spots have hitherto escaped me.

The first-mentioned spot has this year claimed more or less the attention of nearly every telescopist, and is remarkable even if it were only for the different ways it has been seen by different observers. One observer calls it "a beautiful feature like a flame," whilst another, criticising him, remarks that the most astonishing characteristic of this "spot is its dense darkness." This sort of criticism should be withheld, because different observers frequently see the same object differently. The writer has always been struck, when observing this spot, by the want of that dark ground-work which characterizes Jovian markings usually; it has always appeared to him like a simple colour laid on the surface.

But this is not the only difference which has been noted, for some observers describe it as of one tint, whilst others hold that it is quite another, though the differences are not in accordance with the kind of instrument used, as is sometimes the case. Rose-tinted, fiery copper-coloured, red, vermilion, blood-red, and reddish or orange brown, have all been given as its colour. Some of these differences are doubtless due to the changes which have actually taken place in the colour of the spot, whilst others are as undoubtedly due to the personal chromatic equation of the different observers. At about the time Mr. Slack was describing it as reddish or orange-brown as seen with his 6¼-in. silvered With mirror, the writer described it as "decidedly *red*, with perhaps a very tiny tinge of yellow. The N. equatoreal belt is *reddish brown*, with a brilliant and deep *gamboge* fringe in the N. temperate zone," as seen with a 5¼-in. silvered mirror by Calver. A similar disparity is noticeable between observers using refractors. Some observers have fancied that it does not lie quite parallel to the equatoreal belts; but after many careful studies the writer is almost convinced that it does; but the S. equatoreal belt sinks a little under it, or, in other words, appears to conform with its northern outline somewhat, and further broadens immediately *f* the great spot and forms a forked streak. When first seen the spot was by itself, without any appendages; but since Aug. 5th, 1879, the writer has frequently observed trails either preceding or following the spots, and sometimes both. These trails have sometimes appeared of a grey colour, whilst at others they partook of the colour of the large spot. The last evening the spot was seen 1879, Nov. 11, at $5^h\ 55^m$; the *p* end of the spot was about on the central meridian, and a long streamer from the *p* end extended nearly or quite to the limb; and later, at $6^h\ 15^m$, a short one was seen *f* the spot. The Rev. T. W. Webb recently, November 12, thought the spot appeared to be paler in the middle, like two tufts. On one or two occasions lately the writer has fancied the colour more intense towards the ends and edges, but not sufficiently well to speak positively. There is only one more point respecting the great spot—that is, the aureole which many observers describe as surrounding it. I have only seen it on three or four occasions, and have never been able to satisfy myself that it was more than contrast.

Now turning to the torrid zone of Jupiter, we find a number of bright spots and patches lying nestled close to the N. edge of the S. equatoreal belt, sometimes extending nearly across the zone. I have ascertained the longitude as nearly as possible of every spot or patch I have seen, and laid them out on a table, the results showing as given below:—

Aug. 15, spot, 4°; Aug. 25, large spot, 8°; Oct. 5, large spot, 0°.

Aug. 25, large spot, 42°; Oct. 2, ditto, 34°; Oct. 3, ditto, 34°; Oct. 7, pale bright patch, 32°.

Sept. 1, spot, 70°; Oct. 5, pale large spot, 67°; Nov. 25, pale patch, 71°.
Sept. 4, spot, 90°.
Sept. 21, bright patch, 100° ±; Sept. 25, large spot, 100°.
Aug. 11, large spot, 110°; Aug. 23, bright patch, 107°.
Aug. 11, spot, 128°.
Aug. 11, large spot, 157°.
Aug. 6, spot, 163°; Aug. 9, spot, 164°; Sept. 14, large spot, 163°.
Sept. 21, large spot, 178°; Oct. 6, bright patch, 185° ±; Nov. 11, large spot, 181°.
Aug. 9, spot, 205°; Sept. 9, spot, 207°; Dec. 8, spot, 207°; Dec. 10, spot, 210° ±.
Aug. 31, large spot, 216°; Sept. 7, spot, 213°; Oct. 6, spot, 214°.
Sept. 14, large spot, 226°.
Aug. 31, large spot, 246°; Sept. 7, large spot, 253°; Oct. 1, bright patch, 246°; Oct. 6, large spot, 248°; Dec. 6, pale spot, 247° *.
Oct. 16, spot, 264° †.
Aug. 31, spot, 282°; Nov. 29, spot, 280° ‡.
Nov. 2, spot suspected, 290° §.
Sept. 17, large spot, 306°; Sept. 27, large spot, 307°; Dec. 11, large spot, 311°.
July 24, bright patch, 320; Oct. 11, spot, 315°.
July 29, bright patch, 340° ¶.
Sept. 17, large spot, 355°.

The grouping of longitudes given above seems to indicate that the spots remained tolerably stationary, and, on comparison with a similar analysis of last year's observations, some of the spots seem to be identical with spots then seen.

Before concluding, it may be mentioned that the red spot would from its position on the dates given by Mr. Backhouse, p. 250, be invisible at the times he mentions.

Yours faithfully,
Frank C. Dennett.

The Red Patch on Jupiter.

Sir,—

A. P. Holden is mistaken (p. 282) in thinking I called this spot merely a "long dusky mark;" what I stated in your December Number was, that a dusky mark was visible, in August 1878, at about long. 60°. But I see, from what C. W. Pritchett states in your last vol. p. 308, that the red mark must, the previous month, have had a long. of about 240°, and therefore the mark I saw must have been a different one. C. W. P. indeed says that the red ellipse

* Corder saw a spot near here Nov. 2.
† Corder saw a spot near here Nov. 5.
‡ Corder saw a spot near here Aug. 29.
§ Corder saw a spot near here Oct. 24.
¶ Franks gives a spot near here Aug. 22.

had a large proper motion; but the motion he ascribes to it, of "about one fourth the planet's diameter" in "nearly one hour," is only what would be due to the rotation of Jupiter. The dark mark visible this autumn has always appeared to me very red, while the northern belts have generally been strongly tinged with red, though less so than this mark. Its longitude a week ago was 278°.

Yours truly,
T. W. Backhouse.

Sunderland, Jan. 2, 1880.

On the Semidiameter of the Moon.

Sir,—

The letter from Prof. Pritchard in the January Number of the 'Observatory' seems to call for a reply from me. In this it will be most convenient for me to follow Prof. Pritchard in the way in which he has divided my criticisms.

I. The empirical formula given by me in my short note in the 'Monthly Notices' for November only professes to be an empirical formula which closely represents the mean of the observations made at a few selected observatories, those observatories whose observations I have reduced, and mainly the Greenwich and Radcliffe Observatories. It does not profess to represent the observations made by other observers at other observatories; and I cannot say how far it would represent such observations, though I believe it will be found to closely represent the observations made at most observatories by the great majority of observers. Prof. Pritchard attaches great weight to the influence of personality in observing the lunar semidiameter. My own investigations lead me to the conclusion that, in the case of the great majority of observers, the constant difference due to personality is small, though occasionally, as in the case of Mr. Carpenter at Greenwich, it amounts to over 1″. Prof. Pritchard points out that there is a great discordance, amounting to 6″, 8″, or even 10″, between separate results. It is true that at times there are discordances of this magnitude; but they are uncommon, except in the case of inexperienced observers. The discordances between the separate results arise from three separate causes:—first, errors in the theory, which gives rise to systematic errors, but of only small amount; secondly, real variations in semidiameter due to the lofty mountain-ranges on the Moon's limb—a source of a real variation in the Moon's semidiameter which may amount to nearly 4″ (these real variations can be determined and their influence allowed for; but when this is not done they produce apparent discordance between the separate results); thirdly, there are the purely accidental errors, which may reasonably be expected to vanish from the mean of a sufficient number of observations (with experienced observers these accidental errors do not often exceed 1″). I do not see, therefore, why 100 observations of the Moon's diameter, properly corrected for known errors, should not yield the diameter of the Moon within a small fraction of 1″ of arc, or how this value is vitiated by occasional discordances between separate observations amounting to 4″ or 5″, when it is known that there are real variations in the diameter amounting to nearly as much. Nor do these variations due to elevations on the limb interfere with the determination of the mean diameter of the Moon.

I do not see why Prof. Pritchard regards the result arrived at by the Astronomer Royal as vitiating the empirical formulæ found by myself. The Astronomer Royal finds a correction of +0″·18 to Hansen's semidiameter; I make the correction +0″·50. The difference is not very great, and it must be remembered that Sir G. Airy assumes the errors due to theory and real variation will counterbalance one another, which will not be completely the case where these errors are systematic, as in the present case. Prof. Pritchard next refers to the results lately arrived at by Prof. Winnecke; but he has overlooked an important consideration. The results given by Prof. Winnecke are not founded on direct measures of the diameter, but are derived from the apparent error of the lunar tables. The result, in fact, practically assumes that the corrected tables closely represent the real place of the Moon, or, at least, that there are no errors with periods similar to the parallactic inequality and variation. But the tables, even with Prof. Newcomb's corrections, do not represent the Moon's place with sufficient accuracy; so that the correction found by Prof. Winnecke arises from the outstanding errors in the theory. Prof. Pritchard's criticism fails, therefore, I submit, to touch the accuracy of my conclusions on this point.

II. Prof. Pritchard next remarks that in applying my formulæ to the De la Rue *reflector* I have omitted to notice the well-known diffraction-effect of the small mirror. I cannot agree with Prof. Pritchard. It is true in a reflector the diffraction-rings are to a very small extent larger and brighter than in a refractor, but to all intents the diffraction-fringe in a refractor and in a reflector are sensibly of the same breadth. In both theory and practice this is true.

III. Prof. Pritchard next remarks that Bessel's method of determining the place of a point on the Moon, and incidentally the true diameter of the Moon, is expressly devised to avoid the effects of irregularities on the limb. That is true; but it only succeeds provided the irregularities are equally likely on all parts of the limb, and do not affect the results by systematic variations. In both these essentials the method breaks down, for the irregularities are not equally distributed over the limb and give rise to systematic variations of fixed periods.

IV. Prof. Pritchard next says that I cannot possibly be justified in saying that were his own results compared with Burckhardt's tables instead of with Hansen's they would show very great discordances, on the ground that, as he had published no dates, I could have no basis for such a statement. I know that Prof. Pritchard did not publish any dates which would enable us to properly estimate the justness of his conclusions; but he did publish enough to enable me to make the statement which he takes exception to. For did not Prof. Pritchard publish what was practically a comparison between his own results and those of Hansen's tables, showing a tolerably close agreement? Now Burckhardt's values of the semidiameter are widely discordant from Hansen's, the difference varying rapidly and widely; they must differ, therefore, just as widely and variably from Prof. Pritchard's results; so that, had the tables been accepted as correct, the photographic results must have reproduced all these great discordances—that is, unless Prof. Pritchard means to maintain that *all* his photographs were by mere accident taken just at the very instant when Burckhardt's tables happened to agree with Hansen's.

V. My remark about Wichmann's measures agreeing much more closely *inter se* when Burckhardt's parallax and semidiameter were properly corrected, is, as Prof. Pritchard surmises, founded on actual computation. Properly corrected for the known errors in Burckhardt's tables, they do agree very much better. Prof. Pritchard can easily verify this by applying the known corrections to the values from the 'Berliner Jahrbuch' used by Wichmann.

I do not think I need add any thing more to the above, unless it be the hope that we shall have published before long the detail of the valuable researches in this direction which are being so successfully carried out by Prof. Pritchard. I am, Sir, yours faithfully,

January 3, 1880. E. Neison.

Large Meteor.

Sir,—

On the morning of Dec. 13, at $5^h\ 53^m$ A.M. G. M. T., a pear-shaped meteor, the light of which exceeded the full moon, was observed from this place. Its path was more horizontal than vertical. When first seen, the R.A. would be about $10^h\ 30^m$, Dec. 8° N., bursting about R.A. $11^h\ 45^m$, Dec. 3° S.,—originating in or near the ecliptic, but dipping slightly from the above toward the end of light. Yours truly,

1879, Dec. 23. John Robertson.

Lights and Shadows in the Astronomical Society.

Sir,—

In the last Number of the 'Observatory' you printed a letter from Professor Pritchard regretting that "irregularities" should disturb astronomical research at the Society's Meetings, and saying that I seem to have given a gallant officer "considerable provocation" at one of them.

Would you kindly invite the Professor's attention to the 'Monthly Notices' for December last, page 86? where he will read as follows:—

"In 1876 Mr. Brett directed our notice to the fact that a bright spot sometimes casts a shadow. Now on the night on which my sketch was made, the spot of which I am speaking not only cast *a* shadow (assuming these markings to be such), but *shadows*, as two were visible, one on its preceding, and the other on its following edge. I confess that no immediate explanation of this occurs to me."

The author here intended to indicate a discrepancy between my observation and his own, and significantly appealed to the meeting (November last) to say what could be the explanation of this remarkable circumstance.

The Fellows naturally looked towards me. I thought it would be trifling with their time and patience to enter into the obvious explanation that his spots were not my spots, neither were his shadows my shadows; but still I thought it would be as well in the public interest to disabuse his mind of all anxiety respecting the object I had written about in 1874*, so reminded him that he had not been accused of seeing it at all, and that I never supposed he would have known a shadow if he had seen one.

I went simply upon his own assertion that he had seen two shadows of an object falling in opposite directions. It was not competent to me to deny that he had done so, especially with that famous 4·2-inch Ross equatoreal; and the only explanation that occurred to me was that his knowledge of shadows was a minus quantity.

The consequences may have been regretable; but there was no sadness exhibited by the audience.

Another curious "irregularity" has occurred since the one I have referred to, which I fear will still further justify the regrets of the Savilian Professor. I am not personally concerned in it, but it is a curiosity which illustrates the Professor's letter.

At our last meeting (Jan. 9) the circumstances were different from those in November: we had nothing but pure practical astronomy the whole evening; but at its close this same gentleman, who had contributed neither drawing nor paper, but who, being on the Council, of course knew what the meeting was for, got up and made a long and indignant speech, vehemently complaining of its dullness and of the way in which "the time of the meeting was wasted by persons who liked to hear themselves talk!!"

Now, as the Savilian Professor has always taken a great interest in the credit of the Society, I think it may be worth his while to turn once more to our last 'Monthly Notices,' to see the gallant officer's powerful and original design for a new planet. This is the sort of thing our Council chooses to blazon before the world to show what England is doing in these days.

This is the first occasion since my election as a Fellow that I have ever uttered the least reflexion upon anybody's drawing; but now that it is evident my careful reticence has been completely misinterpreted, I may as well speak out and say that every Fellow ought to feel himself insulted by the insertion of this Grotesque in the 'Monthly Notices.'

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* The author misquoted the date of my paper.

Its designer is a person whom the Astronomical Society in its deep sleep goes on permitting year after year to manage its affairs and bring it into the contempt of the world.

Your most obedient servant,

Putney, 1880, Jan. 12. JOHN BRETT.

Mr. Main and the Council of the R. A. S.

SIR,—

I have recently had occasion to consult Mr. Dunkin's admirable and interesting 'Obituary Notices of Deceased Astronomers'—he deserves the thanks of the Astronomical Society for this, among many other excellent services which he has rendered to it. But, as a matter of history, perhaps it may be right to correct an error into which I suspect his amiability alone has led him. He speaks of the late Mr. Main as having "*retired*" from the Council of the Astronomical Society. This is by no means the whole of the case. Mr. Main's name was excluded from the House List of the Council by a very small majority. I was an amazed witness of the act; and I observed that it was the premeditated and preconcerted deed of a small but active section of certain members of the Council, who, however great their other merits may be, cannot be generally regarded as astronomers. A similar attempt was also made at the same time to oust another gentleman, who, like Mr. Main, had long served the Society as Secretary and as President; but it just failed, and only just. Eventually it succeeded by a *surprise.*

After that time, the internal troubles of the Society commenced; and how unhappily they culminated, it is not necessary for me to mention. Mr. Main had, as Mr. Dunkin truly says, always attended the Council on occasions when his advice would be of service; and it seemed, and it still seems to me, an ungracious and ill-advised act to have suddenly dispensed with the services of a most able and experienced astronomer, who had served the Society as Secretary and as President, and on whom it had conferred the distinction of its Gold Medal. After this, my friend naturally declined to allow his name again to be proposed as a Member of Council.

I may here, by the way, ask why Mr. Chambers's name has not been proposed for a seat in the Council? He has rendered great services to our science, and for a long time.

I am, Sir, yours faithfully,

Oxford, Jan. 14, 1880. CHARLES PRITCHARD.

NOTES.

Geodetic Junction of Algiers with Spain[*].—The completion of a great geodetic undertaking has been announced by M. Perrier and Gen. Ibañez on the part of the French and Spanish Governments respectively. The triangulation of Spain and Algiers having been carried out with great care, it was only necessary to connect the two systems of triangles in order to complete an arc of the meridian of 27° extending from the Shetland Islands in lat. 61° to the Sahara in lat. 34°. The difficulties, however, of the undertaking were very great, the distances being the greatest yet attained in geodesy, as we pointed out in a former note ('Observatory,' No. 25, p. 30). Four stations were selected, two (Mulhacen and Tetica) being on mountain peaks of the Sierra Nevada in Spain, and two (Filhaoussen and M'Sabiha) in Algiers between Oran and the frontier of Morocco. The heights of these stations ranged from 1000 to 3550 metres, and thus the lines joining them passed at a height of 300 or 400 metres above the sea, so that abnormal refraction was avoided, although the sides of the triangles were some 70 leagues. Provision was made for the use of the electric light as well as of heliostat signals; and it was fortunate that this was done, for the solar signals failed completely, not one having been seen either in Spain or Algiers. But the generation of the electric light by means of a steam-engine and Gramme-machine on such elevated peaks added greatly to the difficulties of the undertaking; and the transport of large stores of water and coal to the tops of these desert mountains required the services of from 30 to 100 men at each station, with from 15 to 20 beasts of burden. Another difficulty was the shortness of the time available for the observations. Before the middle of August operations in Algiers would be impracticable on account of the intense heat, and after September the fresh snows would drive the Spanish party from their station on M. Mulhacen. Everyone was at his post on August 20; but for 20 days of anxious suspense neither the solar nor electric signals were visible. At last, on Sept. 9, the electric light on Tetica was seen as a red round disk comparable in brightness to Arcturus, which was then rising over the sea. On Sept. 10 the electric signal from Mulhacen was perceived, and observations were continued regularly from that date to Oct. 18. The results appear to be very satisfactory. The enormous size of the four triangles concerned is sufficiently indicated by the values of the "spherical excess" (*i. e.* the excess of the three angles of the triangle over 180°, which forms a measure of its curvature or deviation from a plane triangle), viz. $54''\cdot16$, $70''\cdot73$, $43''\cdot50$, $60''\cdot07$; whilst the accuracy of the observed angles is shown by the smallness of the relative errors of the observed "excess," viz. $+0''\cdot18$, $-0''\cdot54$, $+1''\cdot84$, $+1''\cdot12$, respectively.

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[*] Comptes Rendus, Vol. lxxxix. No. 21.

Independently of the geodetic importance of the undertaking, the successful use of the electric light and its visibility at a distance of 70 leagues are results of much general interest.

TWO NEW NEBULÆ*.—M. Block, at Odessa, has found two bright nebulæ which are not in Herschel's General Catalogue and appear not to have been observed before. Their positions for 1880 are R.A. $3^h\ 28^m\ 8^s{\cdot}6$, Dec. $-26°\ 16'\ 32''$, and R.A. $3^h\ 33^m\ 48^s{\cdot}3$, Dec. $-26°\ 43'\ 42''$. The first is pretty bright and 5′ in diameter and hard to see with a high power, though very striking in the comet-seeker with a power of 27. The position given refers to a star of 10–11 mag. not in the centre of the nebula. The other nebula is bright, much condensed in the middle, and easily observed even in moonlight. It is remarkable that these nebulæ should not be found in the General Catalogue, whilst two fainter nebulæ near them are given, viz.:—No. 734, $3^h\ 29^m\ 53^s{\cdot}0$, $-25°\ 20'\ 17''$; and No. 757, $3^h\ 35^m\ 26^s{\cdot}7$, $-26°\ 36'\ 12''$. M. Block cannot see No. 757, which is faint and small, with his 5-inch Steinheil refractor; but No. 734 (pretty bright and large) is well seen, though decidedly fainter than the two new nebulæ.

SOLAR PARALLAX FROM THE VELOCITY OF LIGHT†.—Mr. D. P. Todd, of the American Nautical Almanac, has collected the various determinations of the velocity of light and deduced the corresponding values of the Sun's parallax, by use of the light equation from eclipses of Jupiter's satellites, and of the constant of aberration. Seeing that the best of the purely astronomical determinations are singularly and unaccountably discordant, Mr. Todd hopes that at some time in the not far distant future the velocity of light may serve to define the solar parallax within very much smaller limits than astronomers have yet known. To show what the method is competent to do at the present moment is the object of his paper.

Passing by Fizeau's experiments in 1849, we have five determinations of the velocity of light:—(1) Foucault's (in 1862), 298,000 ± 1000 kilometres per second (weight 1); (2) Cornu's first (in 1874), 298,500 ± 1000 (weight 1); (3) Cornu's second (1876), 299,990 ± 200 (weight 25); (4) Michelson's first (1878), 300,100 ± 1000 (weight 1); (5) Michelson's second (1879), 299,930 ± 100 (weight 100). The last is given in the 'American Journal' for November 1879, and results from 100 separate determinations, each being the mean of ten separate observations; the largest and smallest of the values are 300,142 and 299,692 kilometres respectively, and the computed probable error of the mean of the 100 results is only 6 kilometres. The computation of probable error is, however, in this case quite illusory; and considering all the sources of error, accidental and systematic, Mr. Todd estimates the probable error at 100 kilometres. Even then, this determination receives nearly four times the weight of all the others put together. From the five determinations Mr. Todd deduces as the most probable velocity of light 299,920 kilometres = 186,360 miles per second.

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* Astron. Nachr. No. 2287. † American Journal, 1879, January.

As regards the light equation from Jupiter's satellites, two determinations have been made:—(1) Delambre, $493^{s}\cdot2$, from eclipses 1662 to 1802; (2) Glasenapp, $500^{s}\cdot84 \pm 1^{s}\cdot02$, from observations of the first satellite, 1848 to 1873. Taking account of the greater precision of modern observations, Mr. Todd gives double weight to Glasenapp's determinations, and deduces the value $498^{s}\cdot3$, which, combined with the adopted velocity of light, gives for the Sun's mean distance 92,866,000 miles, and for the parallax (using Listing's value for the Earth's equatoreal radius) 8″·802.

Again, combining the velocity of light with Struve's constant of aberration, 20″·4451, the Sun's mean distance =92,768,000 miles, and the parallax =8″·811.

Mr. Todd next proceeds to estimate the probable error of these results. Taking each of the elements in the computation, he thinks the uncertainty in the velocity of light may fairly be taken at 70 kilometres, that of the light equation at 1 second of time, and that of the constant of aberration at 0″·025. This last is on the supposition that the motion of the solar system in space is not sufficiently great to affect the astronomical accuracy of determination of this constant. There remains the uncertainty in the relation of the absolute terrestrial velocity to the velocity in space; but this exists only in the result from the light-equation of Jupiter's satellites, the observations with the Greenwich water-telescope having shown that the constant of aberration is not affected by the passage of the light through refracting media.

Combining the maximum velocity of light with the maximum light-equation, and the minimum with the minimum, as affected by the estimated uncertainties, the extreme values of the solar parallax are 8″·782 and 8″·822, and similarly with the constant of aberration 8″·798 and 8″·824.

The mean of all the determinations of velocity of light, combined with astronomical constants, gives for the Solar Parallax 8″·808 $\pm$0″·006, and for the Sun's mean distance 92,800,000 miles.

Prof. **Grant** is preparing a valuable catalogue of about 6,400 stars, formed from 20 years' observations at Glasgow (1860-1880), all reduced to the epoch 1870. The Ertel meridian circle, with which the observations were made, is described (under the head "Circle") in Nichol's 'Cyclopædia of the Physical Sciences,' and is an excellent specimen of the German form of transit-circle. Prof. Grant has taken every care to eliminate instrumental errors, and the results do credit to his well-known skill. The Catalogue, which is intended to supply fundamental places of stars for use in comet and planet observations, will be ready for press by the middle of 1880: and an application has been made to the Royal Society to supply funds for printing it, out of the Government Grant.

Dr. **Terby**, Louvain, Belgium, asks us to mention that he has been engaged for some time on a general study of the observations of Venus, similar to that which he continues on the planet Mars, and that consequently he will receive with thankfulness, and will exchange, observations, drawings, and notes which may be sent him to complete the data indispensable for the study of this planet so hard to observe and still so little known. He would also take the opportunity of returning his thanks to those astronomers who kindly continue to send him their observations of Mars, and thus to facilitate the continuation of his 'Aréographie.'

M. **d'Abbadie** informs us that a site has been selected for a new French observatory on the Mont des Mignons, 300 metres above Nice, and that M. Bischoffsheim has secured 200 acres of ground for the site.

We have received a copy of Dr. Gould's 'Uranometria Argentina.' We gave some account of this fine work (derived from the 'Buenos Ayres Standard') in the 'Observatory' No. 32, and hope at a future time to give a further notice of it. It may suffice now to say that it includes a most complete discussion of the distribution of the stars in the southern heavens, and that the maps give by far the best representation of the heavens that we have met with.

The Total Solar Eclipse of Jan. 11 was (according to a Reuter's telegram) observed by Prof. Davidson and Mr. Frisby from the top of the Santa-Lucia Mountain in California and an intra-Mercurial planet is reported to have been seen on the occasion.

The Rev. E. Ledger will deliver lectures at Gresham College, Basinghall St., E.C., at 6 P.M., as follows:—Feb. 17, Mars, Minor Planets; Feb. 18, Minor Planets; Feb. 19 and 20, Jupiter.

Prof. Pritchett of Glasgow, U.S.A., writes that he has seen and tried to measure both satellites of Mars quite a number of times during the late opposition.

Dr. **Huggins** has recently obtained a better photograph of the spectrum of Sirius, which shows that the K line is really present as a fine line in this star.

Editor.

Astronomical Memoranda, 1880, *February.*

Sun. Feb. 1, sets $4^h\ 46^m$, rises $19^h\ 40^m$; March 1, sets $5^h\ 39^m$, rises $18^h\ 44^m$.

Equation of time:—Sun *after* Clock, Feb. 1, $13^m\ 47^s$; Feb. 11, $14^m\ 29^s$; Feb. 21, $13^m\ 54^s$; March 2, $12^m\ 13^s$.

Sidereal Time at Mean Noon:—Feb. 1, $20^h\ 44^m{\cdot}3$; Feb. 11, $21^h\ 23^m{\cdot}8$; Feb. 21, $22^h\ 3^m{\cdot}2$; March 2, $22^h\ 42^m{\cdot}6$.

Moon.	*rises.*			*sets.*			*sets.*	
	h	m		h	m		h	m
Feb. 1..	11	33	Feb. 11..	6	48	Feb. 21..	17	9
2..	12	55	12..	8	4	22..	17	36
3..	14	18	13..	9	19	23..	17	59
4..	15	36	14..	10	31	24..	18	17
5..	16	44	15 .	11	42	25	*rises.*	
6..	17	38	16..	12	52	26..	6	36
7..	18	18	17..	13	57	27..	7	57
8..	18	48	18..	14	58	28..	9	19
9..			19..	15	52	29..	10	42
10..	*new.*		20..	16	35	Mar. 1..	12	5

Last Quarter, Feb. 3, $3^h\ 38^m$; New, Feb. 9, $23^h\ 17^m$; First Quarter, Feb. 17, $15^h\ 46^m$; Full, Feb. 25, $13^h\ 22^m$.

Mercury is in superior conjunction on Feb. 14.

Venus is a morning star. The illuminated portion of the disk is 0·780 on Feb. 14, and 0·855 on March 15, and the corresponding diameters 13″·9 and 12″·1.

Feb. 1,	R.A. $18^h\ 9^m\cdot 3$,	Dec. 21° 53′ S.,	tr. $21^h\ 26^m$,	rises $17^h\ 22^m$
Mar. 1,	20 38 ·5,	18 32 S.,	22 1	17 34

Mars is an evening star in Taurus, in quadrature on Feb. 23. His diameter is 10″·7 on Feb. 1, and 8″·3 on March 1, and the illuminated portion of the disk 0″·894 on Feb. 14.

Feb. 1,	R.A. $3^h\ 25^m\cdot 8$	Dec. 20° 44′ N.,	tr. $6^h\ 41^m$,	sets $14^h\ 40^m$
Mar. 1,	4 22 ·6	23 28 N.,	5 44	14 1

Jupiter is an evening star, in Pisces, approaching conjunction.

Saturn is also an evening star, in Pisces.

Feb. 1,	R.A. $0^h\ 45^m\cdot 0$,	Dec. 2° 17′ N.,	tr. $4^h\ 0^m$,	sets $10^h\ 16^m$
Mar. 1,	0 55 ·6,	3 29 N.,	2 17	8 38

Uranus is in Leo, about 11^m *f* and 20′ *s* of ρ Leonis, in opposition on Feb. 25.

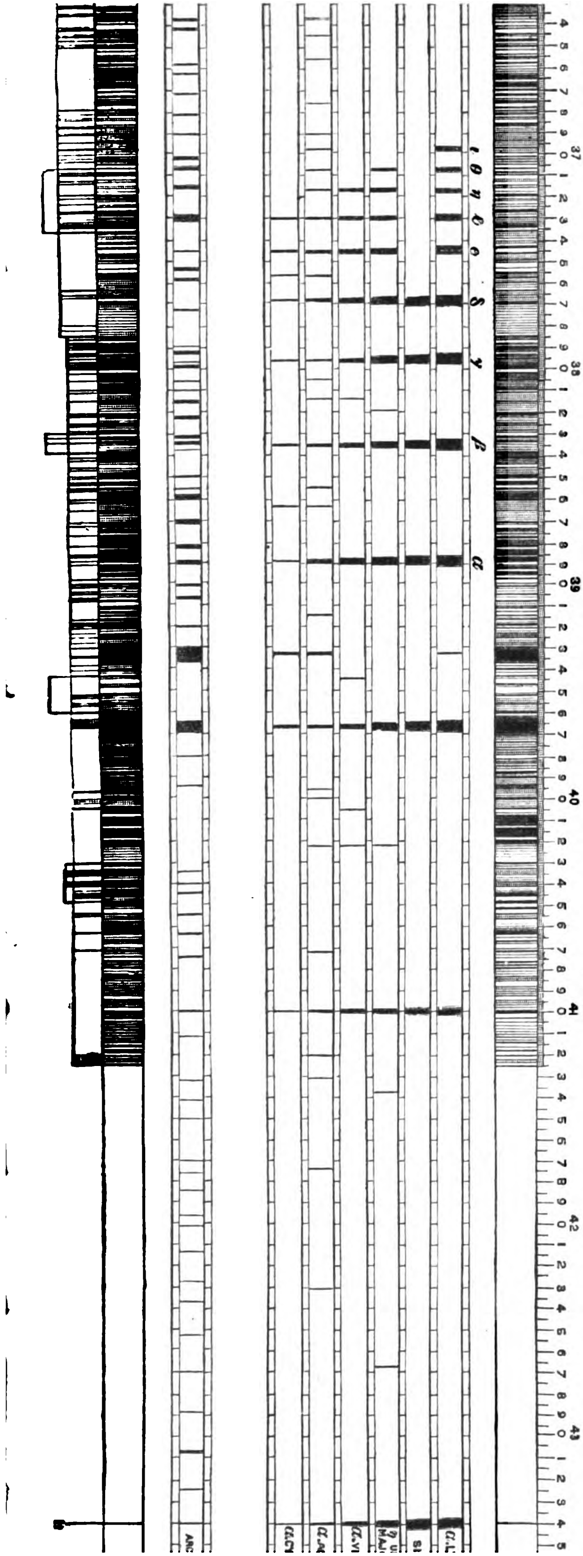
Feb. 1,	R.A. $10^h\ 39^m\cdot 8$,	Dec. 9° 20′ N.,	tr. $13^h\ 53^m$
Mar. 1,	10 35 ·2,	9 47 N.,	11 55

Phenomena.

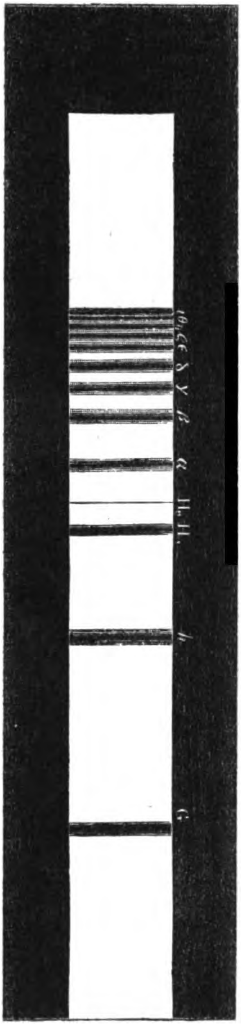
G. M. T.				G. M. T.			
	h	m			h	m	
Feb. 4	16	4	σ Scorpii Oc. R. 241°.	Feb. 18	7	16	62 Tauri Oc. D. 60°.
5	19	23	[5909] Oc. R. 256°.	20	8	17	5 Gemin. Oc. D. 132°.
12	5	27	9 Piscium Oc. D. 117°.	22	17	8	ζ′ Cancri Oc. D. 59°.
–	5	32	κ Piscium Oc. D. 154°.	24	5	18	ξ Leonis Oc. D. 147°.
	6	26	κ Piscium Oc. R. 265°.		10	39	o Leonis Oc. D. 52°.
16	7	37	μ Arietis Oc. D. 78°.		11	54	o Leonis Oc. R. 246°.
17	5	10	66 Arietis Oc. D. 161°.	26	18	6	e Leonis Oc. R. 250°.

The angles are reckoned from the *apparent* N. point towards the right of the Moon's inverted image.

EDITOR.



Photographic Spectra in Wave-lengths, and Cornu's Map of the Ultra-Violet.



Photographic Spectrum of α Lyræ.—W. Huggins.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

No. 35. MARCH 1. 1880.

ANNIVERSARY MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, 1880, February 13.

Lord Lindsay, M.P., F.R.S., &c., *President*, in the Chair.

Secretaries: Mr. J. W. L. **Glaisher**, M.A., F.R.S., and
Mr. A. **Cowper Ranyard**, M.A.

The Minutes of the last Anniversary Meeting were read and confirmed.

The Report of the Auditors for the past year was read, which was to the effect that having examined the Treasurer's Account, and the account of the assets and property of the Society, they certified them to be correct; that the receipts for the past year were £1411 12*s.* 6*d.* (excluding £700 received for the advowsons of Stone and Hartwell) and the expenditure was £1409 15*s.* 6*d.* (excluding £1074 5*s.* 0*d.* invested); that the cash in hand on the 31st of December, 1879, including the balance at the Banker's, amounted to £258 0*s.* 11*d.*; that they found the property of the Society in a satisfactory state; that the books and instruments and other effects had been examined as far as possible and been found to be in a satisfactory condition; and that they had laid upon the table a list of the names of those Fellows who were in arrear for sums due at the last Annual General Meeting.

Sir Edmund Beckett moved, and *Mr. Stone* seconded, a resolution that the Report of the Auditors be adopted and printed, which was carried unanimously.

The President. I should like to mention to the Fellows who are present that Vol. XLI. of the Society's Memoirs, with 18 plates in it, has just been published. It has been worked up by Mr. Ranyard during the last 8½ years, during which period all his leisure time has been devoted to the work. I am sorry that the Astronomer Royal is not here to speak of it: 25 copies only have been sent in, though 200 had been promised; but there will be another hundred sent in tomorrow.

Mr. Ranyard gave a summary of the Report of the Council for the last year:—During the past year 31 ordinary Fellows of the Society have been elected, 23 have died; the total number of Fellows is 592, and of Associates 43.

In the Report of 1878 it was mentioned that all the instruments belonging to the Society in the old lists published by the Council had been found excepting four; and now the Council can account for all these four. There was only one instrument supposed to be very valuable, and that was entered under the title of Abraham Sharp's Quadrant. During the past year a packet has been found containing a brass plate covered with lines and markings, with some notes by Dr. Priestley in which it is spoken of as "Abraham Sharp's Universal Quadrat,"—a very interesting historical instrument, when we remember what Abraham Sharp did for the instruments of the Greenwich Observatory. Besides Abraham Sharp's Quadrant, the Reade transit was missing, and the greater part of it has been found. With regard to No. 6 of the Sheepshanks instruments: all we know about it is that it had been lent to Mr. Jonathan Cape, and during the year we have heard from a gentleman that a friend of his who has had it in his possession was willing to return it to the Society if the Council would purchase for him a similar instrument. That letter was read to the Council in due course, and the Council determined that it was not worth while to replace that instrument by a similar one. It is a small telescope of 2¼-in. aperture, and of no real importance or historic interest. The fourth instrument was entitled "a thermometer;" and I think it is hardly worth while to take very much trouble about "*a* thermometer." Now we need have no paragraph with regard to missing instruments.

Besides Vol. XLI., Vol. XLIV. has been published during the last year. It is also a large volume, and it contains a paper by Mr. Neison, on a general method of treating the Lunar Theory, by Mr. Green on observations of Mars, with a map of the markings on the planet, a paper by Mr. Maxwell Hall, and a paper by Mr. Burnham on Double-Star observations made at Chicago, in the years 1877 and 1878, with the 18-in. refractor.

The Council regret to have to record the loss by death of the following Fellows and Associates during the past year:—J. R. Christie, W. K. Clifford, J. T. Cooper, George Creaser, H. M. E. Crofton, Richard Farley, Isaac Fletcher, J. G. Godsman, Sandford Gorton, Rev. H. P. Hamilton, Sir Rowland Hill, C. H. Johns, W. E. Jones, C. Judd, Rev. H. Cooper Key, Sir Thomas Maclear, H. Mann, Rev. J. N. Peill, J. E. Richard, Rev. A. Robertson, B. Templar, J. Waterhouse, S. C. Whitbread, R. Wilding, Prof. J. Lamont (Associate).

Biographical notices of the above Fellows were given, including one of Sir T. Maclear by Mr. Gill, and one of Prof. Lamont by Mr. Dunkin.

Reports had been received from a number of observatories, including one from Mr. Common and one from Prof. Pickering, Director of the Harvard-College Observatory.

Notices were given of the Discovery of twenty minor planets and of five comets, of Auwers' 'Fundamental Catalogue for Zone observations,' of Gould's 'Uranometria Argentina,' of Houzeau's 'Uranométrie Générale' and of his 'Répertoire des Constantes Astronomiques,' of the Selenographical Society, of G. H. Darwin's 'Researches on the History of the Solar System,' of Oppolzer's 'Treatise on the Orbits of Comets and Planets,' of the publications of the Dunsink Observatory, of Lord Rosse's observations of Nebulæ and Clusters, and of Lord Rayleigh's Investigations in Optics with reference to the Spectroscope—also a Report on Meteoric Astronomy by Prof. A. S. Herschel.

The President. With regard to one thing which is mentioned in the Report, as to there having been five comets discovered during the year, I have received a circular from the Royal Observatory at Kiel communicating a telegram from Dr. Gould as to a great comet passing the Sun northward. I should have sent this circular out myself on receiving it had it not been that it was printed; and I did not feel myself justified in printing the circular without having leave granted me from the person who printed it before. As soon as the comet is discovered, I hope it will be made known, and I will use my best endeavours to forward all the information possible to the Society.

Mr. Dunkin. Which way is the comet coming according to that telegram?

The President. Northwards.

Mr. Dunkin. We received two telegrams from Kiel, the first stating that the comet was going northwards, and the second, dated one day later, that it was going southwards; so that I fear we shall never see it in England,

The President. No Medal has been awarded by the Council this year; and, in consequence, there will be no President's Address. I will now ask that one of the Fellows should propose that the Annual Report be received and adopted, and that it be printed and circulated in the usual manner.

The resolution was proposed by the *Rev. Mr. Harley* and seconded by *Mr. Downing* and carried unanimously.

The President. I have now to appoint three Scrutineers for the Ballot for Officers and Council; and I will nominate Mr. Beck, Mr. Talmage, and Mr. Rand Capron for the office.

Mr. Mattieu Williams. I received yesterday a balloting-list in which it is proposed to substitute the name of Mr. Christie for Mr. Ranyard as Secretary; and I wish to ask the question whether Mr. Christie himself is desirous of this.

Mr. Freeman. It is a question which could not very well be answered. Mr. Christie doubtless would oblige the Society by accepting it.

Prof. Pritchard. If Mr. Christie or any other gentleman is desirous of being the Secretary, I, for one, think he is not the person for the office. It is so important an office, that I think it should be rather thrust upon a gentleman on account of his fitness than that he should seek it. And, gentlemen—although perhaps I should address the Chair—I think that that observation which I have presumed to make should apply not solely to the Officers of the Society. They are not offices that ought to be desired. No man who has a character to make should be a person selected to assist in the government of our Society, but persons who have made a character, and to whom, on account of that character, we can entrust the duty of assisting in the government of this, which ought to be a very noble Society.

Sir Edmund Beckett. I do not know whether this matter is being debated; but Mr. Christie voluntarily resigned his place on the Council, I remember, last year, and declined to be put up again. I merely wish to give that information in answer to the gentlemen who asked the question.

Mr. Christie. As a remark has been made about my own personal conduct, perhaps I may be permitted to explain the motive which led me to resign my seat on the Council, because it seems to have been somewhat misunderstood. I have always strongly urged that there should be every year a considerable infusion of new blood in the Council; and I thought it would be a very good thing, as it seemed invidious to suggest that anybody else should go off, that I should set a good example and go off myself. I never intended to withdraw from the Society or to refuse to serve. I do not wish to put myself forward for any office or for the Council. I can see around me at this Meeting, and also in the Society at large, many who would be far better fitted than I should be myself; and therefore I do not wish to put myself forward in any way. But I did not resign my seat on the Council, as Sir Edmund Beckett has put it, saying that I would not serve. I merely resigned for a period, in order to give some slight impulse to that principle which I thought it would be well for us to adopt in the interest of the Society.

Mr. Neison. Mr. Christie remarks that he does not wish to put himself forward as a Member of the Council; and I can quite understand that; but at the same time it does not apply to any other gentleman in the Meeting who wishes to see Mr. Christie on the Council, and who would be glad to urge his claims upon the Meeting. Now I have always been desirous of seeing Mr. Christie upon the Council; and I think it would materially strengthen the government if Mr. Christie were elected to it.

Mr. Moulton. Is it advantageous to the Society that there should be a discussion of the merits of the candidates before them?

Mr. Neison. I beg your pardon; I am not discussing the merits of candidates. (Confusion.)

The President. I believe there is no question of this being out of order; but it entirely rests with the Society to say whether they will have a discussion of that description or not. There is no matter of order. A simple question has been put, and the discussion is on that question.

Mr. De la Rue. It may not perhaps be out of order; but as a matter of good taste I would ask whether this discussion should be continued.

Mr. Neison. Perhaps the Meeting will allow me to proceed. (Confusion.)

Rev. Mr. Harley. I think it is quite competent to the Meeting to pass a resolution either that the question may be discussed or not; and I beg to move that simple questions asking for information may be put and answered, but that no discussion take place.

Dr. Pinches. I beg to second that proposition, because such a discussion would be interminable. If we are to challenge the qualifications of every Officer that is proposed to be elected to-day, we should have to adjourn *sine die.*

Mr. Neison. May I be allowed to say one word further? (Confusion.)

A Fellow. I rise to a point of order. There has been a motion moved and seconded, and I want to know whether Mr. Neison is speaking to that motion. If not, nothing is to be said by him. I presume that should be put to the Meeting before Mr. Neison continues any remarks on the subject.

The President. I should have put it to the Meeting had I not gathered from what Mr. Neison said that he had nothing more to say but one word before the resolution was put. It rests with the Society to say whether they will hear his remark or pass to the general business of the Meeting.

Sir Edmund Beckett. I think you cannot hear any remark until that particular question is settled. Any body can speak to that particular question.

The President then put the resolution:—"That no discussion be allowed to take place upon the merits of Fellows proposed for Officers or Council, but that simple questions asking for information may be put and answered."

The resolution was carried.

Mr. Chambers. I wish to ask a question for information. About twelve months ago the Society had laid before it a number of very useful draft-alterations in the Bye-laws of the Society, because some of these Bye-laws are really out of date and useless; and I should ask whether it is proposed by the Council to reopen the question of improving the Bye-laws; and, if not, what would be the proper form of action for a private Fellow to take. I ask this because I believe a large number of Fellows at a distance are rather dissatisfied with our present election arrangements, and they desire the introduction of some system of voting by proxy; and if the Council are not prepared to initiate some change, I propose to bring forward a motion on the subject if the Council will put me in the way of doing so. (Hear, hear!)

The President. The method of election of Officers and of the Council, and also the method of introducing any change in the Bye-laws, are clearly laid down in the existing Bye-laws. So far as I recollect, it is necessary for five Fellows to sign a requisition, which is sent in to the Council asking them to convene a Special General Meeting, and stating the certain object for which that Meeting is to be convened. No other business can be done at such Meeting except what is stated on the *agenda* paper. If the Council do not think fit to call such a General Meeting, I believe the Fellows of the Society who signed the requisition may call a Meeting themselves. By the Bye-laws 51 and 54, whenever the Council may think it advisable to propose the enactment of a new Bye-law, or the alteration or repeal of an existing one, they shall recommend the same at the Annual Meeting next ensuing, or at a Special General Meeting specially called for that purpose, and no alteration or repeal shall be brought forward unless one month's previous notice thereof shall have been given to the Council. I fancy, from Mr. Chambers's question, that he wishes to know whether the Council have it in their intention to introduce any modification of the present Bye-laws. The matter was thoroughly discussed, I think, two years ago, and at that time it was considered desirable to drop the question. Personally I should have preferred to see considerable changes made in the Bye-laws, as I think many of them are antiquated and obsolete; but I am not informed that the Council have at the present moment any intention of introducing such a question.

Mr. Neison. May I ask if I am at liberty to make any remark whatsoever upon this proposed Ballot, or will the Society persist in not allowing me to say one word in explanation?

Sir Edmund Beckett. I would suggest to Mr. Neison that one word always leads to two.

Mr. Neison. It will not in this case. It is simply to ask the Meeting to leave my name out of the Balloting-list, and to insert Mr. Christie's instead.

The ballot for Officers and Council was then proceeded with, and resulted in the following list:—*President*: J. R. Hind. *Vice-Presidents*: Prof. J. C. Adams, Sir G. B. Airy, Prof. Cayley, Edwin Dunkin. *Treasurer*: Francis Barrow. *Secretaries*: J. W. L. Glaisher, W. H. M. Christie. *Foreign Secretary*: Lord Lindsay. *Council*: Sir E. Beckett, A. A. Common, W. Huggins, E. B. Knobel, G. Knott, W. Lassell, A. Marth, E. Neison, A. C. Ranyard, Prof. H. J. S. Smith, E. J. Stone, Major G. L. Tupman.

The following were balloted for and duly elected Fellows:—Walter H. Bartlett and Capt. J. Steele.

The President announced that the following requisition, in accordance with Bye-law 51, had been handed in to him:—

"We the undersigned, being five Fellows of the Royal Astrono-

mical Society, desire to recommend to the Council the adoption of such measures as may seem to them expedient for a complete revision of the Bye-laws of the Society.

(Signed) C. Pritchard.
G. F. Chambers.
W. de W. Abney.
Henry Toynbee.
"Feb. 13, 1880." L. P. Casella."

Mr. Lynn proposed a vote of thanks to the Officers and Council for the past year, at the same time expressing his regret that the Fellows had not had the pleasure of listening to an Address from the President at this Meeting.

Mr. Brett seconded the motion, which was carried unanimously.

A vote of thanks to the Auditors and Scrutineers was then proposed by *Mr. Neison*, seconded by *Admiral Sir C. Shadwell* and carried unanimously.

The Meeting adjourned at 6 P.M.

The Great Detonating Meteor of 1879, *February* 24.
Part II.

On examining the different accounts of this magnificent meteor, we find that the York, Hull, and Whitby positions are undoubtedly the most reliable. Most exact will be the end position 41° W. of S. from York, crossed by the Hull direction at Micklefield, 16 miles from York, midway on the line from Selby to Leeds. The line from Whitby passes over Selby 12 miles E. of this spot; therefore we may regard the terminal point as pretty certainly determined. As has been seen, the Dundee report indicates a similar position, the most luminous part of the meteor's descending course there being seen in a direction from Dundee which, when projected by the description on a map, passes a little east of Leeds.

As invariably happens, however, the chief difficulty lies in determining the place of the meteor's first appearance. Here, indeed, we are obliged to make tentative deductions rather than to record simple facts. Using our three records we find that

(1) York observers thought it passed exactly overhead; but experience shows that "exactly" has rather a wide range here, and therefore we should probably pay attention to the indications that the meteor passed a little S.E. of the zenith, if this seems required by other observations; it may even be N.E.!

(2) We assume, with the Hull observer, that the meteor began (behind snow clouds) at least 10° or 15° before he first saw it, *i.e.* five or ten degrees east of north, from his point of view.

(3) Captain Heselton's observation, off Whitby, gives a common bearing of the points of end and commencement, which must be

slightly turned round to bring the end of the track to Micklefield, as already adopted. The York and Whitby lines, obtained by prolonging the meteor's path backwards from its concluded end point through the zenith of each of those towns, cut the Hull line at points 25 miles apart, the point of the York intersection being considerably southwards from the Whitby one. It will be seen that we are obliged to resort to the line of first appearance recorded at Hull, in order to obtain a convergence. The careful diagram, however, forwarded with that observation inspires us with considerable confidence in its positions, especially as Mr. Walliker knew the ground that it referred to, since it was at his front door that he saw and noted the appearance. It does not seem possible that the meteor passed much N.W. of the zenith at York, since observers thought, if any thing, it was on the S.E.; nor is it likely that Captain Heselton was far wrong in his positions. We must therefore compromise, and may finally accept a spot lying between the two points of convergence, giving to the mean of the three York observations a little more weight than to the single Whitby one. This gives us a position over the German Ocean 28 miles due N.E. of Whitby, and 48 miles E. 15° N. of Stockton. The projected line upon a map from here to the concluded end point passed five miles S.E. of the 'Margery's' place off Whitby, and one or one and a half N.W. of York. The meteor's greater actual height when passing near Whitby than when nearly over York would make the zenith-distance about the same at the two places, and hardly appreciable at either of them to ordinary observations. Horizontally, the distance run over by the meteor is 87 miles between the adopted points of first and last appearance. It may be remarked that the observations at Liverpool, Stockport, Birmingham *, Shoreham, and Dundee all point to a real course such as we have concluded, whilst *all* are opposed to a path passing nearly or directly over Stockton, which is what the startling accounts from there would most naturally have inclined us to expect. As to the *height* at appearance and disappearance, the data are small, since ordinary observers usually miscalculate extremely, in merely estimating altitudes. Most reliable should be the altitude of 10° of the end point, obtained at York by a measured comparison with the Minster towers. This altitude, on a base of 16 miles, gives a real height of *only three miles*! So unlikely, however, is this result, that we must look about for other evidence to furnish us with a probable solution of the final height. The violence of the explosion makes it almost certain that the meteor did come into a comparatively dense region of the atmosphere, and therefore very near the surface of the earth. We have, however, a good test of this in the interval between the meteor-flash and the sound of the explosion. An outside limit of the extent of this interval we saw was two or three minutes (24 or 36 miles); but several observers, repeating their movements during the interval, agreed pretty closely upon 1¼ to 1½ minute, or 15 to 18

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* [A letter in the Birmingham 'Daily Mail,' at the end of February last, states that two of the officials of the General Post Office there, when leaving the Post Office and turning westwards into a street at its north-eastern angle, were startled by a very bright light. On looking round they saw in the sky, away down over the bottom end of Newhall Street, a very large and very bright pear-shaped body, which lighted up the whole of the sky and the surrounding objects. It was falling slowly, and leaving a bright tail of some considerable length behind it. Its light resembled the electric light. It soon disappeared, and the sky in that direction became very black. No noise or sound of a report was heard. Some extent of open sky is visible to low altitudes from the point indicated across Hill Street and Ann Street, to near the point of junction (about 300 yards off) of the latter street with Newhall Street at its southern end. A line from the Post Office over the last few houses of Newhall Street, following this open view, bears (true) N.N.E., which is exactly

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the direction of Whitby from Birmingham. The meteor's adopted real course over Yorkshire would appear to descend in just this quarter of the sky from altitude 19° to altitude 5°, with a downward slope, however, *of* 37° *towards the left from vertical.*

The correctness of the indicated direction, and the thoroughly good and apposite descriptions of the meteor's appearance which are generally given in the letter, make it perhaps difficult to reconcile with the simple statement that the meteor "was falling slowly," the very decided northward slope of its motion which the descending body must have presented at Birmingham according to the adopted real path. But although this and the following observation seem to afford some presumption that the radiant-point's position may have been rather more northerly, and that the meteor may really have passed rather more nearly over Stockton than the adopted track assumes, yet the imcompatibility of this correction to any great extent, both with the Shoreham observation and with the many good positions of the meteor's apparent path obtained in Yorkshire, makes it more probable that the two Midland descriptions here related are each of them a little defective in the points where they seem to be somewhat at variance with the general determination. At a place in Cheshire, about 12 miles south of Manchester, Mr. R. P. Greg was informed that the meteor was seen by a watchman, from whom a rough account of its apparent position had been obtained. It appeared to pass "from N.E. to S.E., at an altitude of about 45°." The actual apparent course at the point of view which corresponds to the adopted track would be widely different, or from N.E., alt. 25°, to N.E. by E., alt. 8°. The broad statement of its apparent position communicated to Mr. Greg would therefore seem rather to confirm *very* roughly than in any way to contradict the calculations.]

miles. Even if the observers *halved* the altitude instead of *doubling*, as is usually done, the height is only six miles. Perhaps, under all the circumstances, it would be safer to do this; but further than that, it seems impossible to go.

[We may safely conclude from this that the point of departure of the meteoric sound, and therefore the nearest point of approach of the meteor's track to York, was somewhere between 15 and 20 miles distant from that city; for according to the most recently developed acoustic theory of meteor-sounds, which admirably accounts for their most distinctive and peculiar features, the petard-like report, or "detonation," which almost invariably ushers them in, is not in general (as has been supposed) the effect, either of an explosion of the meteor itself, or of its collision with air strata of increasing density by which its motion is finally arrested; but the united broadside of its "roar," reaching a hearer simultaneously from the parts of its track nearest to which he happens to be placed. The sound which strikes the ear from further-off portions of the track, arriving later in detail, sinks by the effect of interferences which have no influence on the first report, almost as rapidly as the sound broke out, into a fainter "roll" or rumbling sound, which mingles, as it dies away along the track, with the echoes of the opening explosion. All the observers' descriptions of the character of the sound at York (detailed above) present excellent examples of exact accordance with this explanation*. As the calculated end distance of the meteor's path from York is about 16 miles, this distance is in excellent accordance with the sound-interval, if the path was a very steeply descending one; but as the combined evidence of all the observations negatives this assumption, and shows that the meteor reached its end point passing over York by a moderately sloping rather than by a steeply descending course, it is evident that the end height, at the adopted place, must have been considerably above three miles, and probably at least six or seven. Indeed the fireball's proximity at York may have led the end part of its apparent path to be prolonged downwards by the observer there much lower than the dazzling body of the meteor really reached †; and in the far-off view at Hull of the end point,

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[* Refraction of the sound by wind, and warm or cold air strata, as well as moderate flexures of the track, may modify the effect and character of the report at any place, on this view, materially. It is probably to such influences of strong and very different air-currents in the snow-storm, that the extraordinary violence of the explosion heard at Stockton (far surpassing what was heard at Hull and Filey) may be ascribed; and even a curvature of the track (perhaps observed at Shoreham) may have assisted in concentrating the force of its effects to the alarming strength at Stockton which was there described.]

[† Prof. von Niessl records a very similar case of his own observation, at Brunn in Moravia, of the point of disappearance of the very brilliant fireball of June 17, 1873. Instead of passing behind a house-roof at last, as he thought he saw it do, the combined results of numerous exact observations showed that the point of extinction had really been above the roof, and that probably not even any dull embers of the meteor's head could thence have been observed at Prof. von Niessl's distant station to descend behind it.]

near the horizon, an earlier and higher point than that named as the lowest one at York may probably have been the point, in the direction of Micklefield, noted as the apparent point of disappearance.] Other observers at York thought that it disappeared higher up, which may in part perhaps be accounted for by clouds [or it may perhaps be taken to corroborate this explanation]. Serg. Ridley thought it was at 29°, which would make it 9½ miles. He and one other observer seemed to think that it hardly moved so quickly at first, which points to its having passed pretty close above their heads. The Hull angle (20°), at a distance of 42 miles, gives 15 or 16 miles; but halving the angle, as usual, only 7½ miles. There is no other available report; for the 45° at Whitby must have been a point where the meteor passed behind a cloud.

Turning to the *beginning*, we have as follows:—

	Altitude: given.	Altitude: adopted.	Distance. miles.		Resulting adopted height. miles.
York	58°	40	71		60
Hull......	60	40	71		60
Whitby ..	45	(uncertain)	26 (?)		(?)
Shoreham..	11	11	265	(+earth's curve =9 miles)	60

Assuming, as usual, a large exaggeration of the estimated altitudes, the agreement is pretty satisfactory.

[With regard to Captain Heselton's observation, the first sign of the meteor's light which he observed was like that of a rising moon in the N. behind the snowing storm-clouds, lasting a second or so before it illuminated the whole sky. The altitude of 45° at first appearance probably relates, therefore, to the first glimpse of light and to a much earlier and more distant portion of the meteor's flight than was either seen or figured, particularly in the diagrams at York and Hull. It probably represents very nearly a point not far above the meteor's radiant-point, and, assuming his nautical experience of measuring true altitudes at sea, would prevent Captain Heselton's estimate of the first altitude from being very materially overrated; it seems very probable that the radiant's real altitude, from the apparent height of first outbreak recorded in the 'Margery,' was not far from 30° or 35°.] The height above the adopted start point (28 miles N.E. of Whitby) given by the other observations is about 60 miles, while that of the adopted end point over Micklefield is 6 or 8 miles. With a horizontal extent of 87 miles, this gives a track whose oblique length was 102 miles, descending with a slope of 31½° from 39° E. of North, [which agrees very well with what Captain Heselton's account (and also, *see ante*, with what the Brighton observation) seems to indicate. The corresponding celestial position of the radiant-point was at 311° +54°, close to χ Cephei, a position, or rather region, near to which no meteor-shower centre in February has yet been observed. A near approach in the date and a distant resemblance in the radiant-point position is presented by the comet 1845 I. (Feb. 25, 309° +30°·5), and a distant resemblance in both respects also by the comet 1854, IV. (Weiss, Feb. 13, 304° +37°·3). A mean point for this close pair of cometary radiants had a point relatively to the York horizon, at the time of the meteor's appearance, 55° E. from N., altitude 17°, in the direction of Scarborough, and about 15° east of the adopted track and of the second or most easterly of the two low slanted alternative ones that are proposed below, provisionally, for trial with it. But it neither seems possible to reconcile with a track passing, as this would do, 30 miles over Scarborough and 16 or 17 miles over York, the careful observations at Whitby and at York of the meteor's starting place, nor to explain so easily with this more distant track the terrible detonation heard at Stockton. It conflicts also with the Birmingham and Manchester descriptions, although it would perhaps better satisfy the accounts of the appearance given at Shoreham and Dundee.] Although a little latitude must of course be allowed to all these final data of the meteor's real course, yet the thorough support which they receive in general from all the collected observations is sufficiently clear and plentiful to prove that the adopted real course is actually a pretty nearly correct determination. The end point may have been a little further off from York than Micklefield, and the course a little higher over Micklefield than 6 or 8 miles. The track may neither have passed between nor directly over either of the towns of York or Whitby, but perhaps north or south of both of them, and it, especially if the end height was higher, may have been lower than 60 miles over the North Sea and 40 miles over the Yorkshire coast at Whitby, descending towards the middle of Yorkshire with a more slight and gradual inclination to the earth's horizontal surface.

[We may, for example, suppose that it passed, a little before its end point, 10 or 12 miles over Micklefield, 15 or 16 miles above a point two or three miles north of York, and about 30 miles high directly over Whitby; or, again, at the same height, directly over York, and over the Yorkshire coast, seven or eight miles S.E. of Whitby. The shortest distance of the first or nearest of these two tracks, from Stockton, is about 35 miles, and the meteor's height there about 20 miles above the earth. For the adopted real path the least distance from Stockton is rather greater, or 41 miles, with a meteor height at the place of 26 miles; and the nearest point is a little closer, in this case, to the end point, about 42 miles on along the track. The radiants of the two less slanting courses are respectively at 322° +42°, 41° E. fr. N., altitude 18°, and 332° +46°, 33° E. fr. N., altitude 18°. Considering these to be only very doubtfully supported, a range of about 15° in R. A. and 10° in Dec., to either side of the above-determined radiant-place, may be taken as the utmost latitude to be allowed for the possible errors of its derivation. The probable radiant-point, as nearly as it can be reckoned, is then 310° (±15°), +55° (±10°).

The meteor-velocity for a parabolic orbit, with this radiant-point, is 18 miles per second. With a probable flight of 102 miles, performed, as we have seen, in not more than 6 or 8 seconds, the observed velocity was not less than 14½ miles per second. This result of the observations and reductions agrees so well with the theoretical velocity, that it may also fairly be regarded as affording some assurance of the general accuracy of the adopted real path.]

J. Edmund Clark.

Fireballs of September 23–24, 1876 and 1879.

A fireball several times as bright as Venus, producing a considerable illumination both of the sky and at some places also of the landscape and surrounding objects, appeared in Scotland at 8^h 10^m P.M. on the evening of the 23rd of September last, falling almost vertically down in the north, or west, as seen in Edinburgh and from several other places in the east and south of Scotland where it was observed*. Accounts of the appearance were sent to the writer, who requested descriptions of its path from distant observers in a contemporary Edinburgh newspaper, from Silloth (Cumberland), from Southwick and Garlieston in the south-west, and from Kennoway, Newburgh, &c. (Fife), Hawick, and Aberdeen in the east part of Scotland, besides accounts from Boness and the vicinity of Edinburgh. Some of these accounts contained, as the desired communications of the observers' recollections, maps and measurements; and in nearly all of the descriptions the steepness of the meteor's decending path was shown by an arrow-line sketch, inclining a little towards the right from vertical.

The meteor was blue or white, without any tail or sparks or persistent light-streak left upon its course, and it moved rather slowly. It increased gradually in the first two thirds of its course from the brightness of Sirius to several times that of Venus, and then as gradually decreased until, at about the brightness of Venus at last, it rather suddenly collapsed, about 3° before its end point, to a small red spark as bright as a first-magnitude star, which quickly afterwards disappeared. Both the position and the duration of the meteor's course (a path of 25° in between 3 and 4 seconds) were recorded and well mapped by the stars in Edinburgh, from a good view of the meteor's passage from the Calton Hill.

The real position of the end point is well determined by a good bearing on a map (along and across West Union Street) in Aberdeen*. Compared with the view of the meteor's extinction in Edinburgh, this direction shows that the bright nucleus itself disappeared about 25 miles, and the small spark or ember which remained afterwards about 20 miles above a point between Loch Laggan and Loch Oich, 10 miles N.E. from Ben Nevis. The direction of the course by which it reached this spot was from an altitude of 80° above the S.W. point of the horizon, or from a radiant-point at 288° +48° (±5°), between ι Cygni and π Lyræ. This position is established very accordantly by the intersections with each other of six or seven pretty widely distributed, independently recorded lines of flight. It can scarcely be in error more than four or five degrees, from the assemblage at this place, and close proximity all round it, of the backward prolongations of so many nearly vertical, distantly and independently recorded apparent slopes of path.

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* A description of this meteor, as observed in Edinburgh by the writer, was given in Mr. Denning's Notes for November (No. 31, p. 204) of "Recent Meteor Observations."

As mapped in Edinburgh, the meteor began its course at a height of 75 miles, almost vertically over Ben Nevis. It pursued its path to its point of final disappearance for a distance of 10 miles measured horizontally, and of 55 miles in the direction of its line of flight or of its actual descent. The time occupied in this length of path was about 3½ seconds, as observed at Edinburgh, making the meteor's velocity relatively to the earth about 16 miles per second. That of a meteor with the observed radiant-point moving originally in a parabolic orbit round the sun would be 15½ miles per second. This very near agreement of the actually noted velocity of the fireball with a theoretical one belonging to an orbit of the same form as those pursued by the majority of comets, is not, it should be noticed, arrived at in this case by any modification or adjustment to each other of the original observations. The near conformity with the theory is quite unsought for, and it results only from a geographical projection together of well-described positions furnished at two stations sufficiently far apart, and favourably enough situated with respect to each other and to the meteor's track, to afford a satisfactory determination of the length and position of the fireball's real path, the time of flight of which had at the same time been attentively and carefully observed.

The recorded course and velocity of the large fireball of September 24th, 1876, which fell at dusk (6^{h} 30^{m} P.M., Greenwich time) with prodigious brilliancy from about 60 miles above the coast of Belgium to a low height over the English Channel near

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* A compass-measurement from recollection at Kennoway, in Fife, supplies an intermediate direction, between those at Edinburgh and Aberdeen, of the end point, which converges with them pretty accurately to the same position. Azimuths obtained by similar measurements at Hawick, and by the stars at Boness and Newburgh, were, from their close neighbourhood to Edinburgh, and only superficial exactness, incapable of affording useful combinations; and, again, bearings at Silloth, Southwick, and Garlieston, all of them far-distant places pretty favourably situated for determining the meteor's real path, were noted as estimations only, and accordingly failed, as is the common wont of such descriptions, to indicate even approximately true directions.

Ostend*, bear, it would seem (however doubtfully any inferences of connection between the two meteors may be gathered from the fact), a strikingly well-marked resemblance to those of the present meteor of nearly the same date in 1879. Both of these fireballs of September 23–24, appearing in England and Scotland soon after sunset, penetrated the upper regions of the earth's atmosphere for some 50 miles, almost vertically downwards, with slow speeds of 15–16 miles per second, agreeing closely with parabolic speeds of 13–16 miles per second for the two radiant-points from which they were respectively directed, of the former and larger meteor at $285° +35° (\pm 5°)$ near δ, and of the latter, less brilliant one, at $288° +48° (\pm 5°)$ near π Lyræ. The two radiant-points, although they are only approximately determined, cannot, with fairness to the observations, be regarded as truly concentric, or as exactly coinciding with each other in position, but they are yet very near together; the longitudes of the two nodes also scarcely differ from each other two degrees on the ecliptic; and a parabolic, or nearly parabolic path, of each of the two meteors is a common form of their orbits which seems, from the good and particular character of the observations, to be pretty satisfactorily established.

The grand fireball of September 24th, 1876, even with its extreme brilliancy at disappearance, only 15 or 16 miles above the sea, did not, as far as all the known preserved accounts of its appearance state, produce any audible explosion; from other singularities of its appearance also, it can scarcely have been aerolitic or have projected any stony masses of its materials to the earth. It was more probably similar in its nature and in the consistency of its substance to the large but silent fireballs which sometimes form part of the meteor-bearing comet-streams of April, August, and November. It reached two principal maxima of brightness in its course (the first very large, and the last of them thoroughly dazzling and resplendent) in two equal lower section-lengths of its path. Between these two phases of resplendency it sank almost suddenly to its original brightness of an ordinary falling star, which was the rather inconspicuous appearance that its nucleus presented in an upper or previous similar section-length at the beginning of its track. At the last collapse of the bright nucleus, which was as sudden as the first, it disappeared totally, leaving on the lower portion of the track, between about 15 and 30 miles above the sea, a white cloud-streak in the sky, visible at the Orwell-Park Observatory, near Ipswich, for sixteen minutes before it disappeared. The nucleus was round and scarcely, if at all, pear-shaped, without any tail or flakes of sparks flying from it, as described by Mr. J. I. Plummer at the latter place; and its light was white or blue without any prismatic colours. In this, and in its sudden subsidence, first to a small spark in midcourse, and finally to total extinction, the aspect of the nucleus must have nearly resembled, on a much larger scale, the gradually increasing round blue disk, without any tail or sparks, and the sudden diminution of the ball of light at last to a mere spark-like ember in its flight, which were noted in the smaller, but yet very conspicuous and abundantly well-observed fireball of September last.

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* A collection of original accounts of this fireball is preserved and discussed in the volume of the British Association Reports for 1877, pp. 108, 134, and 138. The parabolic velocity, 12 miles per second, given for its radiant-point on p. 134, should be 13 miles per second.

Although the smaller fireball left no streak, yet this is also a common character of the smaller April *Lyrids*, while larger meteors of that shower, and also of the December *Geminids*, leave phosphorescent streaks; and in its first moderate expansion the great fireball of September 24, 1876, itself left no enduring light-streak on its track. But it should also be recollected that the enduring visibility of the large meteor's streak was doubtless due, in great measure, to reflection of sunlight from its particles, since in the cloudless heights of 20–30 miles above the surface of the sea in which its middle and upper portions were deposited, the sun had only set from a quarter to half an hour during the time that it remained in view, whereas it had set from three quarters of an hour to an hour, and the shades of nightfall had begun to settle darkly upon both shores of the English Channel, when observers there were watching the "chalk-mark in the sky" of the meteor's streak resolve itself into cloud-wreaths, and gradually fade away to disappearance. Whether, therefore, the "white wreath" in the sky that marked the end portion of this exceptionally large fireball's track was a truly phosphorescent or only a sunlit smoke-cloud, cannot now be affirmed; and could not, without the assistance of a spectroscope and a sufficiently attentive study of the appearance of its spectrum at the time, have been certainly determined.

There is thus no reason to regard the occurrence in twilight of a light-cloud on the track of the vastly larger fireball, and the total absence of any such appendage in the night-time on that of the much smaller one, as any real proof of physical difference between them, in the nature and character of their materials. The several features and phases of appearance of the nuclei of both these fireballs, it may rather be asserted, favour the supposition that they were originally of a like description in respect of their constitution or of the structure and composition of their substance. The consideration of their appearance, therefore, notwithstanding the great difference of their magnitudes, serves to strengthen the probability of the conclusion that these two September fireballs may perhaps have been somewhat erratic members of a common meteor-stream.

In his general list (1876, No. 113), Mr. Greg notes a pretty active meteor-shower in August and September, at 281° +38°; but the individual determinations which it includes have rather high declination in September. Thus G. and H. B_5, lasting to Sept. 25, at 285° +44°, Heis B_7, Sept. 1–30, 286° +57°, and Schmidt, Sept. 1–10, 290° +58°, are the only meteor-showers in this neighbourhood yet recorded in September. The first of these fairly bears comparison with the observed radiant-points of the fireballs of September 23–24; and as they seem to confirm the existence of such a meteor-stream, shooting-stars and fireballs observed on the two nights of September 23rd and 24th should be mapped and compared with the radiant-region in Lyra from which they emanated, to ascertain if they conform to it. The extent and character of the meteor-system of which these two fireballs may have been representatives will thus, perhaps, in time disclose itself; and much light would be thrown upon the doubtful question of the origin of these, and of similar sporadic fireballs, if the fireball system which they form, or the shooting-star system to which they belong, should, in this way, ultimately come to be more definitely determined.

A. S. Herschel.

The December Geminids, 1879.

The following is a detailed account of the partially successful observation obtained this year of the December Geminids.

Previous to Dec. 7, cloudy weather and the moonlight interfered with the observations, and cloudy skies unfortunately prevented watching on Dec. 8, 10, 11, 13, and 14; but on Dec. 7, 9, and 12 the sky remained generally clear, and good observations were obtained, the deduced results being considered of value, inasmuch as they offer a partial solution at least of the question regarding the cause of this radiant's seeming diffuseness.

The evening of the 7th was remarkably warm and the sky very clear; and a watch of 4 hours was sustained from 7^h to 11^h, 20 meteors being recorded. Of the 20 noted, 8 radiated from positions in Gemini as follows:—5 very accurately from near θ Geminorum, at R.A. 100°, +35°, and 3 indicated quite exactly a position near α Geminorum, at R.A. 110°, +33°.

From the former position, the meteors were long, faint, and rapid, while those from α Geminorum were short, bright, and rapid. Of the unconformable meteors, 2 or 3 were evidently members of the Taurids II., and three short and bright meteors indicated a position near α Aurigæ, at R.A. 75°, +45°. On the 9th a watch of 2 hours was obtained, from 8^h to 10^h, in a nearly clear sky, and only 7 meteors were noted, 4 being from the positions in Gemini, 2 each from θ and α Geminorum.

On the 12th an observation of $3\frac{1}{2}$ hours was obtained in a very clear sky, from 8^h to $11\frac{1}{2}^h$; and meteors were found to be more numerous than during the previous evenings, 37 being recorded, the hour from 10 to 11 producing 16.

The Geminids furnished the majority of those seen, 21 belonging to this shower or showers. Of the 21 noted from Gemini, 6 radiated from the position near θ, and 15 (representing the principal shower) radiated very accurately from near α Geminorum. The meteors from the position near θ Geminorum were generally long, faint, and rapid, as during the previous evenings, while those from the position near α Geminorum were very short, quite bright, and generally very rapid, their extreme shortness being noticeable even at some distance from the centre of radiation, two or three being nearly, if not absolutely, stationary. Several very bright ones were recorded, and these were invariably of a beautiful emerald-green colour.

The brighter meteors were generally accompanied by streaks, but the fainter ones (=2 mag. stars and under) were not.

Of the 21 recorded from Gemini, 4 were seen from 8^h to 9^h, 5 from 9^h to 10^h, 9 from 10^h to 11^h, and 3 from 11^h to $11\frac{1}{2}^h$. The meteor-tracks mapped on the 7th and 9th were deduced separately from those recorded on the 12th, but no shifting of either position could be detected. The mean of the two positions would give a radiant at about R.A. 106°, +34°. No contemporary displays of any intensity could be detected, although 2 or 3 of the unconformable meteors mapped belonged to the Taurids II., and 1 or 2 radiated from the position near α Aurigæ, at 75° +45°. The characteristic features of the meteors belonging to the two centres of radiation in Gemini deduced were remarkably dissimilar throughout the series of observations, and would seem to show almost conclusively that two meteor-streams lie very close together, although perhaps representing detached portions of the same meteor-swarm.

There was a steady auroral glow present in the north on the 12th. **Edwin F. Sawyer.**

Cambridgeport, Mass., U.S.A.,
Dec. 16, 1879.

Proposed Monument to Halley.

In the January Number of the 'Observatory' last year a notice was given of Mrs. Gill's simple narrative of the expedition to Ascension in 1877, made by her husband (whom she accompanied) for the determination of the Solar Parallax by observations of Mars in opposition. It will be remembered by the readers of the modest but successful little book that in it a lively account is given of the pleasant detention at St. Helena of the party whilst awaiting transhipment; and we are pleased that the astronomical associations of this historic island brought to mind by Mr. and Mrs. Gill are not unlikely to bear good results in causing a lasting memento of the great Halley to be erected on the actual site of his observatory. We read at p. 33 as follows:—

"David strolled along to 'Halley's Mount,' to search for the site of the observatory where Halley, in 1677, made his catalogue of southern stars and observed the transit of Mercury. We did not know whether any record of this work remained in stone and lime; and it was a pleasant surprise to find on the spot that an astronomer's eye at once picked out as the most favourable a bit of low wall, duly oriented, and overrun with wild pepper (*Cluytia pulchella*). This had been the observatory without doubt; and near to it is a quarry from which the stones for its erection had evidently been taken. So charmed was my husband with this interesting record of the work of 200 years ago, that his investigations and surmises regarding it left us short time to linger in the little hollow lying near the foot of Halley's Mount."

Then and there the present astronomer at Cape Town expressed to the writer of this notice, who was with him, his desire of seeing this neglected site recognized by some memorial, however humble; and there is now reason to hope that the subject will be ventilated and soon assume a tangible form. A simple monument will best suit the subject and the locality to mark the very spot of Halley's observations; but a more practical monument, and one really needed, would be the reestablishment of Johnson's observatory, whose desecration Mrs. Gill pathetically laments at p. 26:—"I say observatory; alas! it is so no longer. Fallen from its high estate, it is now the artillery mess-room; and in the recesses formed for the shutters of the openings through which Johnson's transit used to peep they stow wine-glasses and decanters, and under the dome they play billiards! It may appear ungrateful to speak so of a change which was productive of so much kindness and hospitality to us; I do not grudge the hospitable St.-Helena mess their mess-room, but I do regret that so fine a site for an observatory is vacant."

To the readers of the 'Observatory' the name of Halley needs no recommendation, although to the modern public we fear the name conveys but misty and vague recollections as nebulous as cometary emanations. The list of his scientific achievements yields to few in rank and importance. His transit method for obtaining the solar parallax, his prediction of the periodic comet, and the discovery of the acceleration of the Moon's mean motion would alone entitle him to a proud position among our scientific worthies, without counting his laborious service to the state by his improvement of the method for obtaining the longitude and calculation of the Lunar tables, his tide observations and cartography of the Channel coasts, and, lastly, his optical investigations. We do not often think of him as a sailor; and yet, previous to Cook, Capt. E. Halley was our first scientific voyager and the constructor of the famous magnetic chart with the isogonal curves of variation, better known as the "*Halleian lines*;" whilst not least, the friend and coadjutor of Isaac Newton, it was owing to his energetic zeal that the first edition of the 'Principia' was published in 1686–7.

The little island of St. Helena has not much to be proud of; surely the great name of Edmund Halley should be inscribed on the mount where he won his spurs on the lonely rock in mid ocean under Capricorn! Whilst speaking of St. Helena, it may not be out of place to remark that this, the smallest and most isolated of our many small insular colonies, possesses in the person of its governor a most enthusiastic astronomer, born and bred as such we may say; for his boyhood was spent beneath the shadow of Johnson's observatory, when it was used as such, and he has ever since imbibed the astronomical atmosphere which environs the island he has never yet left, besides which he is no distant relation of the great Encke.

No more ardent admirer of Halley exists than Hudson Ralph Janisch, C.M.G., F.R.A.S.; and under such an administrator of the government it is fit that the monument should be raised in the island.

As may be imagined, in such an insular position, in the heart of the southern trades, and right in the homeward track of the Australian and East-Indian shipping, the time-ball and clock are most important items in the economy of a colony dependent upon ocean-going vessels for its existence; and, to the credit of the Governor, be it said that he personally regulates both by astronomical observation.

It is an inexplicable anomaly that His Excellency should have to content himself with a home-constructed amateur observing station in the recess of a deep ravine, whilst six hundred feet above it is a scientifically constructed observatory commanding an extensive horizon, solely used as a smoking symposium.

Space does not permit more than this brief notice of the intention of several of those interested in the island, as well as in Halley's work, to erect such a monument as we have mentioned; and from our universities and observatories we hope that a hearty response may now be given to the call for aid in the erection. It is proposed to convoke a meeting in London early this season, when all those who care to aid in the work will be invited to attend. We also hope to be able to announce the long-looked-for biography of Halley, the compilation of which has for some time been in course of preparation for the press.

In the 'Graphic' of February 28th there is given an engraving after a sketch of our own, and well rendered on wood by Mr. M. J. Skill (of the Institute of Painters in Water-colours), taken from the eastern side of the mount; and in an early number of the 'Gardener's Chronicle' this month will be shown a most interesting view in the valley under the western side of Halley's Mount, showing the indigenous composite vegetation, notably *Lachanodes*, the so-called she-cabbage tree of the islanders, which in this secluded spot yet lingers out its doomed existence. The former drawing shows the modern exotics, and the latter the pristine and native plants, which reigned supreme at the time of Halley's visit in the seventeenth century.

S. P. Oliver.

*Astronomy for Beginners**.

ANY ONE desirous of giving his children some notion of the relative positions of the planets, and of their peculiar physical features, will go a great way in converting them into young astronomers by presenting to them a copy of Miss Giberne's excellent little book, entitled 'Sun, Moon, and Stars; a Book for Beginners,' which has lately appeared in an attractive form, with several coloured plates of astronomical phenomena, and a preface contributed by the Savilian Professor of Astronomy at Oxford, who introduces the work to the public as a tale of the stellar universe told with great simplicity in an earnest and pleasant style, free from any considerable inaccuracy or any unpardonable exaggeration. In it we have the outlines of elementary astronomy almost entirely expressed in untechnical language; and we are inclined with Professor Pritchard to regard this successful attempt at explaining a great subject in common words as a considerable feat in popular astronomical writing, and one of much practical utility.

On glancing over the pages of this little volume, which has no pretensions further than as a first book on astronomy, we were much struck with the general astronomical accuracy pervading the work, from the first chapter to the last; and though of necessity only the barest elementary outline of the great results of modern astronomical research is given, the authoress is evidently at home in her subject, and she has availed herself of the material found in the most recent editions of reliable treatises on astronomy. She has therefore from the best sources derived the principal facts worked up by her with great literary skill into the interesting and pleasing story of the progress of our science, which will, we are certain, afford instruction to many a young student, and even to others of more mature years, who are entering for the first time on the study of astronomy, and to whom more advanced and technical treatises are sealed books.

There are, we believe, some astronomers who are inclined to consider that elementary popular works on astronomy of this kind do little towards the real promotion of the science, on account of the general absence of detail in the descriptions, and that such works are looked upon and esteemed more for their artistic appearance and entertaining reading than as a means for acquiring solid information. This is in some respects true; but we happen to know, from experience, that in recent years a love for the science has been created among a large class of inquirers after knowledge by the perusal of such popular and untechnical explanations of the movements of the heavenly bodies as can be found in this little book, and in several others that we could name. In fact, many who have eventually turned out to be good amateur observers have first acquired a taste for astronomy from the attractive manner with which the subject has been often presented to the reader in elementary and popular works. We should like to know how many of the Fellows of the R. A. S. owe their first introduction to the science to the perusal of some popular treatise on astronomy.

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* Sun, Moon, and Stars; a Book for Beginners. By Agnes Giberne. With a preface by the Rev. C. Pritchard, M.A., F.R.S., &c. London: Seeley and Co., 1880.

In Miss Giberne's nicely printed book the descriptions of the different members of the solar system are given in two separate parts, the first of which is in some measure introductory to the second, as the latter contains many additional details respecting each of the planets. A third part is devoted to a consideration of the principal objects in the stellar universe, including the results of the spectrum analysis of the light of different types of stars. The coloured plates will be attractive to many, though we cannot help thinking that some of them are far more sensational and imaginary than what can ever be found in nature, especially the supposed lunar landscape and the made-up illustration of the appearance of the heavens at midnight on Saturn. Both of these might well have been omitted without detracting from the usefulness of the book. We have, however, read the work with a considerable degree of pleasure; and we can have no hesitation in indorsing Professor Pritchard's opinion, that it supplies an undoubted want, and that it is an easy and untechnical exposition of our science well suited to beginners. E. DUNKIN.

The Double Star Σ 547. A Variable of long period?

THIS difficult double star was discovered by Σ in 1829, and was measured by him in 1829 and 1832. He describes it as "Anonyma, major 8·5, minor 11·5," and adds "mensuræ, ob comitis debilitatem, minoris sunt præcisionis."

The next measure, so far as I know, is that by Mädler in 1845. Then, after a long interval, it was looked up by Dembowski in 1865, but without success; the companion was invisible.

In 1879, while compiling a list of binary stars, I frequently turned to this object, but always without seeing a trace of the minute *comes*. However, on receiving a copy of Mr. Burnham's recently published measures of double stars, I was much pleased to find that he too had sought for the missing star. He also failed in 1873, 1876, and early in 1877; but late in the year 1877 he was successful in both finding and measuring it. I at once renewed my efforts to obtain a few measures, but with only moderate success. Once only did I see it, and that was on a superb night early in January; and although I have turned to the object on almost every fine night since, I have not been able even to glimpse the companion. In the hope that amateurs possessing leisure, large apertures, and fine skies will occasionally look at this faint star and secure a few measures, this note has been written.

The subjoined figures will show the history of the star up to the present time:—

°	″		
344·33	4·250	1831·39	Σ.
·7	....	1845·04	Mädler.
Companion invisible.		1865	De.
”	”	1873 & 1876	β.
9·8	2·46	1877·9	”
14·0	2·25	1879·0	”
15·8	2·4	1880·0	Gl.

It appears, therefore, that there has been considerable change since 1829, that either the companion is a variable or the very finest nights are required to render it visible, and that the system is worthy of the careful attention of double-star observers.

The star's place for 1880 is R.A. 4^h $19^{m}\cdot8$, Dec. − 1° 40′.

Joseph Gledhill.

Mr. Edward Crossley's Observatory,
Bermerside, Halifax.

Double Stars for March.

The few but interesting objects which follow, together with those already given in the 'Observatory,' Vol. ii., will enable the amateur to select suitable objects for the aperture at his command. Many of them are excessively difficult, and it is extremely probable that some of them cannot be measured at all in England.

OΣ 92. R.A. 4^h 52^m, Dec. +39° 13′. Mag. 5·7, 9·8.

247°·4	2″·78	1878·0	β.

Binary.

So 503. R.A. 5^h $49^{m}\cdot1$, Dec. + 13° 56′. Mag. A 7, B 8, C 13.

AB	118°·6	7″·07	1875·2	De.
AC	157 ·3	28 ·09	1878·0	β.
BC	167 ·8	....	·0	β.

De called attention to this star in 1875, owing to the remarkable changes in angle and distance due to proper motion. C was discovered by β. South's distant star C is thus given by De:—angle 335°·4, dist. 230″·1, in 1875·2.

Σ 951. R.A. 6^h $33^{m}\cdot8$, Dec. +9° 56′. Mag. A 8·5, B 10·7.

AB	309°·2	21″·43	1878·1	β.
AC	229 ·2	11 ·56	·1	β.

C is one of Mr. Ward's new stars.

OΣ 159. R.A. $6^h\ 46^m{\cdot}9$, Dec. +58° 35. Mag. A 4·7, B 7·2, C 12–13.

AB	1°·0	0″·48	1878·1	β.
AC	31·4	23·64	·1	β.

C is Burnham's new companion.
Binary, in rapid motion.

Mr. Edward Crossley's Observatory,
Bermerside, Halifax.

JOSEPH GLEDHILL.

*Meteor Notes for March**.

MORNING observations may be obtained from the 5th to the 21st, and evening watches may be instituted during the first half of the month. The principal radiants derived from the Italian observations were at 182° +56°, 177° +10°, 158° +28°, and 111° +62°. A radiant of slow brilliant meteors should also be looked for at 300° +80°. On the 7th two active showers, found by Tupman, are due from the points 244° +15° and 233° −18°, and there are many other streams visible at this time in Ursa, Leo, Hercules, Virgo, Serpens, &c. On the morning of March 15, 1877, the writer determined positions at 263° +48°, 218° +36°, 280° +14°, 277° +25°, and 250° +51°. Zezioli had seen the former at 261° +47° on April 1, 1868, and the latter shower (from between Hercules and Draco) is well marked in April. Amongst the numerous showers given by Greg the best appear to be at 50° +48°, 192° +2°, and 147° +47° for this month; but many showers are subject to annual variations which have been very irregularly observed, so that it is impossible to specify those entitled to precedence. In some instances the radiant annually reappears at the same point without any decided difference in intensity; in fact nearly all of the streams with which we are conversant are of annual recurrence, though many give indications of periodical variations as well.

Fireballs should be looked for on the 1st, 2nd, and 4th. The apparition of many large meteors early in this month deserves mention, as probably referable to the action of several rich showers which hitherto seem to have eluded discovery.

Recent Observations.—Several large meteors were seen in January: An observer at Welling, in Kent, writes:—"Saw a large meteor in the N.W. at about $5^h\ 20^m$ this afternoon (Jan. 3). It was of a greenish-blue colour, falling straight downwards as if into the earth, and of an immense size." Later the same night a fireball = ♀ was observed at Writtle, falling in the S. by E., at $11^h\ 20^m$, and leaving a streak. Large meteors were also reported on Jan. 19 and 30, but the positions not fully and accurately described.

On Jan. 12, $11^h\ 5^m$, Mr. Downing on Blackheath saw a 1st-mag. meteor with a course from λ Ursæ Majoris, vertically downwards to within 20° of the horizon. Duration 1 sec. On Feb. 4, 7^h, a bright 1st-mag. was noted by the writer at Bristol, traversing a path of about 15° and passing some 6° below γ Eridani from the direction of the stars in the belt of Orion, and probably belonging to a radiant-point in the region of Cancer.

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* For further notes on March showers, see 'Observatory,' Vol. i. p. 377, and Vol. ii. p. 382.

Of 661 meteor-tracks registered by the writer in 1879, the mean apparent length was 11°·34. This agrees very nearly with the average length of 3203 paths (recorded in the years 1876–78 at Bristol) = 11°·23, and referred to in the 'Observatory,' Vol. ii. p. 306.

Mr. Corder classifies his meteors according to speed and appearance:—(1) The swift streak-leaving meteors; (2) slow, trained meteors; (3) small quick meteors, short, and sometimes streak-leaving. Out of an aggregate of 1463 shooting-stars seen by him in 1879, the swift class represented 43 per cent., the slow 17 per cent., and the remainder (40 per cent.) were of the small quick class. Results of this nature are of course greatly influenced by the period of observation, the swift meteors being largely predominant before daybreak, while in the evenings there is a majority of slow meteors. W. F. DENNING.

CORRESPONDENCE.

To the Editor of 'The Observatory.'

The "Red Spot" on Jupiter.

SIR,—

On the 28th of January I succeeded in obtaining a transit of the spot; and as it will probably prove to be the last during this apparition of Jupiter, I beg to send it to the 'Observatory.' I saw no change in the general appearance of the "spot."

Jan. 26th, 1880. The following end of the "spot" was on the central meridian about 4.30 P.M. G.M.T.

Jan. 28th, 1880. The preceding end was central at $5^h\ 10^m \pm$, the centre at $5^h\ 40^m \pm$, and the following end at $6^h\ 15^m \pm$ P.M.

I am, Sir, yours very truly,

JOSEPH GLEDHILL.

Mr. Edward Crossley's Observatory,
Bermerside, Halifax.

The Meteors of November 13–14.

SIR,—

At the date of my note in the 'Observatory' for January, I had received no particulars in regard to Mr. D. E. Hunter's observations at Washington, Daviess County, Indiana. In a subsequent letter Mr. Hunter informed me that a party of observers, consisting of himself and about a dozen of his students, occupied a square tower, there being three persons on each side. "Most of the meteors," he says, "were seen in the E.N.E., N.N.W., and near the zenith. Very few were seen by the entire company. The first five that I saw all radiated from Leo. When they appeared more numerous we ceased to trace them. Of the entire number I am unable to say more than this—that a large number of those that *I saw*, perhaps one half and possibly more, could be traced to Leo or very near it."

Many meteoroids of this stream have doubtless been thrown into new orbits from time to time by the earth's disturbing influence. The period in such cases may now differ considerably from that of the principal group. I am, Sir, yours truly,

Daniel Kirkwood.

Bloomington, Indiana, U.S.A.,
January, 1880.

The faint Stars in the Trapezium of Orion.

Sir,—

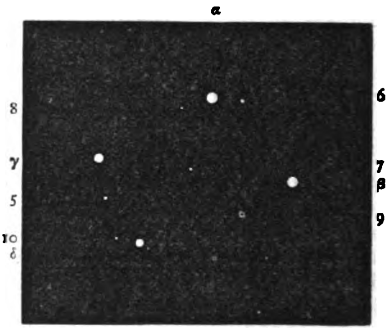
Mr. Burnham having failed * to see more than 6 stars in the Orion Trapezium, some doubt will be thrown upon the existence of the additional stars distinguished by Mr. Huggins in 1866. Mr. Burnham says, "I have very little faith in the real existence of these suspected stars after the failure of this and other large refractors to show them," and doubts that the fifth and sixth stars are variable, from the fact that during the last six years they were always visible in his 6-inch glass when the atmospheric conditions were suitable ('Observatory,' Vol. iii. p. 315). The testimony of so practised an observer must have considerable weight, though it cannot be held to settle the question.

Early in 1870 the late Mr. Edmund Salter, of Manchester, sent me some valuable observations of the Trapezium, obtained with a fine 12-inch mirror by Bird. He says:—1869, Dec. 3, 8h to 13h. "In the Trapezium 9 stars certainly, 10 probably were visible. A youth of 15 saw the nine immediately, though his attention was solicited only to the four large stars and to the fifth star. He was confident also as to the tenth, which he saw by glimpses."

1870, Jan. 25. "Examined Trapezium in Orion. My son readily made out 9 stars thus:—

"The numbers show the degrees of brilliancy. A tenth was suspected to the left of the lowest star. I saw all these myself, but not all at once."

1870, Jan. 29. "Again made out 9 stars in the Trapezium. In σ Orionis distinctly made out 15 stars, 9 faint ones and 6 plain ones. Aperture 12 inches, power 320."



Trapezium in Orion, Jan. 25, 1870.

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* See his "Double-Star Observations made in 1877–8 at Chicago," Memoirs of the R. A. S., Vol. xliv. pp. 238, 239.

The positions approximately assigned to the small stars by Mr. Salter agree with the diagram of this asterism given by Mr. Huggins ('Monthly Notices,' Vol. xxvi. p. 71) from observations on Jan. 6, 7, and 8, 1866; and their actual existence can hardly be questioned in the face of such closely corroborate observations. Moreover, Mr. Huggins, in a note to his paper referred to, says, "On this evening [Jan. 8] my friend Mr. S. B. Kincaid visited the observatory and saw all the stars figured in the diagram."

That Mr. Huggins and Mr. Salter could have independently assigned coincident positions to objects having no real existence is beyond the range of conjecture. In each case the stars were seen on several evenings; and the results are so distinctly and confidently expressed, that it is difficult to see how the observers could have been led into error.

As a proof of the excellence of Mr. Salter's telescope, it may be mentioned that on Jan. 15, 1870, he saw 19 spots or craterlets upon the floor of the lunar crater Plato.

I am, Sir, yours faithfully,
W. F. Denning.

Ashleydown, Bristol,
Feb. 8, 1880.

Disappearance of Jupiter's Satellites.

Sir,—

On the morning of October 15, 1883, I find there will be an entire disappearance of Jupiter's Satellites for a short time. The configurations given in the 'Nautical Almanack' are:—

1883, Oct. 14^{d},	iv Tr. I.	12^{h}	1^{m}
	i Ec. D.	12	42
	ii Tr. I.	14	33
	iii Tr. I.	15	56
	i Oc. R.	16	15
	iv Tr. E.	16	15
	ii Tr. E.	17	26
	iii Tr. E.	19	27

whence it appears that from $3^{h}\ 56^{m}$ to $4^{h}\ 15^{m}$ A.M., or for about 19 minutes, the planet will appear without any visible attendants, and will be well above the horizon of Greenwich for this curious circumstance to be noticed. Observers will remember the last occasion was on August 21, 1867, when Jupiter was denuded of his comites for a much longer period.

Smyth, in his 'Cycle,' does not notice the other instances of this, viz. in 1843, 1826, and 1802, but mentions an observation of the like character by Molyneux on November 2 (O. S.), 1681, inferring that it was an instance of three satellites being simultaneously eclipsed—"a conjunction," he remarks (p. 177, vol. i.), "which will require more than three thousand billions of years to occur again!" It does not, however, appear that any coincidence of such rarity took place on this occasion, but merely such a disappearance as has been noticed four times since the commencement of the present century.

Flamsteed seems to have paid attention to the phenomena of Jupiter's satellites; but he has no recorded observations of them in 1681, in his 'Historia Celestis,' from October 16 to November 8.

Query, Is there no recorded instance of the like coincidence being noticed in the interval between Molyneux's observation in 1681 and Sir W. Herschel's in 1802? Faithfully yours,

Abbenhall, Mitcheldean, Gloucestershire, February 12.

SAMUEL J. JOHNSON.

The Royal Astronomical Society.

SIR,—

I had no intention, at the Annual Meeting of the R. A. S. on Feb. 13, of making a speech, for it would not have been regular to have done so at the time when I rose to ask the President the question which I put to him. Nevertheless I think it may be desirable that I should take an opportunity of expressing my object in seeking to bring about an alteration in the bye-laws.

There prevails amongst the Fellows generally, especially amongst those living at a distance from London, a feeling that the Council is not, except in name, a representative body, that it is too much self-elective, and is not the deliberate choice of the Fellows at large—in other words, that a dozen or two of men annually choose the Council in derogation of the electoral privileges of 500 and odd distant Fellows. This is a notorious fact, and, as we have seen during the last few years, is very prejudicial alike to the interests of the Society and of the science of astronomy generally.

I had proposed to myself the other day no further task than that of re-modelling the bye-laws which deal with the annual election of the governing body; the change which I contemplated was voting by proxy coupled with a more open system of nominating officers, for the present system of a "house-list" gives rise to interminable jobbery and cliqueism. It was entirely in consequence of a conversation which I had with Lord Lindsay and Mr. Barrow, that I expanded my proposal into a scheme for a general revision of all the bye-laws *en masse*. I fully believe that such general revision is necessary, but I had no desire to open up so large a subject. I may add, I do not intend that my limited scheme of improved election arrangements should be swamped or indefinitely postponed pending the general revision of all the bye-laws. A scheme for altering all may readily be made the pretext for a long postponement of the very small and easily accomplished revision of the election bye-laws, which could be brought about by the alteration of merely a few dozen words in the present bye-laws.

I should be glad if you would publish this note, because I have reason to know that a large number of Fellows living at a distance from London have long been very anxious for proxy-voting, or something like it, to be established as soon as possible.

I am, Sir, your obedient Servant,

Northfield, Eastbourne, February, 1880.

G. F. CHAMBERS.

NOTES.

Harvard College Observatory.—Prof. Pickering's Report for 1879 presents an interesting record of the important work done at this observatory. Thanks to the liberality of the citizens of Boston, who, in response to the Director's appeal, have subscribed a sum of 5000 dollars a year for five years, it has been found practicable to keep both the Meridian Circle and Equatoreal actively employed, and also to proceed with the reduction and publication of the old observations. Amongst the observations with the Equatoreal may be noticed photometric measures of Iapetus made on 101 nights in the autumn and winter of 1878–79, which with similar measures made in the previous year furnish a determination of the law followed by the satellite in its changes of brightness. Also a series of measurements of all planetary nebulæ made with a double-image micrometer, an examination of their spectra, and measures of their light made with Photometer Q, by comparison with the image of a star thrown out of focus by such an amount that it appears of the same intrinsic brightness as the nebula. In this way the light of a nebula may be described in stellar magnitudes, thus adopting the same unit and same scale for nebulæ and stars. Some observations for stellar parallax have been commenced, and several asteroids, as well as Comet ii. 1879, have been observed with the Equatoreal.

As regards the work with the Meridian Circle:—the zone of 8000 stars between 50° and 55° N. declination, undertaken by this observatory, was completed in 1879, Jan. 26, having occupied a large part of the time of the observer, Prof. Rogers, for over eight years, and forming one of the largest astronomical undertakings which have been carried to completion in the United States. The observations of the list of 258 stars for the U. S. Coast Survey were completed in 1879 January, and nearly 300 observations were made of Mr. Gill's list of 10 stars to determine the personal equation between bright and faint stars, and with bright and faint fields, &c. Since February 1879, the Meridian Circle has been devoted to the determination of the absolute places of 108 stars, declination observations of the Sun being made to determine the position of the equinox and the obliquity of the ecliptic. Altogether 3000 meridian observations have been made during the current year. Before beginning the absolute work a few instrumental changes were made, the most important being the substitution of new micrometer-screws for the old ones, which required large corrections, and the provision of a separate arc of 15° graduated to 5′ for determination of the division-errors.

A work of some magnitude has been undertaken, in the determination of the light of all stars visible to the naked eye in the latitude of Cambridge, U. S. To avoid loss of time in identifying the objects, they are observed in the meridian with a photometer which consists of a horizontal telescope pointing to the west and having two objectives. By means of two prisms mounted in front of the telescope, the pole star is reflected into one object-glass, and the star to be measured into the other. The cones of light are made to coincide by a double-image prism, the extra images being cut off by an eye-stop. The star to be measured is thus seen in the same field with the pole star, with the same aperture and magnifying-power. An assistant is required to set the reflecting prism, read the circles, and keep the record. Two persons can thus measure about 40 stars in an hour. Each of the 4000 stars will be observed on three evenings, four settings being made each time, and three observers will take part in the work.

Of the 'Annals,' Vol. xi., containing the results of over 25,000 photometric observations, has been published, and a large part of Vol. xii., which gives the observations with the Meridian Circle in 1874–5, is in type.

MR. WEBB'S PLANETARY NEBULA.—Mr. Dreyer writes that this nebula is identical with No. 27 of M. Stéphan's list in the 'Comptes Rendus' for Dec. 2, 1878. M. Stéphan describes it as:—"Etoile 4004 Arg. Zone +41°, 8·5 mag. Elle est fortement nébuleuse, mais la nébulosité est très-condensée."

THE SOLAR ECLIPSE OF 1880, JAN. 11.—From the 'San Francisco Daily Bulletin' and the 'Chicago Tribune' of Jan. 12 we take the following account of the totality, which was well seen near Fresno at $3^h\ 53^m$ local mean time:—"As the last ray disappeared the corona of clear white light, entirely encircling the Moon, flashed into view. Near the edge of the disk the light was of a brilliant white, which gradually faded towards the outer limit, until, at a distance of about one-tenth the diameter of the Moon, it blended into the leaden hue of the sky. Those provided with good glasses now witnessed a scene never to be forgotten. Extending along the border of the lower left-hand third of the Moon was an irregular fringe of prismatic-coloured light as sparkling and as brilliant as the red and purple fire from an exploding rocket; while at the top of the Moon was seen a bright yellow triangle of light equal in height to one-sixth the diameter of the disk, and at the middle of the right-hand side a similar but smaller protuberance. Projecting from the upper right-hand side was a broad faint ray of light, in shape like an imperfect isosceles triangle, equal in height to the Moon's diameter; while at the lower left-hand side were two similar rays of about the same length, whose combined bases covered one quarter of the Moon's circumference. The gorgeous scene lasted 31 seconds. The corona remained a few seconds after the first flash of the appearing Sun and then disappeared. On turning round and facing the Sierra, the rapidly retreating shadow was distinctly seen sweeping up the slope of the mountains. The central portion was darkest, though the curve which formed its outer limits was sufficiently defined to enable a good judge of distances to roughly estimate its width."

GREAT SOUTHERN COMET.—On Feb. 5, Dr. Gould telegraphed to Prof. Peters at Kiel, "Great Comet passing Sun northwards," and on Feb. 6 sent a second telegram, "Comet moving southwards."

The two telegrams are not absolutely inconsistent, as the comet's motion may have changed from north to south in the interval; but it would seem from the second dispatch that this comet will not be visible in the northern hemisphere. The Emperor of Brazil also announces to the French Academy the discovery of this comet, but no details are given. The following telegram has also been received from Mr. Gill*:—"Comet, twelfth February, 08 [hours] 58 [minutes], Cape [mean time], 123° 11′ [N.P.D.?]; daily motion [in R.A.] plus 235 [minutes of arc], [in N.P.D] minus 20 [minutes]." It would seem that either the R.A. or the N.P.D. has been omitted, and it is uncertain whether this telegram refers to Dr. Gould's comet or not. There are at present two distinct codes for telegrams announcing the discovery of comets, one emanating from the Vienna Academy, the other from the Smithsonian Institution, Washington. The result of the confusion seems to be that such telegrams are practically useless, neither convention being strictly adhered to by those who send them.

In a letter to the Astronomer Royal, dated Cape of Good Hope, Feb. 2, Mr. Gill states, with reference to the comet, that its tail had been seen on Feb. 1, setting in the sea at 8 P.M., by a gentleman on the W. side of Table Mountain; and that on Feb. 2 he himself saw the tail projecting over the S. flank of the mountain, as far as β Gruis, and 10′ W. of that star, parallel nearly to β and δ Gruis.

Two New Minor Planets have been discovered—one of the twelfth magnitude by Herr Palisa, at Pola, on Feb. 6, and the other of the eleventh magnitude by Prof. Peters, at Clinton, New York, on Feb. 16.

Astronomical Memoranda, 1880, *March.*

Sun. March 1, sets $5^h\ 39^m$, rises $18^h\ 44^m$; March 31, sets $6^h\ 30^m$, rises $17^h\ 36^m$.

Equation of time:—Sun *after* Clock, March 1, $12^m\ 26^s$; March 11, $10^m\ 2^s$; March 21, $7^m\ 9^s$; March 31, $4^m\ 5^s$.

Sidereal Time at Mean Noon:—March 1, $22^h\ 38^m{\cdot}7$; March 11, $23^h\ 18^m{\cdot}1$; March 21, $23^h\ 57^m{\cdot}5$; March 31, $0^h\ 36^m{\cdot}9$.

Moon.	*rises.*			*sets.*			*sets.*	
	h	m		h	m		h	m
Mar. 1..	12	5	Mar. 11..	6	57	Mar. 21..	16	1
2..	13	25	12..	8	10	22..	16	20
3..	14	37	13..	9	23	23..	16	39
4..	15	33	14..	10	34	24..	16	56
5..	16	17	15 ..	11	42	25	*rises.*	
6..	16	48	16..	12	45	26..	6	54
7..	17	13	17..	13	42	27..	8	19
8..	17	33	18..	14	27	28..	9	45
9..			19..	15	5	29..	11	10
10..			20..	15	36	30..	12	26
						31..	13	28

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* The words in brackets are not in the telegram, and have been inserted conjecturally.

Last Quarter, March 3, $11^h\ 6^m$; New, March 10, $12^h\ 47^m$; First Quarter, March 18, $12^h\ 36^m$; Full, March 26, $1^h\ 23^m$.

Mercury is an evening star, at greatest elongation (18° 15′ E.) on March 11, when he sets a little N. of W. at $7^h\ 46^m$, $1^h\ 50^m$ after the Sun. He is in inferior conjunction on March 28.

Venus is a morning star, rising about 1^h before the Sun. The illuminated portion of the disk is 0·855 on March 15, and 0·916 on April 15, and the corresponding diameters 12″·1 and 10″·9.

Mar. 1,	R.A. $20^h\ 38^m{\cdot}5$,	Dec. 18° 32′ S.,	tr. $22^h\ 1^m$,	rises $17^h\ 34^m$
Mar. 31,	23 2 ·4,	7 33 S.,	22 26	16 59

Mars is an evening star in Taurus, very near the Moon on March 17.

Mar. 1,	R.A. $4^h\ 22^m{\cdot}6$	Dec. 23° 28′ N.,	tr. $5^h\ 44^m$,	sets $14^h\ 1^m$
Mar. 31,	5 32 ·6	25 4 N.,	4 55	13 24

Jupiter is in conjunction with the Sun on March 15.

Saturn is an evening star in Pisces, approaching conjunction.

Uranus is in Leo.

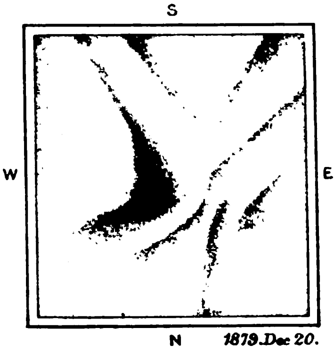
Mar. 1,	R.A. $10^h\ 35^m{\cdot}2$,	Dec. 9° 47′ N.,	tr. $11^h\ 55^m$
Mar. 31,	10 30 ·8,	10 13 N.,	9 52

Phenomena.

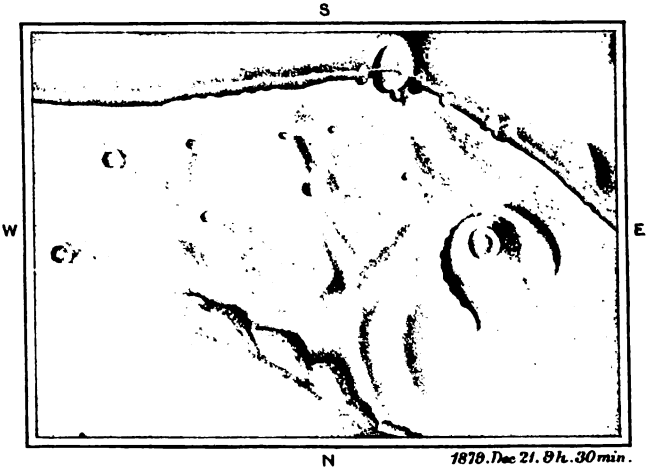
	G. M. T.		
	h	m	
Mar. 13	7	35	101 Piscium Oc. D. 84°.
17	6	13	[1518] Oc. D. 77°.
	12	30	103 Tauri Oc. D. 54°.
	12	35	*Mars* Occultation near approach 175°.
18	6	13	132 Tauri Oc. D. 115°.
Mar. 21	7	16	d^2 Cancri Oc. D. 101°.
24	13	36	p^2 Leonis Oc. D. 10°.
25	11	10	[4006] Oc. D. 20°.
29	11	46	42 Libræ Oc. R. 221°.
	18	41	[5253] Oc. R. 239°.
30	13	6	25 Scorpii Oc. R. 221°.

The angles are reckoned from the *apparent* N. point towards the right of the Moon's inverted image. EDITOR.

Publications received:—Prof. E. C. Pickering, *Annals of Harvard College Observatory*, Vol. xi. Part ii.—*Annual Report, Harvard College Observatory*, 1879 (Cambridge, U.S., 1880)—Ormond Stone, *Cincinnati Observations, Micrometrical Measurements of* 1054 *Double Stars*, 1878–79 (Cincinnati, 1879)—*Annuaire de l'Observatoire Royal de Bruxelles*, 1880 (F. Hayez, Bruxelles)—L. Niesten, *Note sur la Tache Rouge observée sur la planète Jupiter*, 1878–79 (F. Hayez, Bruxelles)—Dr. F. Terby, *Études sur la planète Mars—Tableau synonymique des dénominations données aux Taches* (F. Hayez, Bruxelles)—Prof. E. Loomis, *Contributions to Meteorology*, 12*th paper* (Amer. Journ. 1880, Feb.)—Camille Flammarion, *Astronomie Populaire* (C .Marpon and E. Flammarion, Paris, 1880, 836 pp., Price 10 francs). EDITOR.



HYGINUS. N.



THE REGION NORTH OF HYGINUS.

H. Pratt del. Mintern Bros, imp.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

No. 36. APRIL 1. 1880.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, 1880, March 12.

J. R. Hind, F.R.S., *President*, in the Chair.

Secretaries: W. H. M. Christie, M.A., and
J. W. L. Glaisher, M.A., F.R.S.

The Minutes of the Meeting of January 9 were read and confirmed.

Mr. Christie reported that during the last month 86 presents had been received, including a volume of the 'Philosophical Transactions,' completing the set from 1801 to 1833; Harvard College Observatory, Annals, Vol. xi. Part ii.; Nine Drawings of Mars, by Mr. T. Brewin, and four Drawings of Jupiter, by Mr. Gwilliam. The thanks of the Society were voted to the Donors.

The President announced that the names of 12 Fellows were suspended, as defaulters for sums of 8 guineas or more.

Mr. Glaisher announced a paper by *Mr. Gledhill*, "Phenomena of Jupiter's Satellites observed at Mr. E. Crossley's Observatory, Halifax." Also "Observations of the Satellites of Mars," by *Prof. Asaph Hall*, consisting chiefly of tabular matter. Speaking of Phobos, Prof. Hall writes:—"After Dec. 1 the weather, which had been unusually favourable, became bad, and the image of the planet was so unsteady that no further observations of Phobos were made, though the brighter of the two satellites." Also a paper by *Prof. Pritchard*, "Observations of the exterior Satellites of Mars at the Oxford University Observatory;" accompanied by a letter from Prof. Pritchard, in which he adds:—"But I do think it a loss of time and trial of patience to look for such minute objects in telescopes of 12 to 15 in. aperture."

Lord Lindsay. I rather protest against the idea that a telescope of 12 or 15 in. is of no use.

Mr. Christie read two letters from *Mr. Gill*, giving his observations of the great southern Comet from 1880, Feb. 1 to Feb. 15, and also accounts of the same comet by *Rev. S. S. O. Morris* and *Lieut. Gwynne* on board H.M.S. 'Garnet' at Monte Video. The positions of the comet and its tail, as described by Mr. Gill, were exhibited on a globe, and it was explained that the comet could not have been seen in northern latitudes, as it would set nearly with the Sun, the tail being parallel to the horizon. The tail, as observed by Mr. Gill, must have been a very striking object about Feb. 5, when it was about 90° long, and some 10° or 15° broad. The nucleus was not seen till Feb 8. A curious point was that Lieut. Gwynne, on Feb. 8, saw the tail without any nucleus extending lower down than the position given for the nucleus by Mr. Gill. Lieut. Gwynne describes the tail when first seen, on Feb. 1, as having an altitude of about 20°. Mr. Christie also mentioned that *Mr. Janisch*, Governor of St. Helena, in a letter to him, described the tail as 34° in length and 2° in breadth on Feb. 5, and resembling the remarkable comet of 1843.

Mr. Christie. I may mention that Mr. Gill sent a telegram about this comet, in which there was some doubt as to the interpretation. According to the usual code adopted by the Vienna Academy, the figures which he gave, 0858, followed by the word "Cape," should have meant the Cape mean time of the observation; but it appears now from his observation that they may really have referred to the right ascension of the comet, as expressed in arc, though in that case there ought to have been another "0" before them—there ought to have been five figures. There was a little difficulty in interpreting the telegram in consequence. In the newspaper accounts which were sent from Greenwich the interpretation was adopted that the right ascension was $8^h\ 58^m$; but in telegraphing to observers generally, the actual words telegraphed by Mr. Gill were repeated without any attempt at explanation. The reason for putting such a statement in the newspapers was that a comet at R.A. 8° 58′ would not have been visible in this country, and it seemed useless to announce any thing in the newspapers which could not have been observed. In the telegram the convention had not been strictly adhered to; hence the difficulty.

Lord Lindsay. One interpretation of the telegram went on the assumption that the right ascension had been omitted altogether. I thought that $8^h\ 58^m$ was certainly the Cape mean time, because a letter that I got from Mr. Gill referred to an observation made at $8^h\ 50^m$.

Mr. Christie. It is perfectly ambiguous. According to the observation the time given is 9 hrs., and the right ascension is 9° 15′, and it is quite conceivable that 8° 58′ may have been a rough reduction of his first observation. I mention this as an explanation of the difficulty in which we have been placed.

Lord Lindsay read a newspaper account of the comet which he had received from Mr. Eddie.

Mr. Glaisher read a paper by *Prof. E. S. Holden* on observations of Mimas, and of an occultation of Rhea observed with the Washington 26-inch equatoreal. Also a note by the *Rev. S. J. Johnson* on the Disappearance of Jupiter's Satellites in 1883.

Mr. Knobel. I should like to ask whether the substance of that paper has not appeared somewhere else this week. I have certainly read it, though I cannot remember where.

Mr. Glaisher. It came to the Society as a new paper. I know nothing of it.

Mr. Knobel. It is in the 'Observatory,' I believe.

Mr. Glaisher. I have not read it in the 'Observatory.' It is a new paper so far as I was aware.

Mr. Glaisher read a paper by *Prof. H. C. Vogel*:—"Notes on the Spectrum of Baxendell's new star in Canis minor."

Lord Lindsay. This paper is the outcome of a circular I issued some time ago on Mr. Baxendell's star. I sent one to Prof. Vogel and he sent me a letter shortly afterwards. I think I mentioned at the last Meeting that the spectrum of the star did not show any peculiarities.

On the receipt of this letter from Dr. Vogel we set to work to examine how it was that the peculiarity of the spectrum had been overlooked, and we found that if it had been overlooked, it was owing to the fact that we used too high a dispersive power; we have used prisms of rock-crystal and salt and other kinds, and I have just had this report sent me to-day:—"We have repeatedly observed Baxendell's star with the spectroscope. The spectrum is not interesting, nor were the blue rays absorbed to any extraordinary extent."

Mr. Glaisher read a note from *M. C. Wolf* on Mr. Maxwell Hall's paper on the nebula in the Pleiades, in which he remarked:—"I have completed the observations Mr. Hall was in want of, in a memoir giving a description of the group of the Pleiades, in the 'Annals of the Observatory of Paris,' of which I have sent a copy to the Royal Astronomical Society." Also a "Note on the Error in Hansen's Lunar Tables," by *Mr. Lynn*, supplementary to the paper printed at p. 82 of the present volume of 'Monthly Notices,' 1879.

Mr. Neison read two papers on photographic observations of the Moon, and on the determination of the personal equations of lunar observers:—

Mr. Dunkin, at the January Meeting, read a paper in which he asked me for further information as to the methods by which I obtained the personal equations given in a previous paper, and this note furnishes the information. It shows that in obtaining these personal equations I had not assumed that they remain constant for a greater period than one year at a time. It also shows how it is possible to reduce them to a uniform standard, even when the observers have been changed. Mr. Dunkin also draws attention to the fact that when he comes to compare the personal equations for 1877 and 1878, in the observations, to which I had not access at the time, he finds that the personal equation is very materially altered, and therefore he thinks that the explanation I gave of the fictitious increase in the error of mean longitude of the Moon is not satisfactory. I would only draw attention to the fact that when you compare the fictitious increase which seems to me to have existed in the years 1875–76, and the personal equations as then determined with those determined for subsequent years, they are in perfect accord, both varying together. So that this to a great extent accounts for this rather surprising change, which has been puzzling me as well as many others. It seems to me to be almost entirely due to the changes in the Greenwich observers.

The second paper is merely a short note with regard to a paper by Prof. Pritchard, in which he shows that all the observers at Oxford agree very nearly in their determinations of the Moon's diameter. His method of measuring distances to different points of the limb would not eliminate the irregularities if they were of an undulating character extending over considerable portions of the limb.

Mr. Dunkin. The few figures inserted in my short paper were deduced directly from the tabular errors determined in 1877 and 1878. They show that the method of observing has changed in some of the observers from what it was in 1876; but I cannot see how a change of this kind can be the reason for assuming that the tabular error of the Moon has increased. I think, however, it is scarcely possible to obtain accurate results for personal equation from the few meridional observations of the Moon made by some of the observers in recent years. For this reason I would rather wish that Mr. Neison would wait a little longer, so as to obtain a few more observations, before making any theory upon the subject. I know from experience that personal equations do occasionally change from year to year, and especially during the last ten years. It appears to me, therefore, that it is rather a difficult matter to compare two observers in 1867, and again one of these observers with a new observer in 1875, and then to deduce the relative personal equations of the three observers, as the quantities in question are so small. A second of arc is a very small quantity; and, in order to get the truth, I should be glad to have not 15 observations, which is the average number of each observer in one year, but more likely 50 or 60 before I should expect to obtain a correct result.

Mr. Christie. As to the point Mr. Dunkin has been referring to, I may cite a remark made by Le Verrier, which was, that what was required in the present state of astronomy was not to multiply observations, but to multiply observers and observatories; that the chief error in the solar, planetary, and lunar tables came from the personal errors of observers and the variations in them, and from systematic errors of observatories.

With regard to the question of the Moon's semidiameter, which has been at issue between Prof. Pritchard and Mr. Neison, I do not wish to enter into that. The only point that strikes me, with regard to the irregularities of the limb, is that, as far as I remember, Professor Pritchard's method was to measure distances to the Moon's limb at intervals of 60°, or something like that, which must be supposed to eliminate those irregularities. I am not very clear about the matter, because it has not been explained in detail.

There is one other point, raised on a former occasion, which may be worth mentioning—that is, as to the true value of Hansen's semidiameter. As a matter of fact, I find that the value adopted by Prof. Pritchard was also adopted by the Astronomer Royal, and on very good grounds, because it depends on the adopted mean parallax. The Moon's mean parallax is a purely conventional thing. It depends upon what long-period terms you include. Hansen included long-period terms depending upon Venus, whilst other theorists have excluded them. The process adopted by the Astronomer Royal was to compare Prof. Adams's values of the parallax throughout 1862 with those used by M. Hansen for the same period. The two sets of values agreed within 0″·1; and yet the values of the mean parallax adopted by Prof. Adams and by M. Hansen respectively differ by 2″·3, simply because they attached a different interpretation to the definition of mean parallax. It is not a thing which exists in nature at all. What exists in nature is the parallax of the Moon at any given time; but the mean parallax depends upon the theory which one adopts. The mean diameter of the Moon must depend upon the mean parallax which we adopt by our definition. Supposing we have got the true diameter of the Moon at a given time, to get the mean diameter we must compare the parallax at the given time with the assumed mean parallax.

Mr. Neison. I think if Mr. Dunkin reads my paper he will see that all the objections he has mentioned will be removed. He will find the personal equation, which he thinks so variable, coming out year after year with differences less than 10 or 12 per cent. As to what Mr. Christie said about parallax, I confess I do not understand it. First we are told that Hansen's mean parallax is not the mean parallax adopted by Hansen, but ought to be the mean parallax adopted by somebody else; and when he comes to tell us how Hansen arrived at his parallax, I cannot reconcile that with Hansen's own tables, in which he says especially that he includes all quantities which are periodical, including terms of long period, and only excludes the secular terms due to the action of the planet upon the excentricity &c. of the terrestrial orbit. One cannot imagine for a moment that we are at liberty to adopt somebody else's parallax and then clap it on to Hansen's tables, and say that it is Hansen's parallax. As a matter of fact, if you take the assumptions which are embodied in Hansen's tables, it is no matter whether you get your theoretical result from Delaunay, or from Adams, or from Hansen. It has been shown that in these cases the value of the lunar parallax would vary by an almost infinitesimal quantity. If you choose to imagine a smaller terrestrial diameter in one case than in another, you may get a variation in the amount of apparent parallax; but, assuming the same data, the parallax will come out the same in all cases.

Lord Lindsay read a paper by Dr. Copeland on the phenomenon known as "shadow-bands."

Mr. Ranyard. I should like to make a remark about one interesting observation mentioned in that paper. It appears that Dr. Copeland has observed the shadow-bands when the disk of the Sun was only partially hidden, I think, one third eclipsed by a hill. During eclipses shadow-bands have been seen only just before totality, usually only a few seconds before the solar crescent disappears—that is, when the light of the Sun is greatly diminished. I rather incline to the opinion that the scintillating light which causes the shadow-bands is derived from the bright clouds upon the horizon rather than from the solar crescent. For there are accounts by some four or five observers, who assert that they have observed the bands running over the earth during totality. And we know that the light of the corona and prominences is not strong enough to cast a shadow. That is to say, the light derived from the clouds outside the zone of totality (which causes the heavens all round to have a sunset appearance) is sufficiently strong to overpower the light of the corona and prominences. The light derived from clouds upon the horizon would, like the light of the setting sun observed by Dr. Copeland, have to pass through a long distance of the lower part of the earth's atmosphere, in which there appears to be much greater variations of density than at higher altitudes. If we do not accept this theory, we must assume that just about the time of totality, a violent disturbance is set up in the atmosphere, which would cause a very noticeable scintillation of objects at the Sun's altitude.

Lord Lindsay. I had a conversation some time ago with Prof. Respighi at Rome, and I think he mentioned to me that the effect of scintillation was that he got a series of bands crossing the spectrum of the star. I dare say Mr. Christie knows something of those observations, which might possibly be something of the character of those referred to. The stars were very low down, and looked at through a great thickness of atmosphere.

Mr. Ranyard. These are not absorption bands in a spectrum, but shadows.

Mr. Knobel. I think you will find them figured in the eclipse volume.

Mr. Ranyard. Yes, you will.

Mr. Christie. The phenomenon of scintillation to which Lord Lindsay referred appears to be caused by the total reflection of some part of the spectrum from the varying densities in different strata of the atmosphere, so that some of the rays would be reflected, whilst other rays differently refracted escape total reflection. It is some time since I looked up the literature of the subject, but I think that was what was put forward as the explanation by M. Respighi and generally accepted.

Mr. Glaisher read a paper by Mr. J. Tebbutt on the longitude of his Observatory, Windsor, New South Wales.

Mr. Green read a paper on some Changes in the Markings of Mars, and exhibited a large drawing of Mars in elucidation of his paper.

In the course of reading his paper he observed:—We have present Mr. Brett, with whom I can shake hands as a fellow artist; and if Mr. De la Rue were here I could appeal to him on this point, that one of the most difficult things for a young student of art is to learn to see the very delicate and faint tones which form the highest modelling of the surface. They will see the high light on the deep dark very definitely; but when they are called upon to depict the most delicate shades, they either do not see them or they misrepresent them. It is possible that some of those delicate lines which have been drawn by various observers are in reality the edges of tones, the remainder of which they have failed to see.

Mr. Knobel. I should like to ask Mr. Green a question with regard to that portion of the map of Mars near the Kaiser Sea. In the drawing by Jacob made at Madras, there is a distinct band connecting two seas, and there is the same band to be found in some drawings published by M. Terby in his 'Aréographie;' and also the same bands were noticed and drawn by myself in 1873. In 1873 Mr. Green mentioned this particular faint band here, which, although I examined this portion of the planet very carefully, I could not see at all; and the three bands of which I have spoken, and which have been drawn by Jacob, Mr. Green did not see. I should like to ask Mr. Green whether he has any notes on this portion of the planet which may lead him to give credit to other observers for having observed these bands. I think myself that that is a portion of Mr. Green's map which will require modification.

Mr. Green. I can credit any body's observations, I believe, that there are constant changes of these delicate markings over the surface on different evenings. I myself depicted those which I saw; but a great many of these delicate markings and streaks come and go. I made drawings at Madeira of what I considered atmospheric lines; I have seen lines radiating over a large continent, but I have not drawn them, because I considered them to be atmospheric. I have not the slightest doubt about lines having been seen by Mr. Knobel and others, only I did not see them. Whether they will be seen again when the planet is sufficiently tilted up to enable us to see the north pole I do not know; but to enter upon the maps any markings but those which have been repeated over and over again, would bring us into considerable confusion.

Mr. Knobel. These were not faint markings, they were good bold markings, visible on three or four of my drawings, and they were visible the whole time whilst the planet was visible.

Mr. Green. We must hope that we shall see them again. I will try to very earnestly.

Mr. Christie. With much of what Mr. Green has said I should be willing to agree; but there are one or two points which I should like to allude to, merely as showing the difficulties connected with the study of Mars. It seems to me a most difficult planet to make any thing out of. I agree with Mr. Green that one great difficulty is to determine what really belongs to Mars, and what belongs to our atmosphere. I examined one of the markings (the Maraldi Sea) at the last opposition with great care for about an hour or so, and I had very great difficulty in satisfying myself whether there was a break through it, or whether it was merely a tremor caused by our atmosphere. After careful watching I came to the conclusion that there really was a break right through; and when I came to refer to a drawing which I made of the same phase quite independently in 1877, I found that the same break was shown clean through. As regards the bright spot or "island," which Mr. Green points out as having been seen in 1879, but not in 1877, I may remark that in 1877 I saw two bright spots or "islands" there, and I recorded them in a drawing. So that, as far as the establishment of a change in Mars is concerned, I certainly should be rather inclined on that particular point to dispute it. But I should be very sorry to put forward that drawing as deciding the question by any means; I only mention this to show the difficulties attendant on this point, some caused by our own atmosphere, and others caused by the difficulty of drawing things which we do not see clearly.

Mr. Green. The changes that I speak of I do not suppose for a moment to be actual changes in the planet, but changes in the appearance of the planet, and doubtless in a great measure due to its atmosphere.

Mr. Christie. I may mention that *Mr. T. Brewin* has sent a series of nine drawings of Mars, which have been handed round to the Meeting.

Mr. Ranyard was called upon to read a paper on his observations of the Eclipse of 1878; he said:—I ought to apologize for not having given an account of these observations before; but I had undertaken to write an account of other peoples' observations, and I thought I had better finish what I had undertaken first. In the spring of 1878 I wrote to Professor Young asking advice as to the best station to go to, and in his answer he kindly invited me to join him. He said he was going to encamp near Denver, and that that would be a very easy place to get my instruments to; in fact, he offered me the hospitality of his station. I gladly accepted his invitation, and took with me a large camera of 13 in. aperture and 6 ft. 2 in. focal length. It was rather a problem to me what mounting I should take; the German mounting with counterpoise and clock would make a very large instrument, not only to take to America, but all across the continent. Ultimately I determined to make the case in which the camera was carried into an equatoreal mounting. The camera was a long box of deal, and would be about 7 feet long; it was necessarily packed in rather a stroug box. The two sides were made of very thick elm boards, and the camera was pivoted into the sides of the box, so that it could be swung out of the box and the box itself tilted up for the polar axis, and moved by means of the clockwork motion of an ordinary equatoreal mounting so as to follow the Sun. On the top of the camera was a small spectroscopic camera with a single prism of 60°, lent me by Prof. Young. This was used without any slit, and the exposure was made just before the commencement of totality, in the hope that a series of images of the lower chromosphere, frequently spoken of as Young's bright-line stratum, might be obtained; but, although only one prism of 60° was used, no trace of the spectrum of Young's stratum is to be seen upon the plate. The same plate was exposed during the whole of the totality; and there is an image of the continuous spectrum of the corona which extends from a little below G for about one fourth of the length of the spectrum of the solar crescent visible after totality, but no separate images of the corona are visible corresponding to the bright lines which have been observed in the spectrum of the corona. With the larger camera, two photographs of the corona were obtained, but, owing to an accident by which the camera was pushed from off the Sun's place, no further photographs were obtained with it.

While the friends who had volunteered to assist me were making the exposures with the large camera, I was observing the polarization of the corona with a small 3-in. telescope and a biquartz. I found that the polarization of the corona could be detected by strongly marked colours quite down to the Moon's limb, both in the area of the polar rifts and within the brighter equatoreal structure of the corona; and I succeeded in tracing the radial polarization of the corona to a distance of a solar diameter and a half from the Moon's limb in the eastern equatoreal regions. How far it may have extended to in the polar regions I do not know. The coronal extension in the equatoreal regions was very large compared with the extension in the polar regions, as seems to have been very frequently the case during eclipses which have happened when there have been few spots upon the Sun. But no groups of synclinal structure are visible—such as are referred to in the Eclipse Volume as having been seen in so many coronas of the Sun-spot maximum type.

Capt. Abney. What is the focal length of the lens that was used in the camera?

Mr. Ranyard. The lens was of 13 inches aperture, and 6 feet 2 inches focal length.

Lord Lindsay. The lens is one made by Ross, which was exhibited a long time ago.

Capt. Abney. I want to know the ratio of the aperture to the focal length of the camera.

Mr. Ranyard. There have been reflecting telescopes made of much shorter focal length; but I thought it unwise to use a reflector, because there seems to be so much scattered light from a reflector.

The corona is of very different brightnesses at different parts. The lower part is very bright compared with the upper part.

A Fellow. Were the photographs taken with dry plates?

Mr. Ranyard. Some of them were dry plates—Mawson and Swan's.

Mr. Glaisher read a description of a new uniform pressure clock by *Mr. T. Buckney*. The clock movement and pendulum were each enclosed in partially exhausted bell-glasses respectively above and below a central base-plate. The clock was wound by electricity, contact being made every second.

Lord Lindsay. So far as I understand the construction, it is not a new one. It was introduced by Thiele of Berlin. With a certain amount of courage I ordered one from him of that description in 1873, but it has never been delivered yet.

Mr. Glaisher. At the end Mr. Buckney makes this remark:—"I may state in conclusion, in order to avoid the charge of appropriating to myself and taking credit for the invention of others, that the endless chain was invented by a Dutch philosopher, Huyghens. The method of winding is the only thing that the author claims as new."

Lord Lindsay. The method of winding that it was proposed to adopt was by electrical contact.

Mr. Christie. I have a paper on the mean heliographic latitude of Sun-spots for the years 1874 to 1879, from photographs taken at the Royal Observatory, Greenwich. A paper was sent a little time ago, giving the mean areas of Sun-spots for the years 1873 to 1879. This is an extension of the paper giving the mean heliographic latitude. Dr. R. Wolf and others, I believe, have pointed out that the mean heliographic latitudes, north and south of the equator, are larger at the maximum of Sun-spots and smaller at the minimum. The Greenwich photographs show that the outburst of spots which commenced in the latter half of last year was accompanied by an increase in the mean distance of spots from the Sun's equator, thus supporting Dr. R. Wolf's conclusion. I may also mention to the meeting that we have been taking lately at Greenwich some photographs of the spectrum of Sun-spots, and from those we have got some enlargements. They are rather in the nature of experiments at present; but it may interest some of the Fellows to look at them and see how certain lines are broadened over a Sun-spot.

Mr. Brett. About two months ago a question was raised before this Society by Mr. Marth, and we were invited to observe the comparative brightness of Mars and Aldebaran during the month of February, with the view of marking the date at which their brightness was considered equal. We have come now to the end of February, and I thought it might be desirable to mention that up to the 28th of February, Mars, according to my judgment, maintained a decided predominance in brightness over Aldebaran. On the 4th of March, which was the next opportunity of observation, this condition was considered at the first moment to be reversed, and Aldebaran appeared the brighter; but on further attention I came to the conclusion that they might be considered equal on that day. The difference in the quality of their light was so decided that it would be impossible to compare them very accurately, Mars appearing to be shining with a comparatively well-defined light, whereas Aldebaran appeared as a point with a good many rays of great brilliance. The temperature was very even on both nights, the wind westerly, and the definition remarkably good. The conclusion which I arrived at was that up to the 28th of February, Mars was the brighter, but on March 4th they might be considered equal.

Mr. Chambers. May I ask what has become of the paper by Mr. Burnham on the Bedford Catalogue?

Lord Lindsay. Would you allow me to make a remark upon what has fallen from Mr. Brett? That subject has been attended to at my Observatory, and I got a report dated March 10th, saying that Mars still remains brighter than α Orionis, but the light of that star does not seem to exceed that of Aldebaran as much as usual. We had photometric measures on the 3rd, which will be reduced as soon as Mars at all declines to the brightness of α Orionis.

The President. As to Mr. Burnham's paper, I may say that the paper has been referred to a member of Council.

The Meeting then proceeded to the ballot, and the following gentlemen were elected Fellows of the Society:—

Rev. C. S. Harris, M.A., and Rev. J. J. M. Perry, M.A.

The Meeting adjourned at ten o'clock.

Klein's supposed new Crater near Hyginus.

SOME of your readers may remember that in my article on this subject in the 'Observatory' for Jan. 1879, I endeavoured to show, from the observations of Dr. Klein and others, my reasons for doubting the reality of the crater character which Dr. Klein had attributed to the subject of his observations, and that I also indicated the probability (as it seemed to me) that its true nature would be found, after more thorough examination, to be not that of a crater, but a shallow depression, perhaps of a complicated form.

And writing now, after a lapse of fourteen months, I can only say that, although acquainted with all the observations of this object which have been published, yet I am unable to find any reason to alter the opinion then expressed; and will give, as shortly as may be, an outline of the difficulties which seem to prevent the acceptance of "Hyginus N" as a deep black crater.

On 1878, Dec. 31 (Moon's age $7^{d}\cdot8$), with a power of 270 on my 8·15-in. speculum, I observed a shallow and very irregular depression with shaded and bright sides, including in the deepest shade what appeared to be the shadow of a low hill.

Again on 1879, Jan. 4 and Mar. 29, I made similar observations.

On 1879, Dec. 5 (Moon's age $22^{d}\cdot0$), Mr. J. Rand Capron, using a power of 500 on his 6-in. Cooke equatoreal, thus described his own observations:—"I naturally looked for a crater-like object, but instead of that saw clearly and distinctly what struck me at once as more like a round hill than a crater. It had no defined margin and appeared gradually sloping on all sides into the plain in which it stood. I thought I once caught sight of a very small dark spot or patch of shade between it and the deep shadow of the Spiral mountain."

On 1879, Dec. 20 (Moon's age $7^{d}\cdot3$), definition being good, I again examined the object myself, the illumination of the region being suitable. With a power of 120 I saw a dusky spot in the locality of Klein's object on the western edge of a very irregular shading. The spot could scarcely be called round. With a smaller aperture it might be seen as a dark spot in a sort of penumbra. Suspecting that its true character was not fully revealed, I changed the power to 270, when the irregular shading appeared as a shallow valley skirting the gently curved rise which forms a sort of outer rampart to the Spiral mountain on its N.W. This irregular shading was complicated by the shaded margin of a curved and very slight elevation, which is the position of a hill in my drawing in the 'Observatory' for Jan. 1879. And in this curved shading is a darker spot, neither round nor oval, concave on its western, and convex on its eastern side; in fact the true shadow of a higher elevation rising from the mound. It would have required much imagination to have supposed the dark spot to be a deep black pit, and it seemed strange that such a description of it could ever have been entertained. Prolonged and careful scrutiny yielded no other view, but rendered it certain that no other dark spot was visible near. The darker spot or true shadow was of the size of Hyginus *b*, not larger (Plate V.).

On 1879, Dec. 21, 9^{h} (Moon's age $8^{d}\cdot4$), the region being presented in less exaggerated relief, a more favourable opportunity occurred for seeing the true form of this mysterious object. Definition being exceedingly fine, I used a power of 450 with perfect sharpness of the image. The view of the previous night was confirmed and much more detail seen. The larger drawing, reproduced on Plate V., was then made, and renders description unnecessary. The intensity and size of the shadow which still occupied the place of Klein's crater was found to be rather less than that of the little hill on the south side of the eastern arm of the Hyginus cleft. No other object compared so well.

On 1879, Dec. 22, 6^{h} (Moon's age $9^{d}\cdot3$), Mr. Capron searched carefully, but found no crater visible.

On Dec. 23, 9^{h} to 11^{h} (Moon's age $10^{d}\cdot4$). Definition good and power 270. I found *the minute craterlets* in the region *still exhibited their true character*; but if Klein's object be a crater three miles across, *why could no trace of it be seen?*

On Dec. 26, 6^{h} 40^{m} (Moon's age $13^{d}\cdot3$), Mr. Capron again examined the region; and of course the solar altitude at the spot was too great to expect to see Klein's object, but he observed a white spot very nearly in its place.

On 1880, Jan. 19, 4^{h} 30^{m} (Moon's age $7^{d}\cdot7$), Mr. Capron observed in the place, or close to it, "a faint dull spot with a very faint light border." Between this object and the Spiral he "could trace a ridge system of no great elevation culminating in a bright point or spot, which seemed to occupy the position of the small dark spot" seen in his last observation. "Round the position of Klein's object he "had a strong suspicion of some circular bright ridges."

On 1880, Jan. 20, 5^{h} (Moon's age $8^{d}\cdot8$), Mr. Capron records "Klein seen as a very faint greyish-black spot." He was "impressed with the idea that it was an elevation with light ring round, and central light spot. The greater part of the shadow was *from* the Sun, but there was a smaller shadow on the opposite side. Definition was bad."

On 1880, Jan. 19, 6^{h} to 6^{h} 30^{m} (Moon's age $7^{d}\cdot8$), I found definition fair but unsteady; and having tried many powers on the object and simply confirmed my observation of 1879, Dec. 29, I had myself a good experience of the difficulties which have prevented all observers from detecting the real nature of this curious problem. With powers up to 180 the unsteady definition caused it to look like a dark blotch in a penumbra; and any one who had never had such a view as I was so fortunate as to secure on Dec. 20, would be likely to suppose it to be a crater. To myself that was impossible. With 220 and 270 the dark spot and irregular penumbra-like shading was again resolved, with the same result as that of the previous month, although with more difficulty; and it seemed a power of 450 on an aperture of 8 in. was needed to discover what the formation really is. The size of the shadow was about the same as that of the hill on the southern side of the eastern Hyginus cleft; and although at times it seemed a trifle larger, the unsteadiness of definition accounted for that. This night's experiences showed clearly how any one may have been misled unless he observed with as large an aperture, as high a power, and as exceptionally fine definition, *all combined*, as I have been favoured with myself.

Having closely studied the lunar features for many years, it would be a pleasure to me to know of one instance which could be adduced as unmistakable evidence of present activity in the case of an object so large as "Hyginus N." The difficulties in the way of accurately determining the nature of an observation are often great, because of the deceptive appearances which certain formations assume. The best of observers are liable to mistakes from this cause; and for thus opposing Dr. Klein's interpretation of this object I have but one simple reason—that of arriving at a true knowledge of what it really is. I have before me tabulations of all the observations of it which have been published in this country, some published abroad, and some unpublished; and, as far as I am able to estimate the value of evidence, it seems very unlikely that it is (after all that has been written of it) really a crater. The preponderance of testimony seems to me decidedly negative.

In determining its real nature, I submit that the recent observations of Mr. Capron and myself deserve some consideration, and that the observations of others, if carefully compared with them, will be found to harmonize more completely than they do with those of Dr. Klein. The small drawing, reproduced on Plate V., will better illustrate the immediate neighbourhood of "Hyginus N" as I have seen it at sunrise than many lines of description. It will be found that the formation is one peculiarly likely to afford deceptive appearances. The long valley which runs from Hyginus past the Spiral mountain, and ends at the foot of the bright ridge of mountains, is joined at the place of "Hyginus N" by a small valley which curves round a hill, and the junction is for a little distance only partial, from the intervention of a slightly curved and decreasing bank which juts out northwards from a small elevation formed at the junction of the more elevated portions of the boundaries of the two valleys.

From these peculiar features may, I think, be derived a harmony of all the observations, including those of Dr. Klein.

(*a*) It will be seen from the small drawing that when the Sun has just risen on the region the shadow of the rounded hill will fill the small valley as far as the curved bank, and give rise to "*a black spot much like a crater.*" The illusion will be enhanced by the curved bank supplying a lighter margin partly surrounding the black spot, and in conjunction with the hill itself on the opposite side will produce an appearance which may easily be supposed to be a crater, if the state of definition will not allow of a high power. As instances, see some of the notes of Messrs. Klein, Neison, Green, and Common.

(*b*) When the Sun has attained a slightly greater altitude the shadow of the hill will be lessened, exhibiting "a black spot" still, if seen under otherwise similar circumstances, and the object will be thought "crater-like, but yet not decisively so," and the black spot will be surrounded by a nebulous border. For examples compare some notes of Messrs. Baxendell, Knott, Penrose, Neison, Green, and Edgcumbe.

(*c*) In a case exactly like the last, but with the finest definition and a power of about 500, the object will be seen not perfectly round, but like the shadow of a hill, and the nebulous border will be resolved on one side into the curved bank, and on the other into the shaded side of the hill itself, the hill being recognized as such, as in some of the observations of Mr. Capron and myself.

(*d*) With a still slightly increased solar altitude, the blackness of the shadow being diminished, if observed under definition of only fair quality and with only moderate power, the object will appear as "a white hill with a dark spur S.W.," as described by

M. Gaudibert, or as "a lightish grey crater-like spot" as seen by Mr. Durrad; while under the finest definition and high power its more complicated character will still be visible, as in some of my own observations.

These remarks all apply to the lunar morning, when "Hyginus N" is usually seen. At the time of lunar evening there has been no occasion, so far as I know, on which it has presented a crater-like appearance—a circumstance which is somewhat remarkable, and which of itself detracts considerably from the probability of its crater character, as indicated in my previous paper. A reference once more to the small drawing will explain why it does not exhibit that form at sunset, and also why the hill and its shadow are then more easily recognized.

If all the published notes of various observers be carefully compared, it will be found that if "Hyginus N" is really a crater a very considerable contradiction exists between them; and that if it be the peculiar formation which I have described, that apparent contradiction becomes, I think, harmonized and explained—a circumstance which appears to me to add some weight to the hypothesis which I have advanced; and if the region be closely studied under the finest definition and with powers of about 500, I think the Moon itself will establish the accuracy of my explanation.

At the suggestion of Mr. Capron I have made a model of the region from my own observations. When this is suitably illuminated, and seen at a distance of several feet through a small telescope of low power accurately focused, the true form of the model is of course apparent. But a very slight alteration of focus destroys the sharpness of the shadow of the hill, and that, in conjunction with the peculiar form of the valley, produces a dark nebulous spot with a black nucleus of uncertain form, the whole surrounded by a lighter border. This, it will be noted, is the aspect of "Hyginus N" which is most often seen, and the cause of which is, as I think, a combination of insufficient definition and power. A similar result is obtained by observing the image of the model on the finely ground screen of a camera having a Dallmeyer lens. Of course I do not offer this as proof of the nature of "Hyginus N," but merely as in some degree supporting what I have stated above.

18 Preston St., Brighton. HENRY PRATT.

An Appeal to Amateur Astronomers possessing Telescopes of Large Aperture.

It is well known that many telescopes of very considerable aperture are now in the hands of amateur astronomers, and it is not an uncommon complaint with these gentlemen that no one will take the trouble to point out in what way they can do useful astronomical work. I hope, therefore, that I shall not be considered obtrusive if I venture to ask those who are so fortunate as to possess large reflectors or refractors to give a little of their time, either occasionally or regularly, to the examination of the objects described below.

Double-star observers who have the misfortune to suffer from a bad climate, or whose instruments are not sufficiently powerful, often waste a large amount of valuable time in attempts to see and measure such stars. The difficulty may arise from the extreme faintness, or the variability or the closeness of the companion; and it is to the second of these causes that attention is here more especially drawn. If the period of the variable were known, even approximately, much valuable time might be saved. With the cases of extreme closeness and extreme minuteness of the companion, larger instruments alone can successfully deal. In addition to these, the interesting phenomena of occultations and the supposed instances of 'missing' or 'lost stars' may be named. An examination of the subjoined list will show the nature of the observations here solicited, and also the instrumental and other requisites necessary for the work. In the case of 'missing' stars and variables, the entry will be simply 'seen,' with an estimate of the magnitude, or 'not seen.' Such expressions as 'seen with difficulty,' 'seen with ease,' 'seen steadily,' will also be useful. In the case of occultations, the form of entry may be, 'not round—elongated in such and such a direction,' or 'quite round.' In all cases a few words descriptive of the atmospheric condition should be added, together with the aperture and power employed. If the stars be always observed on or very near the meridian, all the better; if not, the rough hour-angle should be given.

The instruments *necessary* for this work will obviously be equatoreally mounted reflectors or refractors of not less than 8 in. aperture, and a sidereal clock or a common clock or watch set to sidereal time nightly. A micrometer will not be necessary, but a rough one would add to the value of the results in certain cases. The other conditions of success will be a good eye for minute points of light, a good sky, some practice in the estimation of stellar magnitudes, and much patience.

Any results which observers may obtain in such an examination of the list given below (or of any other difficult pairs) would, I am sure, be received by Mr. Burnham, or any other of our leading double-star observers, with much pleasure. And I may add that if the skies which we have experienced had been such as to lead us to think that such an undertaking could be successfully prosecuted here, this appeal to other observers would not have been made. When, however, we are not able to see the 5th star of the trapezium of Orion really well more than two or three times, and the 6th very seldom indeed during an apparition, further proofs of the great difficulties we have to contend with at Bermerside need not be given.

The places of the stars are for 1880. β = Burnham.

Σ 95. R.A. 1^h 4^m·4, Dec. −5° 36′. Mag. A 8·5, B 9·7.

De could not see the companion in 1864–5, nor could β in 1873. In 1874 β with the 6-inch saw it readily; distance 14″·18.

Polaris, Σ 93. R.A. 1^h 13^m·7, Dec. 88° 40″.

Small stars have been suspected about 4″ distant. β, after several examinations, is pretty sure the suspected stars do not exist.

Σ 293. R.A. 2^h 35^m·5, Dec. 56° 33′. Mag. A 8·5, B 11·7.

De in 1867·6 failed to see the companion, but saw it in 1866. Gl and β have seen it since; distance 8″·3.

Σ 366 *rej.* R.A. 3^h 8^m·2, Dec. 22° 30′. Mag. A 7, B 10, C 11.

Σ suspected the duplicity of the companion, but finally rejected it. β measured it in 1877 and 1878; distance 1″·86.

OΣ 61 *rej.* R.A. 3^h 36^m·3, Dec. 7° 31′. Mag. A 7·2, B 10.

An elongation of A and a small star at 1″·2 distance were suspected by OΣ in 1842; it was rejected in 1850 by OΣ. The small star was readily seen and measured by De in 1867; β measured it in 1877; distance 1″·87.

Σ 453. R.A. 3^h 42^m, Dec. 23° 41′. Mag. A 5, B 8.

This is the well-known star *Atlas Pleiadum*: seen double by Σ in 1830; distance 0″·79. Dawes, Secchi, De, β, and others have always found it single.

OΣ 77. R.A. 4^h 8^m·2, Dec. 31° 24′. Mag. A 7·8, B 7·8.

β strongly suspects the distant companion (distance 56″·5) to have an exceedingly minute attendant; distance about 0″·7.

Σ 547. R.A. 4^h 19^m·8, Dec. −1° 40′. Mag. A 8·5, B 11·5.

Measured by Σ in 1831, seen by Mädler in 1845; invisible to De in 1865, and to β in 1873 and 1876. Seen and measured by β in 1877, and by Gl in 1880; distance 2″·25.

Σ 609. R.A. 4^h 43^m·6, Dec. 0° 59′. Mag. A 8½, B 8·7.

Single to De in 1865, and β in 1874. Seen and measured by β in 1878; distance 2″·7.

Σ 607. R.A. 4^h 45^m·8, Dec. 25° 14′. Mag. A 9, B 10·8.

De failed to see the companion in 1866, and β in 1873. Measured by β in 1877; distance 14″·1.

OΣ 97. R.A. 4^h 59^m·3, Dec. 22° 56′. Mag. A 6, B 7·8.

Elongated by OΣ in 1842; single to Mädler in 1844, and elongated by him in 1846, 1848, and 1849; measured by OΣ in 1852; elongated by De in 1866, but single in 1869.

β Orionis. R.A. $5^h\ 8^m{\cdot}9$, Dec. −8° 20′. Mag. A 1, B 9.

Both Mr. Burnham and Mr. Sadler have been led to suspect an elongation in the 9th mag. companion of Rigel. The former observer has devoted much time to it since 1871, with both the 6-inch and 18½-inch object-glasses; and in 1877 this most persistent scrutiny was rewarded by the conviction that it is really a double star: a small elongation was obtained; "the true central distance is certainly less than 0″·2;" and Mr. Burnham adds, "it is the closest, and perhaps most difficult pair of this class I have ever seen."

θ′ Orionis. R.A. $5^h\ 29^m{\cdot}4$, Dec. −5° 28′.

The 5th and 6th stars have been suspected of variability. Other minute stars are said to have been seen within and near the trapezium.

OΣ 160. R.A. $6^h\ 47^m{\cdot}2$, Dec. 21° 19′. Mag. A 7, B 10.

De failed to see the companion in 1867, and β in 1876. Measured by β in 1878; distance 1″·43.

OΣ 162 *rej.* R.A. $6^h\ 52^m{\cdot}9$, Dec. 16° 7′. Mag. A 7, B 10.

Seen by OΣ in 1841, and rejected by him in 1850. De failed in 1866, and β in 1875. Measured by β in 1878; distance 20″·98.

OΣ 165. R.A. $7^h\ 1^m{\cdot}5$, Dec. 16° 9′. Mag. A 5, B 10–11.

In rapid motion. De has never seen the companion, owing no doubt to its faintness; nor could β glimpse it in 1875. β measured it in 1878; distance 2″·45.

Σ 1058. R.A. $7^h\ 10^m$, Dec. 9° 37′. Mag. A 8½, B 11.

De failed to see the companion in 1865, and β in 1874–5 and 1878, after a very careful search. In 1879 β measured it; distance 22″·32.

Pollux. R.A. $7^h\ 38^m$, Dec. 28° 18′. Mag. A 2, C 12½.

The small star C (angle 90°·4; distance 205″·5) was not seen by Sir W. Herschel nor by South; Da and Smyth saw it, but made no measures; Flammarion measured it in 1877, as above. According to Smyth it is as bright as B (angle 72°·1, distance 175″·0, mag. 11).

Σ 1143. R.A. $7^h\ 41^m{\cdot}7$, Dec. 5° 42′. Mag. A 7, B 11.

Discovered by Σ in 1825; the companion was never again seen by Σ; De failed in 1863–4, and β in 1874–6.

OΣ 185. R.A. $7^h\ 51^m{\cdot}1$, Dec. 1° 28′. Mag. A 6·8, B 7.

Elongated or measured by OΣ from 1844 to 1869, but found single by him in 1870 and 1873. Probably an occultation.

Σ 3123. R.A. $12^{h}\ 0^{m}$, Dec. 69° 22′. Mag. A 6·5, B 6·5.

Discovered by Σ in 1832; distance 0″·3. It has been single since 1862. Probably an occultation.

OΣ 255 *rej.* R.A. $12^{h}\ 40^{m}$·1, Dec. 3° 7′. Mag. A 7, B 12.

Not seen by De in 1866. Measured by β in 1878; distance 20″·2.

Σ 3081. R.A. $13^{h}\ 38^{m}$·8, Dec. −11° 14′. Mag. A 8·8, B 9·2.

De failed to see the companion in 1865. β measured it in 1878; distance 2″·02.

Σ 1801. R.A. 13 59^{m}·7, Dec. 6° 32′. Mag. A 9, B 10½.

Not seen by De in 1865. Measured by β in 1878; distance 19″·37.

Σ 3124. R.A. $14^{h}\ 11^{m}$·9, Dec. 51° 55′. Mag. A 4½, B 4½, C 8.

AB form a very close double star, discovered by Σ in 1836; it has never been separated by any instrument: distance of C 38″.

Σ 2062. R.A. $16^{h}\ 28^{m}$·7, Dec. 8° 56′. Mag. A 8·3, B 11.

De could not see it in 1865; β saw it in April 1874, but it was invisible on a very fine night in June of that year; he saw it well in May 1875: distance 2″·8.

OΣ 333. R.A. $17^{h}\ 31^{m}$·2, Dec. 10° 39′. Mag. 7.

De found it single in 1865; β has seen no trace of the companion. On the other hand, Mädler has a measure in 1845; distance 0″·2.

72 Ophiuchi. R.A. $18^{h}\ 1^{m}$·7, Dec. 9° 33′. Mag. A 4, B 14.

AB is OΣ 342; but OΣ has several times measured a companion; distance 1″·5. Newcomb, Hall, and Burnham have all failed to see this star.

OΣ 365. R.A. $18^{h}\ 52^{m}$·3, Dec. 44° 4′.
Mag. A 7·4, B 8·5, C 11·1.

AC is Σ 3130; distance 3″. In 1841 OΣ readily saw that A was a close double; it was single in 1852, measured in 1857, and single ever since. Probably a binary of short period and variable.

ζ Sagittarii. R.A. $18^{h}\ 55^{m}$, Dec. −30° 3′. Mag. A 3½, B 4.

Discovered by Winlock many years ago. Newcomb measured it in 1867: β examined it many times prior to 1878, but without seeing the companion; in 1878·7, however, he saw and measured it; distance 0″·42. Probably a case of occultation.

OΣ 380. R.A. $19^{h}\ 36^{m}$·9, Dec. 11° 33′. Mag. A 5·7, B 7·7.

The close pair is thus given by β in 1877·7 :— 74°·6, 0″·64. OΣ saw a third star (distance 1″·2) in 1842, but failed to see it again. Newcomb and β have also failed to see any trace of it.

β 151. R.A. $20^h\ 31^m{\cdot}9$, Dec. 14° 11′.

This is β Delphini, the close pair forming a remarkable binary star; it was discovered in 1873, and has been gradually closing up ever since: distance, in 1878, $0''{\cdot}24$.

OΣ 424. R.A. $20^h\ 53^m{\cdot}6$, Dec. 15° 6′. Mag. A 7–8, B 8–9.

Discovered in 1845 by OΣ, distance $0''{\cdot}46$; elongated by De in 1865; single to β in 1876 and 1877. An occultation.

Mr. Edward Crossley's Observatory,
Bermerside, Halifax.

J. Gledhill.

Double Stars for April.

The most important pairs from 7^h to 9^h have already been given. Some of the following objects present points of interest; others need remeasurement. CO = the Cincinnati observers Ormond, Stone, Howe, and Egbert.

Σ 1047. R.A. $7^h\ 7^m{\cdot}4$, Dec. +15° 59′. Mag. 8·5, 10·3.

22°·3	$21''{\cdot}51$	1868·4	De.

Σ 1046. R.A. $7^h\ 7^m{\cdot}8$, Dec. +14° 46′. Mag. 8·6, 11·7.

234°·2	$10''{\cdot}92$	1867·7	De.

Σ 1049. R.A. $7^h\ 7^m{\cdot}9$, Dec. −8° 43′. Mag. 8·3, 10.

42°·8	$3''{\cdot}5$	1867·7	De.
40 ·2	3 ·37	79·2	CO.

Σ 1051. R.A. $7^h\ 12^m{\cdot}1$, Dec. +73° 19′. Mag. 6·5, 8·5, 9.

AB	278°·4	$1''{\cdot}22$	1858·1	Mä.
AC	81	31	fixed.	

Σ 1066. R.A. $7^h\ 12^m{\cdot}9$, Dec. +22° 12′. Mag. 3, 8.

This is δ Geminorum.

184°·1	$6''{\cdot}5$	1781·8	H.
202 ·9	7 ·12	1876·2	W.
204 ·3	7 ·37	1878·1	Dob.

Probably binary; difficult; small proper motion.

Σ 1081. R.A. $7^h\ 17^m{\cdot}0$, Dec. +21° 42′. Mag. 7·5, 8·5.

224°·6	$1''{\cdot}4$	1867·8	De.
226 ·1	1 ·36	78·1	Bu.

Σ 1134. R.A. $7^h\ 37^m{\cdot}2$, Dec. +3° 47′. Mag. 8, 11.

147°·1	$10''{\cdot}02$	1869·3	De.

Σ 1136. R.A. $7^h\,41^m{\cdot}8$, Dec. +65° 13′. Mag. 7·3, 11.
244°·9 9″·51 1869·3 OΣ.

Bu 24. R.A. $8^h\,48^m{\cdot}4$, Dec. −8° 18′. Mag. 7·8, 9.
177°·3 1″·12 1878·2.

α Leonis. R.A. $10^h\,2^m$, Dec. +12° 33′. Mag. 8·4, 13.
BC 85°·0 3″·17 1876·3 Hall.
88 ·5 ·29 8·1 Bu.

"The minute companion to H's star was detected by the late Professor Winlock some years ago with the 15-inch of the Harvard College Observatory." (Bu.)

JOSEPH GLEDHILL.

Mr. Edward Crossley's Observatory,
Bermerside, Halifax.

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*Meteor Notes for April**.

THE Lyrids should be looked for on the mornings of the 19th–20th. The Moon being past the first quarter will to a certain extent interrupt observations until about $2^h\,30^m$ A.M.; but this annual shower of Lyrids appears to have fallen off considerably during the last few years and to have become a display of minor importance. On the 11th and 12th† large meteors are abundant, and the Moon being absent this year the epoch is a favourable one for the reobservation of the showers to which they are to be attributed. The chief radiants have been determined at 249° +51°, 212° +65°, and 184° +59°, and several fireballs have been directed from the Lynx and fore part of Ursa Major. There are active showers also near β Boötis (223° +42°), η Ursæ Majoris (206° +44°), and in Hercules two well-defined radiants are visible at 257° +37° and 265° +23°. A new shower in Cepheus (335° +62°) also requires confirmation; and amongst the constellations included in the zone from Leo—Ursa to Aquila—Cygnus a great number and variety of showers are displayed many of which have hitherto been slenderly observed. These will be revealed by a series of well-directed and long-continued watches, which are also likely to bring to light a few conspicuous streams not seen by former observers. At this period of the year the region near Lyra has received a monopoly of attention, and thus the major showers in the neighbourhood of

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* For further notes on April showers see 'Observatory,' Vol. i. p. 417, and Vol. ii. pp. 416–17.

† A list of the principal showers for April 9–12 is given by the writer in the 'Monthly Notices,' Vol. xxxix. pp. 404–5; and for April 19–22 in Vol. xxxviii. p. 396.

Cassiepeia (visible in the N.W. in the evening hours) have been comparatively neglected.

Recent Observations.—The Rev. S. J. Perry mentions * a radiant-point at R.A. 155° 12′, N.P.D. 54° 54′, observed on the mornings of 1879, November 13–15, at Stonyhurst Observatory, and gives the position of a 1st-mag. stationary meteor at R.A. 157° 18′, N.P.D. 52° 30′, Nov. 14, $13^{h}\ 34^{m}$. The shower in Leo Minor to which these observations relate was pointed out in the 'Observatory,' Vol. iii. p. 202, as one of the co-Leonid centres of radiation. It was seen by Major Tupman in 1869, Nov. 7, at 160° +40°, and by the writer in 1876, Nov. 26–29, at 155° +36°, when it was described as "a very fine shower of bright swift meteors with streaks like the Leonids" †. Two meteors of the same stream were simultaneously recorded at the Royal Observatory, Greenwich, and at the Stonyhurst Observatory on the morning of Nov. 15, 1875, one of which, as observed at the former station, was nearly stationary at 158° +40°, and the other had a radiant at 154° +37° from the combined tracks ‡.

Several large meteors are described in the 'Science Observer' (Boston, Mass.), No. 25, as follows:—

(1) 1879, Nov. 3, $6^{h}\ 43^{m}$. > ♃. Appeared immediately below ♄ and moved perpendicularly downwards, passing θ Ceti and disappearing at τ Ceti without separating into fragments. Duration 1½ to 2 seconds. Observer W. H. Numsen, Baltimore, Md.

(2) 1879, Nov. 5, $6^{h}\ 20^{m}$. A small fireball started from α Persei and burst into four fragments a few degrees below Polaris. Train of 10° or 12° in. length. "The colour of the meteor was at first a bluish purple, becoming more and more brilliantly tinted, and finally exploded with a strontian flame red. I have several times noted this change of colour in explosive meteors; in this instance no noise was heard." Observer G. H B. Trouvelot, Cambridge, Mass.

(3) 1879, Dec. 9, $7^{h}\ 19^{m}$. = ♀ at max. Passed from 215° +72° to 198° +63°. The sky was completely obscured by heavy high clouds, no stars or planets being visible. Its position was referred to one of the roofs near which it passed and the apparent path determined by the stars on a subsequent evening. Observer O. H. Landreth, Nashville, Tenn.

(4) 1880, Jan. 19, $7^{h}\ 20^{m}$. A double meteor. The colour and brightness of the primary = ♄; the secondary was half as bright and distant 1½°. The meteor was first seen about 3° from Capella, and it moved towards θ Ursæ Majoris, the line being about parallel to the line joining β and γ Ursæ Majoris. Observer Dr. L. Waldo, New Haven.

W. F. Denning.

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* 'Monthly Notices' of the R.A.S. Vol. xl. p. 140.

† Monthly Notices, Vol. xxxvii. p. 110.

‡ B.A. Report on Luminous Meteors, 1877, pp. 126 and 162.

CORRESPONDENCE.

To the Editor of 'The Observatory.'

The Great Southern Comet.

SIR,—

I suppose that you will before this have received from some of the Southern observatories the particulars of the visit of our last Great Comet. I could not succeed in getting a perfect observation of it. I saw it first on Thursday evening, Feb. 5, in the W.S.W. horizon, or rather the upper portion of the tail was visible; light clouds persistently hid the lower part from view, so that it was impossible to ascertain the exact position of the nucleus, but from the direction of the tail it could not have been far from Fomalhaut. The portion of the tail visible was about 34° in length, 2° in breadth, its long straight narrow form resembling the one of 1843, which I remember well, although inferior in length and brilliancy to that famous comet. The tail passed over ϵ Phœnicis without affecting the light of the star in any degree. It then stretched upwards toward Achernar, passing about 3° N. of that star. For such great magnitude the tail seemed deficient in brilliancy, its light hardly equalling and certainly not exceeding the light of the minor Magellanic cloud which was then not far off, and so provided a good test for comparison. It was too faint to allow it to be visible at 7.15 in the twilight. It could be discerned at 7.30, but did not show well out until 7.45.

I saw it again on Feb. 6 and 7; but a similar belt of cloud near the horizon covered the head, so that I could not obtain even a glimpse of it, which I felt the more sorry for, as I am sure it must have been both large and bright. The position had so far altered that on Feb. 6 the tail passed over α, β, and δ in Phœnix, and κ and ϕ in Eridanus; and on Feb. 7 it passed in a line north of those stars between β and ν in Phœnix, showing that the comet was moving in a N.E. direction, and I should say rapidly receding from the earth, as the tail showed each night much fainter. Since then the weather has set in rainy and cloudy, preventing any further observations whatever.

I am, Sir, yours very faithfully,
HUDSON R. JANISCH.

Sirius and Piazzi.

SIR,—

I have had occasion lately to make a careful diagram of Sirius and the principal neighbouring stars visible with a low-power ocular, magnifying 80 diameters, on my equatoreal of 8 in. aperture. Smyth in the Bedford Catalogue quotes the following note from Piazzi's catalogue, "alia 8ª magnit. præcedit 3″ temporis, 3′ ad Boream." Now it is evident that an 8th-magnitude star 3′ north of Sirius would be conspicuous with 8 inches aperture, yet

I can find no star at all answering to this description. I have verified the accuracy of Smyth's quotation by referring to Piazzi's catalogue. I placed the wires of my parallel wire micrometer at 3′ or 180″ distance, and made a star run along one of the wires when the telescope is moved in R. A., and with this have carefully scanned the neighbourhood of Sirius, and cannot find any conspicuous star so near. I presume Piazzi's description would make the position-angle of his star about 340°.

There is another puzzle in the Bedford Catalogue in connection with Sirius. Smyth mentions a distant 10th-magnitude star, of which he gives the Pos. = 45°·0, Distance = 150″ (it is really much further off, but that does not affect the matter in hand), and adds, "a line through the two here cited passes nearly upon that mentioned by Piazzi, 'alia 8ᵃ magnit. præcedit &c.'" Smyth's distant 10th-mag. star is in the *n. f.* quadrant, and Piazzi's in the *n. p.*; and a moment's consideration would show that a line through Sirius and a distant star the in *n. f.* quadrant could only "pass nearly upon" a star in the *n. p.* quadrant, when the position-angles are respectively very near 90° and 270°, or 0° and 360°. Obviously these conditions are not fulfilled in this case.

I am, Sir, yours truly,
GEORGE HUNT.

Hopefield, Alleyn Park,
West Dulwich, 1880, March 15.

Deflection in the Path of a Meteor.

SIR,—

In the 'Observatory' you have lately entertained discussions on meteors which were observed as much as two or three years ago. I venture to think, therefore, that it might perhaps be worth while for me to mention that in November 1876 I observed here a fine meteor, and mentioned it in a letter to 'The Times.' Two other correspondents from different parts of the country also wrote to the same effect.

I put myself in communication with the writers, one of whom was Mr. Launcelot Sharpe, of St. John's College, Oxford; and both these observers very kindly sent me all the particulars they could, and I made out a path &c. for the meteor in accordance with these data. I had intended to have sent a paper on the subject to the Royal Astronomical Society; but something prevented me, and I have now searched for it in vain. But this one point came out very clearly, and this is my excuse for bringing the thing to your notice.

The observations, which seemed to have been carefully taken, were irreconcilable on the theory of a straight path taken by the meteor, but pointed out that its flight must have been curvilinear in consequence of having been deflected horizontally, and that when first observed it was nearly moving in a direction which, if projected on the earth's surface, would have been W.S.W., but at its disappearance was moving rather northwards of west. Such deflection from the straight line would be almost certain to occur if the shape of the projectile was irregular or flat-sided; and it may be very often the case when meteors get entangled in our atmosphere, and may serve to explain apparent discrepancies in the accounts of different observers.

I am, Sir, yours truly,
F. C. Penrose.

Colebyfield, Wimbledon,
March 22, 1880.

Suggestions to Astronomers.

Sir,—

In view of the liability of errors being made in the transmission of telegraphic announcements of cometary discoveries, of which we have recently had a surfeit, I beg leave to offer the following suggestions, which, if adopted, will, I think, have a tendency to give at least a clue to the vicinity of the comet's position.

1. In every instance, give the constellation the comet was in at discovery, and if near the boundary of two constellations, name both.
2. State if it is large or small, bright, faint, or very faint. This information will enable one to decide if there is any prospect of success in searching in moonlight, or, in its absence, with small telescopes. Let, say, 13 Messier be a standard of comparison for large and bright.
3. Make the dispatch short, by omitting every thing confusing and that does not assist in finding it, such as magnitude, nucleus, tail, seconds of time and of arc.

Perhaps further improvments can be suggested; if so, after what we have lately endured (liable at any time to be repeated), let some one be selected to decide on the best form to adopt.

I am, Sir, yours respectfully,
Lewis Swift.

Rochester, N. Y.,
March 8, 1880.

NOTES.

Annals of Harvard College Observatory.—In Vol. xi. Parts i. and ii. are given the important results of the photometric observations made principally with the 15-inch refractor in the years 1877–79. It may be remembered that Prof. Pickering fixed on this class of work as the most suitable for his large equatoreal; and some account of the observations made was given in his annual reports (*cf.* 'Observatory,' Vol. i. p. 408; Vol. ii. p. 391). Various forms of photometer were employed, composed for the most part of a Double-image prism and a Nicol prism, so that two images of each of two bright objects in the same field were formed, either of which by turning the Nicol could be made as faint as was desirable. Thus the faint image of the brighter object could be reduced to equality with the bright image of the fainter, and the true relative brightness would then be deduced from the angle through which the Nicol is turned.

Photometers A and B have a double-image and Nicol prism combined with an eyepiece; C is a similar arrangement, but without the eyepiece, and is used without a telescope to compare Saturn and Mars, and Jupiter and Venus. In Photometer H the double-image prism slides in a tube in front of the field-lens of the eyepiece, and thus the separation of the images can be varied from 0 to 64″ with the 15-inch, and from 0 to 190″ with the 5-inch refractor occasionally used. For comparing objects at a greater angular distance achromatic prisms were placed in front of the central portion of the object-glass. O is a combined spectroscope and photometer, a direct-vision prism being placed within the principal focus, so that the brightness of different parts of the spectra of two objects can be compared. For faint objects photometers of a somewhat different class were used, the faint object being viewed directly through an eyepiece in the usual way and compared with some bright object, assumed as a standard, the light of which, passing through a small auxiliary telescope, was reflected into the field. In D the standard was reduced to equality with the faint object by means of two Nicol prisms; in E, G and I a diaphragm of variable aperture (formed by two sliding V-shaped pieces of brass) was used for the same purpose. To adapt photometer D to bright objects, the Nicols were removed and its eyepiece was replaced by photometer C. This modified form is called F.

The observations made with these various photometers from 1877 August to 1879 March, to the number of 5000, are given in order of date in Table I., Journal, and are afterwards discussed in the next two chapters.

The conjunctions of the planets Saturn and Mars, and Jupiter and Venus, in 1877, afforded a good opportunity of comparing their relative brightness. From 600 comparisons of Saturn and Mars made on 25 nights, and from 300 of Jupiter and Venus on 11 nights with photometers B and C, without a telescope, Prof. Pickering finds:—

Albedo of Saturn = 4·62 × Albedo of Mars,
" " Jupiter = 0·86 × " " Venus,

allowance being of course made for their different diameters and distances from the Sun. In the case of Mars there is great uncertainty respecting the diameter. Le Verrier gives 11″·10 (at distance unity), as deduced from the Greenwich observations; and this value is used in the 'Nautical Almanac,' 'Berlin Jahrbuch,' and 'Connaissance des Temps.' The American Ephemeris uses 10″·10, based on the Washington observations; and Kaiser finds 9″·4 from good micrometric measures *. Prof. Pickering adopts the intermediate value 10″·10, considering that a part of the discrepancy may be explained by irradiation and enlargement of the disk due

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* Le Verrier's and Kaiser's values would give 29″·4 and 24″·9 respectively for the diameters at the opposition of 1877.

to bad atmospheric conditions. It is to be remarked that the diameter of Mars varies greatly, whilst the irradiation and enlargement remain sensibly the same whatever be the diameter. Thus they would have a larger proportionate effect in the case of small diameters, which form a large proportion of the Greenwich observations, than on large diameters, such as would be used by preference for micrometer measures. The latter would also seldom be made when the state of the air was not good, whilst meridian observations are often made under bad conditions.

The rest of the volume is devoted to the discussion of the photometric observations of about 180 bright double stars, selected from Struve's 'Mensuræ micrometricæ,' the separate results being given in a ledger and afterwards collected in a catalogue. In these observations comparisons were also made of the brightness through red and blue glass, and of the red, yellow, green, blue, and indigo light in the spectrum formed by a small direct-vision prism. In the case of double stars showing a marked contrast in colour, such as η Cassiopeæ, γ Andromedæ, and β Cygni, no difference was found in the measurements of different observers, such as might have been anticipated from the effect of personality as regards colour. The effects of aperture, distance of the images compared, position-angle, focus, and proximity of the bright star are fully discussed. With regard to the last-named source of error, it was found that a variation exceeding 3 magnitudes in the apparent brightness of the companion to a second-magnitude star resulted from a change in the distance of the components from 3″ to 60″. In deference to custom, Prof. Pickering gives the probable errors of his results; but he is careful to point out in his preface that these are to be taken as a test of accordance and not of correctness, when any undetected systematic errors may be present.

Amongst other interesting questions, Prof. Pickering has examined the polarization in the light of the companions of bright stars, but has not hitherto been able to detect any. In the case of Sirius he points out, as the result of the photometric determination of brightness, that if the companion shines by reflected light its diameter must be 0″·5 at least, and must therefore exceed that of the Earth's orbit.

The accuracy attainable in photometric observations by the method of extinction, was tested by a series of measurements, which show that a variation of several magnitudes may be expected in measures by different persons on different days, and that even when the observations are differential, the residuals are so large that the results have little value.

Part i. concludes with a comparison of Struve's magnitudes with the photometric. From this it appears that when the two stars are near together the small star is always estimated too faint, and that Struve's scale makes the difference between consecutive magnitudes too small, and therefore those of Herschel and other observers err still more in the same direction. The scale adopted by

Prof. Pickering is that of Pogson, in which 0·4 is used as the divisor of the logarithmic ratios of the brightness, to obtain the difference expressed in magnitudes. On this scale a star of any given magnitude is 2·512 times as bright as a star of the magnitude below, and 100 times as bright as a star five magnitudes below. Thus a first-magnitude star would be 100 times as bright as a sixth, and 2·512 times as bright as a second-magnitude star.

THE GREAT SOUTHERN COMET.—In the Report of the R.A.S. Meeting, p. 363 &c., will be found a summary of various observations of this remarkable comet. Dr. Gould, at Cordoba, first saw the coma 2′ or 3′ in diameter on Feb. 4; and on Feb. 5 Mr. Eddie, at Graham's Town, picked up the nucleus, a faint object about the size of the ring-nebula in Lyra (1¼′ in diameter). On Feb. 7 it passed nearly over γ Sculptoris. By Feb. 23 the comet had become so faint that Mr. Gill (in strong moonlight) could not discern the least trace of it. The following sets of elements (communicated by Mr. Hind) have been computed respectively by Mr. Finlay, the Chief Assistant at the Cape Observatory, and by Mr. Hind from Dr. Gould's observation on Feb. 4 and from the *rough* places on Feb. 10 and 15 sent home by Mr. Gill. For comparison with Mr. Hind's orbit, the parabolic elements computed by Hubbard for the Great Comet of 1843 are appended:—

	Finlay.	Hind.	Comet 1843.
Per. Pass. ..	1880, Jan. 27·55	Jan. 27·6027 G.M.T.	
Long. Per...	280° 16′	279° 6′·8	278° 35′·1
Long. Node.	123 24·5	4 1·9	1 20·6
Inclination .	75 12	35 39·8	35 38·2
Per. Dist. ..	0·008001	0·0059390	0·005511
Motion	Direct.	Retrograde.	Retrograde.

Sun's radius 0·004664 (Sun's mean distance = 1).

In a letter to the Astronomer Royal, in which he communicates these results, Mr. Hind remarks on the resemblance between his orbit for the comet of 1880 and that for the great comet of 1843:—"Can it be possible that there is such a comet in the system almost grazing the Sun's surface in perihelion and revolving in less than 37 years? I confess I feel a difficulty in admitting it, notwithstanding the above extraordinary resemblance of orbits." From rough places on Feb. 10, 13, and 15, Mr. Hind had previously found elements somewhat different from the above. Both sets differ *in toto* from Mr. Finlay's.

The following places for the comet have been computed by Mr. Hind:—

8^h G.M.T.	R.A.	N.P.D.	Log. Δ.
1880. March 31	$4^h\ 55^m{\cdot}8$	98° 47′	0·2849
April 2	4 59·6	98 22	0·2978
4	5 3·2	97 58	0·3104

The comet is now probably excessively faint.

THE SOLAR ECLIPSE OF 1880, JAN. 11.—Mr. George Davidson's Report, published in the 'Chicago Tribune' of Feb. 20, effectually disposes of the statement that an Intra-Mercurial planet was seen during the eclipse. Mr. Davidson did not even look for Vulcan, as the sky was too bright on account of the contracted diameter of the cone of shade. Jupiter and Mars were seen; but no stars. Mr. Davidson's party were posted on the Santa-Lucia Station, 5700 feet above the sea, and were provided with a 6⅜-inch and a 3-inch equatoreal, besides smaller telescopes. Prof. Frisby, of Washington Observatory, occupied the same station for independent work. The four contacts were observed and the corona sketched, as well as the brighter circle concentric with the Sun and the prominences. During the progress to totality the Moon's disk was remarked to be perceptibly darker than the sky adjacent to the Sun's disk, yet unobscured (*i. e.* the Sun's corona was perceived during the partial phase). During totality the concentric circle of bright white light round the Sun was very striking; and Mr. Dickins sketched a second but fainter concentric circle. "The corona had the general form of a parallelogram, with the angles prolonged in the direction of the longer sides, and stretched at an angle of about 30° with the vertical from the upper left to the lower right of the Sun." Mr. Davidson remarks that this "eclipse is another confirmation of the theory that the exhibition of Baily's beads, the ligament and black drop in the transits of Mercury and Venus, the hanging of a coloured star on the Moon's bright disk at occultation, and similar phenomena are the consequences of atmospheric disturbances occasioned by irregularities &c. of refraction." He adds that this view has been strongly controverted. According to his own statement, "there was no distortion from atmospheric disturbance" on this occasion; and the breaking of the last line of sunlight, from 30° to 40° in length, "was on account of the lunar irregularities of sunlight, and presented the appearance of a line of irregular dots, dashes, and spaces." This account appears to agree with the usual descriptions of Baily's beads; and the appearances seen by Mr. Davidson would seem to have been due to irregularities on the Moon's limb, in accordance with the ordinary explanation, and not to atmospheric disturbance.

A NEW ASTRONOMICAL REVIEW.—We have received the first two numbers of 'Ciel et Terre,' a bi-monthly popular review of Astronomy and Meteorology, edited by members of the staff of the Brussels Observatory. No. 1 contains the following articles:—"La Conquête de l'Heure," by J. C. Houzeau; "Les Grands Froids de Décembre 1879," by A. Lancaster; "L'Analyse spectrale," by C. Fievez; "Le Ciel pendant le mois de Mars 1880," by L. Niesten; "Revue Météorologique de la Quinzaine" (with a plate), by J. Vincent; besides "Notes" and "Bibliographie." No. 2 contains:—"Histoire du Thermomètre," by C. Hooreman; "La vitesse de la Lumière et la Parallaxe du Soleil," by L. Niesten; "La Mer et ses Profondeurs" (with a plate), by F. Van Rysselberghe; "Sur la Vitesse du Vent," by C. Lagrange; "Le Spectre Solaire," by C. Fievez; "Revue Météorologique de la Quinzaine," by J. Vincent; "Notes" and "Bibliographie." This last, contributed by A. Lancaster, will be found very useful, as it gives the titles of the papers on astronomy and meteorology which have appeared in the course of the month in the various scientific publications. 'Ciel et Terre' is intended to popularize astronomy and meteorology, and the two opening numbers are all that could be desired for this object. Its connexion with the Brussels Observatory, which, under M. Houzeau's directorship, has shown so much activity in various branches of astronomy, may be taken as a guarantee of its permanent success. Each number contains 24 pages, and the annual subscription (24 nos.) is fixed at 10 francs (including postage) for Great Britain and other countries in the postal union. Subscriptions should be sent to M. C. Hooreman, Rue du Cadran 10, à St. Josse-ten-Noode, Belgium.

CAMBRIDGE OBSERVATIONS, 1861–1865.—Vol. xxi. of the Cambridge Observations, made under the superintendence of Prof. Adams in the years 1861–1865, has just been published. It contains for each year the Transit and Mural-Circle observations in order of date, giving all the necessary particulars for their reduction, admirably arranged in a very compact form, ledgers of R.A. and N.P.D., showing the separate results for each star, and a yearly star-catalogue—results of observations of the Sun and planets and determination of the position of the ecliptic—occultations of stars by the Moon—and observations of comets with the Northumberland Equatoreal and of Mars in 1862. In the introduction are given the various instrumental corrections and formulæ and tables used in the reductions. The most troublesome of these corrections is that for irregularity in the form of the pivots of the transit; and great care seems to have been taken to determine the deviations from circularity by means of a dot placed on each pivot. In 1865 Professor Adams made a fresh examination of the errors from this cause, and the results are exhibited by means of curves. A glance at these shows that the greatest deviations occur nearly in the positions in which the instrument has been most frequently used. The transit-instrument has since been replaced by the new Transit-Circle, which, it may be hoped, is not affected with any large pivot-errors.

TWO NEW Minor Planets have been discovered, viz. No. 214 by M. Palisa, at Pola, on March 1, and No. 215 by Prof. Peters, at Clinton, New York, on March 18.

PROF. VALENTINER announces that the Observatory of the Grand Duchy of Baden has been moved from Mannheim to Carlsruhe.

EDITOR.

Astronomical Memoranda, 1880, *April.*

Sun. April 1, sets 6^h 31^m, rises 17^h 34^m; April 15, sets 6^h 55^m, rises 17^h 3^m; May 1, sets 7^h 21^m, rises 16^h 31^m.

Equation of time:—Sun *after* Clock, April 1, 3^m 47^s; April 11, 0^m 56^s; *before,* April 21, 1^m 26^s; May 1, 3^m 5^s.

Sidereal Time at Mean Noon:—April 1, 0^h 40^m·9; April 11, 1^h 20^m·3; April 21, 1^h 59^m·7; May 1, 2^h 39^m·2.

Moon.	*rises.*			*sets.*			*sets.*	
	h	m		h	m		h	m
Apr. 1..	14	15	Apr. 11..	9	28	Apr. 21..	15	16
2..	14	50	12..	10	32	22..	15	33
3..	15	17	13..	11	31	23..	15	54
4..	15	37	14..	12	20	24	*rises.*	
5..	15	56	15..	13	1	25..	8	44
6..	16	13	16..	13	34	26..	10	5
7..	16	28	17..	14	1	27..	11	15
8..			18..	14	23	28..	12	11
9..	*sets.*		19..	14	40	29..	12	51
10..	8	18	20..	14	59	30..	13	21

Last Quarter, April 1, 18^h 13^m; New, April 9, 3^h 7^m; First Quarter, April 17, 7^h 14^m; Full, April 24, 10^h 50^m.

Mercury is a morning star in Pisces, at greatest elongation (26° 57′ W.) on April 25, when he rises about 35^m before the Sun.

Apr. 15,	R.A. 0^h 9^m·7,	Dec. 0° 22′ S.,	tr. 22^h 32^m,	rises 16^h 30^m
May 1,	1 0 ·7,	3 16 N.,	22 22	16 0

Venus is a morning star in Pisces, rising about half an hour before the Sun. The illuminated portion of the disk is 0·916 on April 15, and 0·961 on May 15, and the corresponding diameters 10″·9 and 10″·2.

Apr. 1,	R.A. 23^h 7^m·0,	Dec. 7° 6′ S.,	tr. 22^h 27^m,	rises 16^h 57^m
Apr. 15,	0 10 ·5,	0 33 S.,	22 35	16 30
May 1,	1 22 ·8,	7 4 N.,	22 44	16 2

Mars is an evening star in Gemini.

Apr. 1,	R.A. 5^h 35^m·0,	Dec. 25° 5′ N.,	tr. 4^h 54^m,	sets 13^h 23^m
May 1,	6 50 ·6	24 33 N.,	4 11	12 35

Jupiter is a morning star in Pisces, rising shortly before the Sun. There are no phenomena of the Satellites visible at Greenwich.

Apr. 1,	R.A. 0^h 0^m·5,	Dec. 1° 7′ S.,	tr. 23^h 17^m,	rises 17^h 18^m
Apr. 15,	0 12 ·7	0 11 N.,	22 34	16 29
May 1,	0 26 ·0	1 36 N.,	21 44	15 31

Saturn is in conjunction with the Sun on April 7.

Apr. 1, R.A. 1^{h} 9^{m}·4, Dec. 4° 57′ N., tr. 0^{h} 29^{m}
Apr. 15, 1 16·0, 5 37 N., 23 37
May 1, 1 23·3, 6 21 N., 22 41 rises 16^{h} 5^{m}

Uranus

Apr. 1, R.A. 10^{h} 30^{m}·6, Dec. 10° 14′ N., tr. 9^{h} 48^{m}
May 1, 10 28·2, 10 27 N., 7 48

Phenomena.

	G. M. T. h	m	
Apr. 7	17		*Saturn* ☌ ☉.
11	7	27	47 Ariet. Oc. D. 122°.
14	8	38	121 Tauri Oc. D. 51°.
	18		*Mercury* ☌ *Venus*, *Mercury* 19′ N.
15	14		*Venus* ☌ *Jupiter*, *Venus* 30′ S.
16	10	43	56 Gemin. Oc. D. 26°.
	12	48	61 Gemin. Oc. D. 76°.
Apr. 17	10	58	ζ^{1} Cancri. Occultation near approach 157°.
	21		*Mercury* ☌ *Jupiter*, *Mercury* 48′ S.
21	12	10	*e* Leonis Oc. D. 72°.
25	9		*Mercury* elongation 26° 57′ W.
28	16	57	26 Sagitt. Oc. R. 223°.
May 1	3		*Venus* ☌ *Saturn*, *Venus* 47′ N.

The angles are reckoned from the *apparent* N. point towards the right of the Moon's inverted image.

Conjunctions of four of the Planets.—Mercury, Venus, and Jupiter will be very close together in Pisces on the mornings of April 15, 16, 17, and 18 (civil reckoning); and Saturn will be within 20° of them, as will be seen from the R.A. and Dec. given above for the several planets on April 15, and from the respective conjunctions of Mercury and Venus (April 14, 18^{h}), Venus and Jupiter (April 15, 14^{h}), and Mercury and Jupiter (April 17, 21^{h}) given in the "Phenomena." On May 1 Venus is in conjunction with Saturn, Mercury is about 6° distant, and Jupiter about 20°. The Planets rise only about half an hour before the Sun. EDITOR.

Publications received:—Prof. Lewis Boss, *Declinations of Fixed Stars* (U.S. Northern Boundary Commission, Appendix H)—Prof. D. Kirkwood, *The Cosmogony of Laplace*—Ch. Fievez, *L'Intensité rélative des raies spectrales de l'Hydrogène et de l'Azote* (F. Hayez, Bruxelles, 1880)—H. A. Howe, *Three approximate Solutions of Kepler's Problem* (Cincinnati Society of Natural History)—H. Pratt, *On the Rotation Period of Jupiter* ('Monthly Notices' R. A. S. 1880, Jan.)—*Ciel et Terre,* Revue populaire d'Astronomie et de Météorologie, Nos. 1 and 2 (F. Hayez, Bruxelles, 1880, March 1 and 15)—*Science Observer*, No. 25 (Boston Scientific Society, U.S.)—*American Journal of Science*, 1880, March (J. D. and E. S. Dana, New Haven, Conn.)—*Popular Science Monthly*, 1880, March (D. Appleton, New York)—*Kansas City Review*, 1880, March—*The Analyst*, 1880, March (Des Moines, Iowa)—*Sirius*, 1880, February (Leipzig). EDITOR.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

No. 37. MAY 1. 1880

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, 1880, April 9.

Edwin Dunkin, F.R.S., *Vice-President*, in the Chair.

Secretaries: W. H. M. **Christie**, M.A., and
J. W. L. **Glaisher**, M.A., F.R.S.

The Minutes of the previous Meeting were read and confirmed.

Mr. Chambers. Would you kindly oblige me, Sir, with an official answer as to the decision of the Council on the requisition which I have handed in as to the proposed alteration in the bye-laws?

The Chairman. The requisition was laid before the Council at their Meeting, not to-day, but in March last, and the Council, by a majority, declined then to proceed any further in the matter. A motion was proposed by the Foreign Secretary that the question be referred to a Select Committee, and that motion was lost.

Mr. Chambers. I consider that an answer which is not quite satisfactory to the five Fellows. May I further ask if it is competent to me to give notice of a Special Meeting to consider the question in June? Or what would be the proper step to take under the bye-laws, for it is not very clear?

The Chairman. Perhaps in answer to Mr. Chambers I may read the bye-law referring to this. It says:—"Any five Fellows not being on the Council may recommend the adoption or rejection of any measure, or the adoption, repeal, or alteration of any bye-law to the Council by a letter addressed to one of the Secretaries. Such recommendation the Council shall take into consideration at their next Meeting; and if their decision be not satisfactory to the Fellows proposing the recommendation, the Council, if required by them, shall bring forward the same for the opinion of the Society at large, at a Special General Meeting to be held for that purpose, and which they are hereby required to convene within one month after such requisition."

Mr. Chambers. Would you kindly read bye-law No. 55?

The Chairman. "No new Bye-law, nor alteration or repeal of any existing Bye-law, shall be brought forward at any Meeting of the Society except in the manner directed in this and the preceding Section, unless one month's previous notice thereof shall have been given to the Council agreeably to the Charter."

Mr. Chambers. What I do not understand is, how I am to proceed if I wish to do so. These two bye-laws seem to be contradictory. In the one case I am to give a month's notice, and in the other case you are to convene a Meeting within a month.

The Chairman. The bye-laws seem certainly to need revision on many points; but I have no doubt the strict law of the case is that if these five gentlemen who signed the requisition are not satisfied they can, by this latter bye-law, compel the Council to call a Special General Meeting within one month after the request is made.

Mr. Chambers. Then I take it that five weeks' notice before the June Meeting would enable the Council to fix a Special General Meeting.

The Chairman. I should think that would be quite correct; but it would be necessary for the five gentlemen who signed the requisition to do that.

Mr. Neison. Perhaps it is rather informal, but still I think it a matter of importance. If I understood Mr. Chambers's requisition properly, it did not call upon the Society to summon a General Meeting, it was merely a recommendation to the Council to adopt a certain measure; and therefore if he wishes to have a General Meeting summoned it will be necessary to have a requisition formally calling upon the Council to summon a Meeting to take into consideration the question of the alterations in the bye-laws. If my memory serves me aright, the requisition handed in was not for that purpose, but merely recommended the Council to revise the bye-laws. It was not such a request as is contemplated in the bye-laws.

Mr. Barrow. I might perhaps make a remark upon that. The difficulty seems to be this, in the minds of some of the authorities present, that Mr. Chambers's requisition was not in accordance with the bye-laws because it did not specify any particular measure which it required the Council to adopt; but the requisition was merely to take into consideration the adoption of some measure, without specifying what; and it seems to be the opinion of a good many Members of the Council that the requisition was not sufficient in that respect. The very circumstance that we are discussing this matter to-night, involving as it does the question of the bye-laws, seems to me to be pretty clear evidence that some alteration or revision of the bye-laws is very strongly required.

Mr. Chambers. I do not wish to prolong the discussion at the present moment, but I think Mr. Neison has rather misapprehended me. I have handed in a requisition duly signed to the Council to do something which I thought was very clear and explicit, viz. to revise all the bye-laws. The Council, in their judgment, have decided not to adopt that request; and now I propose, if others will join me, to follow up that requisition in terms of the bye-laws by a new requisition in terms of the bye-laws, asking for a Special General Meeting in terms of the bye-laws.

Mr. Neison. That is right.

Mr. Ranyard. I think also in the Special Notice you must specify the bye-law which you wish particularly to have altered.

Mr. Christie announced that 74 presents had been received since the last Meeting of the Society, including a rare book, Marinonius 'De Astronomicâ Speculâ' (Vienna), presented by Mr. Knobel; also Cataldi's 'Pratica Arithmetica,' presented by Lord Lindsay; a fine photograph of the Moon by Mr. Common; and also an instrument called a "Cometarium," presented by Mrs. Jackson-Gwilt.

The thanks of the Meeting were presented to the respective donors.

Mr. Glaisher read a "Note on a Precessional Globe," by *R. H. M. Bosanquet.*

Also a paper by *Dr. Nicholas de Konkoly,* of O-Gyalla, Hungary, containing a list of 410 radiants of shooting-stars deduced from observations in Hungary from 1871 to 1878, and 80 probable radiants deduced from the 410 radiants. The author mentioned that the calculation of 410 radiants was founded on 3000 observations of meteors; and of those 3000 observations only 1600 were actually employed in determining the radiants. The rest of the paper consisted mainly of tabular matter.

Also another paper, "On Meteors observed during a voyage from London to Melbourne and back, 1879 July to 1880 March," by *Mr. D. W. Barker,* second officer on board the ship 'Superb,' communicated to the Society by Captain Toynbee.

The Astronomer Royal read a paper "On the Theoretical Value of the Acceleration of the Moon's mean motion."

Perhaps before entering on this I might make allusion to some matters which have no connexion with this subject. In the first place, at the Anniversary Meeting of the Society, I was absolutely prevented by illness from attendance, or else I should have been very glad indeed to have adverted to the account of Solar Eclipses published by the Society under the care of Mr. Ranyard, and to say that I think it does honour to the Society and to Mr. Ranyard. The next thing is that I had the great advantage this afternoon of seeing the present which has been made by a Member of the Council to the Society, of a book on the state of the instruments in the Vienna Observatory in the reign of Maria Theresa. I look upon that as very valuable, as occurring at a critical point when the great change in modern Astronomy was just begun, when the excellence of the clocks made by Graham enabled Astronomers to determine the intervals of right ascension by observation of time instead of determining them, as they were determined by Tycho and Flamsteed, by spanning the distance from one star to another all round the sphere. Having passed over these things, which have nothing to do with my present object, I will, if you please, proceed.

The subject is "On the Theoretical Value of the Acceleration of the Moon's mean motion in longitude produced by change of eccentricity of the Earth's orbit."

The Astronomer Royal then read his paper, in which—after adverting to his new method of treating the Lunar Theory by applying to the numerical coefficients found by Delaunay small numerical corrections so that the differential equations of motion should be perfectly satisfied—he explains how he has applied the same form of investigation to the examination of the effects of introducing small forces not contemplated in the original theory. These small forces may, in some instances, depend on geometrical considerations (as in the effect of the Earth's oblateness), or, in other cases, they may depend on the time (as in the slow change of solar energy producing the secular acceleration of the Moon). In any case the small corrections or variations must be so small that their squares and higher powers may be neglected.

A statement is then given of the units employed in the theory and of the lunar inequalities depending on the eccentricity and inclination of the Moon's orbit, which it has not been necessary to consider in this special investigation. Taking the radial and tangential disturbing forces P and T, a "Factorial Table" has been prepared, exhibiting in a symbolical form, as factors of the variations of $\frac{a}{r}$ and v*, the values of the forces P and T which can produce an assigned geometrical disturbance. The object, however, is, by a reversal process, to find the geometrical disturbances corresponding to an assigned external force. In forming these equations, the variations of the Earth's and Sun's attractions, depending on the change of the Moon's place produced by the disturbance, are accurately taken into account.

Taking the mean parallaxes of the Sun and Moon as $8''\cdot91$ and $3422''\cdot3$, the following values are obtained:—

Mean tangential disturbing force at epoch $= -\cdot008284 \sin 2D$;
Mean radial disturbing force at epoch $= +\cdot002683 + \cdot008245 \cos 2D$

(where D is Moon's mean longitude − Sun's mean longitude); and supposing, in progress of time, the solar energy which produces these forces to change from 1 to $1 + bt$, the corresponding new supplemental forces are these expressions multiplied by bt.

Complex equations are then formed, connecting these supplemental forces with the corresponding variations of $\frac{a}{r}$, v, and $\frac{dv}{dt}$; and, in order to solve them, a form is carefully selected for $\delta\frac{a}{r}$ and δv, based on considerations which, it is apprehended, are far more general than any that have been considered before, terms depending on the change of the argument of the "Variation," which result from the secular acceleration being introduced, as well as terms depending on a change in the "Variation" proportional to the time.

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* r being the Moon's radius vector, and v her true longitude.

The value thus deduced for the secular acceleration of the Moon's mean motion is $10''\cdot1477$; and it is remarked that this numerical value is founded on the assumed numerical value of the solar parallax, depending, in fact, on the inverse cube of the parallax. If, for instance, the parallax be diminished by 1-60th part, the acceleration will be diminished by 1-20th part.

The *Astronomer Royal* continued:—That is the state in which I present the results to the Society. I think that this is the toughest question I ever had in my life. It has occupied me, in one shape or other, for many years—sometimes attacking it, sometimes repelled by it, and sometimes adhering to it for months together. It is the most slippery thing that I ever laid my hands upon; but I have got a grasp upon it at last; and I have not the least doubt of the perfect accuracy of it. I would not otherwise have presented it to the Society. I need not say that the matter in which I have been engaged is a matter of great importance, more especially in distant chronology. The total solar eclipses of former times depend entirely upon this small quantity; and I think that in this investigation, which has cost me so much pains, I have worthily bestowed time and trouble to give a little certainty to that point. (Applause.)

Mr. Stone. Some years ago I worked out this question for my own satisfaction, not quite on the lines on which Prof. Adams has worked it, but taking the differential equations of motion. I got Adams's value; but, as the Astronomer Royal says, it is undoubtedly a very slippery subject indeed. It is impossible to follow the details of his work from hearing it read; but I am quite sure upon one point, that we shall all agree in our admiration of the energy and skill which the Astronomer Royal has displayed in this investigation. He began life as an astronomer, with theoretical investigations in planetary theories, making one of the most brilliant discoveries ever yet made. His time for many years appears to have been absorbed in the great work of directing the Greenwich Observatory, and reducing planetary observations and lunar observations. Now towards the close of his life we see him engaged in a great investigation, and carrying it on, we hope, to a successful conclusion.

I cannot find words sufficient to express my admiration at the course which he has adopted. I can only hope that it will lead to a successful conclusion. (Applause.)

The Chairman. After the very interesting remarks of Mr. Stone, you will all agree with me in returning your thanks to the Astronomer Royal for his most valuable paper, which I know, from my knowledge of him, has taken him a very long time to prepare, and I hope that he will be rewarded at last by producing a result which will receive the confidence of astronomers generally.

Mr. Neison. If I understand Sir George Airy aright, it practically amounts to this, that when the subject is treated in the manner he has laid before us, it shows that Prof. Adams, M. Delaunay, Prof. Cayley, and others have omitted something from their investigations, and that their results are imperfect; and that M. Le Verrier (who took the opposite view to Prof. Adams and M. Delaunay) was right. We only hope that Prof. Adams will go over his investigations, from the point of view indicated by Sir George Airy, and let us know what results he comes to.

The Astronomer Royal. I am sure I join in the hope that that will be done; for it is a question of very serious investigation. Perhaps I may say that I had attacked the problem in at least six different methods, and I have found myself baffled in one after another, until at last I resorted to this, which was not known to me in the first instance, namely, the application of the factorial table which I have described; and even in that, difficulties seemed to come on time after time, until at last I perceived, five days ago, that those difficulties were overcome by a particular assumption. In that very last term, analogous to the constant in the solution of a differential equation, which expresses a sort of permanent disturbance of the quadrantal equations, amounting, I believe, to a fraction of an inch, or something like that, a difference in the form of the assumption set all right. But all the preceding terms, as will be seen, were absolutely adopted upon careful consideration of what should be expected under the circumstances, and upon that were founded the true coefficients; but I could hardly trust the evidence myself when I saw how small a matter set all right. (Applause.)

Mr. Christie read a paper on "Observations of the great Southern Comet," by *Mr. Ellery* of Melbourne, in which he says, "The comet whose appearance I announced to you by last mail was observed from the 9th to the 17th of February"—and gives the deduced R.A. and N.P.D.

Also a paper by *Mr. Russell,* giving an account of the comet as observed at the Sydney Observatory.

Mr. Christie further said (in reply to an inquiry):—We have received no communication whatever from Mr. Gill for the Society.

I am very sorry that our President is obliged to be absent to-night, as he might have given us some valuable information on the subject of this comet. I have received some very interesting letters from him, and perhaps I may give some idea of the substance of them to the meeting. Mr. Hind has worked out the orbit of this comet, and found a remarkable resemblance between its elements and those of the great comet of 1843. He has treated the question from several points of view, using observations by Dr. Gould on Feb. 4, and by Mr. Ellery, as well as the rough places sent over by Mr. Gill to the Astronomer Royal, and in each case gets elements sensibly the same as those of the Comet of 1843. Prof. Weiss, of Vienna, has also discussed the matter. He has, from the elements of the 1843 comet, computed the positions on the assumption that the perihelion passage would be on January 27·6 this year; and, computing the places for its appearance, he finds a very close agreement with Mr. Gill's rough places—that is to say, as close as we can answer for; for it must be remembered that those places are only given to minutes of time and minutes of arc, so that we cannot answer for small quantities.

I have a letter which Mr. Hind wrote to me, which I will read to the Meeting, as he is not here to speak on the subject himself. He says:—"Prof. Winnecke, judging from a comparison of the places deduced from the orbit of the great comet of 1843 with Gould's position on Feb. 4, and Gill's later rough ones, appears to be of opinion that the identity of the comets of 1843 and 1838 hardly admits of a doubt. He also suggests that it might be worth while to search for the manuscript observations of the Comet of 1695 at the Depôt de la Marine, Paris, upon which Burckhardt founded an orbit very different from that of Comet 1843, since ¼ (1843–1695) gives for a period a close approximation to the interval 1843–1880. But I think I am able to say with confidence that the elements of Comet 1843 are quite incompatible with the observations (such as they were) of Comet 1695. Burckhardt states besides, that he gave an exact copy of the MS. in the 'Connaissance des Temps, 1817,' where his orbit was printed. Several years since I looked closely into the probable orbit of Comet 1695; and though I got elements approaching Burckhardt's, it was evident the observations were affected with too large errors to allow of a close approximation to the orbit."

It appears, then, that this comet of 1880, as far as we can judge from observations which we have, is probably identical with the Comet of 1843. Prof. Weiss concludes that its period is 36·9 years, and thinks it probable that it may have been identical with the comet of 1106, which is carrying things rather far back. That comet, from the scanty accounts we have of it, was somewhat similar to this. It will be noticed that Mr. Hind here speaks of the Comet of 1695; but there was another comet in 1689, which has been supposed to be identical with the comet of 1843. With regard to the Comet of 1843, the observations seem to have been quite insufficient to determine its period with any degree of certainty. The most accurate determination appears to have been Prof. Hubbard's ellipse of 533 years. But all sorts of periods have been found for this Comet, ranging from about 7 years to 500 or 600 years; in fact, a parabola would satisfy the observations very fairly. 37 years was one of the periods suggested. So that there is great uncertainty about the period, and some uncertainty about the elements. As regards the Comet of 1689 the observations appear to give elements which are not very different from those of the comet of 1843. If these two Comets are identical, it would seem that there has been some acceleration in the period of the Comet since then, because, according to the period from 1843 to 1880, 36·9 years, the perihelion passage should have occurred in 1695 instead of 1689; but of course, with a Comet passing so near the Sun some acceleration might be expected. It is possible that this Comet of 1843 may have been identical with some Comet previous to 1689; but things are very uncertain. There were Comets in 1582 and in 1577 which it might be worth examining; but I should not like to give an opinion upon this point. Mr. Hind, perhaps, could answer better for the uncertainty in the elements and the probability of the Comets being identical. This Comet has such a large inclination that I suppose it is not likely to be much disturbed by any of the planets of our system; but passing so near the Sun it will get rather mixed up, at any rate, with the corona, if not with something still more intimately connected with the Sun; and there is no saying what changes we may have going on there.

Mr. Neison. May I ask if Mr. Christie remembers what observations were made with respect to the Comet of 1843 (which was a good deal before my time), whether good observations were obtained, whether good orbits were determined for the Comet, and what the period was supposed to be at the time? Perhaps Mr. Chambers might tell us, if Mr. Christie cannot.

Mr. Christie. I mentioned that the periods found for the Comet of 1843 ranged from 7 years to 500 years and upwards.

Mr. Neison. Those observations were not good.

Mr. Christie. As the Comet passed so close to the Sun, the observations necessarily could not fix the period with any very great accuracy. The Comet of 1843 was not observed till after the perihelion passage, when it was seen at noon time close to the Sun. The observations were rather rough, and they did not extend over a large arc of the orbit.

The Astronomer Royal. May I ask if there are any recorded observations of a Comet in 1806?

Mr. Christie. No; I looked specially at that date: that is the date at which one would expect to find a previous appearance of the Comet; but there was nothing that would answer to this Comet, between 1843 and 1695 or 1689. The very small perihelion distance marks this Comet out from nearly all those which have preceded it.

Mr. Chambers. My recollection of the observations of the Comet of 1843 is that they were very defective. I have an idea that there was a period of 3000 years got out by somebody with regard to it. At any rate there was such a wide discrepancy in the various periods that our knowledge of the Comet of 1843, with reference to its orbit, must be regarded as altogether indefinite.

Mr. Neison. So I gather, from what Mr. Christie told us—otherwise you could not get periods of seven years and of 500 years with half a dozen good observations of such a comet, as we know to be the case.

Mr. Marth. As the comet of 1668 was not mentioned, I may call to remembrance that Henderson found that the very rough observations made in 1668 at Goa, in India, could be represented within a degree by the elements of the comet of 1843. Supposing this comet of 1843 is the same as that of 1668, it would not be very wonderful that it should reappear now, after 37 years, instead of 175 years. The velocity of a body moving in the solar system depends simply on its distance from the Sun, and on the major axis of its orbit. If the velocity is reduced by a resisting medium there will be a reduction of the major axis, and there is nothing whatever unreasonable in the supposition that, however weak the corona may be, its resistance would have a very great effect upon the motion of a comet which rushes through it; so that I should not be at all surprised if it should turn out that this comet of 1880 is the same as the comet of 1843 and that of 1668, and that its revolution has been so much affected that possibly it may return in, say, 17 years.

Mr. Common. As soon as the ephemeris of this great southern comet was published, I at once instituted a search for it. I looked on several nights lately, aided by the ephemeris published by our worthy friend Mr. Hind; but, I need not say, without effect.

Mr. Chambers. Allow me to add to this Mr. Hind's account. Mr. Hind, having reviewed the facts of the case, winds up as follows:—Calculation shows that the paths of the comets of 1668 and 1689 might be tolerably well represented by the orbit of the comet of 1843, but equally well in the former case, and much nearer in the latter, by numbers altogether different, so that we can come to no definite conclusion on the subject.

Mr. Jenkins. If the elements of those two comets of 1843 and 1880 are so very similar, I cannot understand how it is that the comet of 1843 may have a period ranging from 7 years up to 3000, or practically a parabola, and yet in the one of this year you can get a very definite period like 37 years.

Mr. Christie. I may answer Mr. Jenkins first by saying that this period of 37 years is simply inferred from the perihelion passages of the comet in 1843 and in the present year, assuming that the two are identical. We have only got parabolic elements for the orbit of the comet of 1880; but those elements agree so closely with those for the comet of 1843, that the assumption of identity appears very probable; and therefore we are justified in assuming that there is a period of something like 36·9 years, or some fraction of that period. It is quite possible that the period may be half, or a third, or a quarter of 37 years, as far as the observations in 1843 and 1880 are concerned.

Mr. Marth. If the comet returned after a certain number of years and a half it would come to perihelion somewhere in July or August under circumstances that would make it invisible from the Earth, so that there may have been one or several appearances between 1668 and 1843 without being perceived. But it is possible that there may have been none, and yet that at the next revolution it may have come in 1880 after only 37 years, because the slightest difference in the resisting medium close to the Sun will have such an enormous effect upon the major axis as to cause that uncertainty in which we are at present, and it is not unreasonable to assume that the period has been so very much shortened.

Mr. Ranyard. As to the resistance offered by the corona, I should be very glad if more accurate information could be obtained. If it can be shown that this comet is identical with those which have been seen before, but that the period has changed, it may throw a great deal of light on the constitution of the corona. We know that deep down in the region of the prominences there is considerable resistance. In a remarkable observation of Prof. Young's, he saw a prominence shoot up to a height of 6′ above the photosphere; and during the ten minutes that he saw it, there evidently was an enormous velocity of the prominence-matter amounting to something like 300 miles in a second. We know what great resistance there is when a meteor enters our atmosphere. At the height of 70 miles from the Earth's surface it becomes white hot with the resistance; and one would expect that there would be from the corona some similar resistance and very great retardation; but we know nothing at all of the law according to which the matter of the corona is distributed. No doubt the great heat of the Sun will give rise to a very different law from that in our own atmosphere, and it would be a new way of attacking the great solar question of the distribution of the matter of the corona.

Mr. Marth. At perihelion the distance of the comet of 1843 from the Sun's surface was, if I remember rightly, two tenths of the radius of the Sun. If you say that Prof. Young has observed a protuberance 6′ high, it will just amount to that or more than that. Against the probability of the solar corona having such an effect, it may be said that it has not been shown by the Comet of 1680, which approached the Sun to one third of the radius, and which was observed before and after perihelion. Unfortunately there is only one useful observation of that comet made before perihelion. This single observation, made by Kirch, is reconcilable with the observations after perihelion; so that that might show that the effect of the approach to the Sun to a third of the radius has not been sensible, at least in those observations of 200 years ago. The elements of the comet of 1880, deduced from the rough places which have been furnished by the southern observers, and those of the comet of 1843 are so close as regards the node and inclination, and the distance and direction of the perihelion, that it is really difficult to surmise that they can be different comets. A resisting medium will have no direct effect upon the node and inclination, but will merely affect the velocity of the comet, and consequently the major axis and excentricity.

Mr. Common read a paper giving the results of observations of the nebula in the Pleiades made with his 3-feet telescope. He finds the direction of the Nebula near Merope differs considerably from that given by Mr. Maxwell Hall in the January Number of the 'Monthly Notices' for this year, and that there is also a detached nebula north of that star. He also mentions an observation on one night of a nebulous patch near Alcyone.

The paper was accompanied by a sketch.

Mr. Stone read a paper, in continuation of discussions that have taken place in the Society, on Mean Refractions:—In my paper of 1867 I divided the Greenwich observations into five groups. I found some very considerable residuals amounting to $-0''\cdot88$ for the group 37° 40′ to 43° N.P.D., and to $-2''\cdot02$ for the group 43°–46½°. These residuals are very considerable quantities, and a good deal of discussion has arisen in the Society with respect to the accuracy of the results. I have gone over anew the calculation of this last residual, introducing into it every one of the corrections that I have found required, and the result I obtain is $-2''\cdot10$, with a probable error of about $0''\cdot2$.

In this result I have corrected the observations for the error of the thermometer and for the error of the barometer, and any other correction that I could conceive of. So that I am certain that in this work of 1867 I did introduce all the necessary corrections throughout. I have lost the original paper, and it is impossible to assert from memory what any one did thirteen years ago. Then comes this question: If these residuals do not arise from inaccuracy in my work, what do they arise from? They must arise, so far as I can see, from one of three other causes. If my work be accurate, as I say now it is, then they must arise from systematic error in the Greenwich results or from Bessel's refractions requiring correction either for the constant of refraction or for imperfections of his theory. With regard to systematic errors which are running through the period from 1857 to 1865, I have no hesitation in saying there are no such quantities as these. You may get $0''\cdot5$ or $0''\cdot6$; but when you have got that you have got the very extreme.

With regard to the observations from 1868 to 1876, not only is it the case that they are affected by systematic errors, which were much larger than those from 1857 to 1865, but during the years 1870 to 1874 there is a large correction to the nadir-point as found with the reflecting eyepiece, and the whole of those observations with the reflecting eyepiece have been virtually rejected at Greenwich. I do not say that this is not justified, but it does introduce a certain amount of doubt. But still even that result is partially compensated by the quantities themselves, and you have nothing like the quantities mentioned above to deal with.

You can account for those quantities in the way I proposed in 1867, and thus reduce those large residuals to about 0″·1. And the same thing applies to the observations made from 1868 to 1876; if you take the whole of the groups, the residuals, as a matter of fact, are also reduced to small quantities.

The Cape observations made quite recently do not appear to show that Bessel's refractions require diminution, and on that account I did not in the Cape catalogue of 1860 diminish the refractions. I saw that they agreed pretty fairly with the Greenwich observations, and I thought it was better not to introduce the diminution of the refraction till the matter had been further investigated.

We should be obliged to accept the refractions, only I find that Bessel's theory contains another constant which he has introduced to explain the law of the diminution of density of the atmosphere.

I have gone into this question, making the refraction agree with observation for 85° Z.D., and the result is that there is considerable change for all zenith-distances after 75° Z.D. They agree with Bessel's up to 75°, but at 75° they differ by 0″·14, at 76° by 0″·16, and so on progressively till at 80° they differ by 0″·46, and at 82° by 0″·93, and at 85° by 0″·97.

I pick up thus an investigation which Mr. Main made a good many years ago. From the discussion of observations from 1834 to 1854 he found that the refractions at 82° were too big by about ·0023, and at 85° by about ·0048. That is just about this quantity. Not only does this account for that result of Mr. Main's investigation, but it accounts for the result obtained by M. Bakhuyzen.

It appears to me that there is this difficulty in Bessel's theory, that you get for the coefficient of refraction of air a value which does not agree with Biot's experiments. The explanation generally given of that is that Biot's value of the coefficient is for dry air, and Bessel's coefficient is determined from the moister air of the atmosphere; but I do not think that can be considered a satisfactory solution, because in moister air the physical refraction should be greater. I cannot see that there is any explanation but one, and that is that the fundamental assumption in Bessel's theory regards the atmosphere as extending into infinity; but if you suppose the refractive index at the boundary of the atmosphere to have a small sensible value, you can make Biot's value of the coefficient of refraction agree with Bessel's refraction, which agrees pretty fairly with the observations at 45° Z.D. Unfortunately that is not the case. It would appear that Bessel's value is a strained one, whatever one likes to call it; his constant has not a physical meaning, and consequently there must be an outcrop at large zenith-distances.

I think I have shown that Bessel's theory, if you accept it, of physical refraction, requires these corrections, and that these corrections are sufficient to account not merely for the results in the paper of 1867, but also for the corrections in use for many years at Greenwich, rather than those that reduced Bessel's refractions at 82° by ·0023, and then assumed that they remain unaltered till you come to 85°, which is impossible—the curve must be continuous.

Mr. Downing. Might I ask Mr. Stone how he reconciles his theory of the necessity of the diminution of the mean refractions with the comparison he has exhibited between the places of the 'Nautical Almanack' stars in his great Catalogue and the catalogue in the 'Nautical Almanack' for 1880? His comparison shows that there is no appreciable error in the refractions; and the 'Nautical Almanac' places for 1880 are taken from the Greenwich Catalogue for 1860, in which the old refractions are used—namely Bessel's, undiminished as far as Z.D. 82°.

Mr. Stone. The explanation to a very great extent is that those errors in the refractions do not get very large till you get to 82° Z.D. Then Bessel's refractions are not used; the Greenwich ones are used.

Mr. Glaisher read a paper by Lord Lindsay on the relative star-magnitude of Mars in February and March 1880.

Mr. Brett. Did the author say what machine he used?

Mr. Ranyard. A photometer.

Mr. Christie. I have a note from Mr. Brewin on the rotation-period of Jupiter. He had observed the red spot in Jupiter from the 7th of August, 1879, to the 4th of February, 1880, making 437 rotations; and he deduces from his observations the rotation-period of $9^h\ 55^m\ 34^s{\cdot}1$, agreeing closely with the value $9^h\ 55^m\ 33^s{\cdot}91$ found by Mr. Pratt ('Monthly Notices,' 1880, January). Mr. Pratt's value was about 8^s larger than the previously received value.

Mr. Marth. I have received a letter from Prof. Pritchett, of Glasgow, Missouri, whose observations go down to the beginning of February. I should much like to know whether anybody has observed the passage of the middle of the red spot across the central meridian in 1878, because just now I am in perplexity. I intend for the next meeting to prepare the ephemeris for Jupiter for the present year. Last year's observations agree fairly well with a uniform motion, which is slower than that assumed in my ephemeris from old observations; but there is one observation, made by Prof. Pritchett in 1878, which certainly does not agree with this. I should be very much obliged to anybody who has made observations of the red spot in 1878, for some distinct observation of the passage of the red spot across the central meridian. If I cannot get that, I think I shall go on with the rotation-period which I have assumed for this year, and merely give the result of the observations of 1879; because, as those observations only extend to half a year, it would be rather hazardous to predict for a whole year in advance. If Prof. Pritchett's observation of 1878 is correct, it is quite clear that the motion of the spot has been retarded, in contradiction to Mr. Pratt's result for the last year, which may be reconciled with regular motion, but cannot be reconciled with Prof. Pritchett's observations of July 1878.

Mr. Christie. There must necessarily be a good deal of uncertainty about the actual rotation-period of Jupiter, because there seems some probability that this red spot has a considerable drift; and as Mr. Marth, I think, infers, that drift may be variable. It seems to me that the problem is something like that of the Sun's rotation, where we get different rotation-periods from different spots. It is not the errors of observation that we have to guard against in this case, but this irregularity in the motion of the spot itself, which is different from the general drift of the atmosphere; so that it is important not only to get observations of a prominent object like this red spot, but of as many spots of all kinds as possible.

Mr. Marth. I have published an ephemeris for years past, expecting series of observations to correct the assumed rotation. Unfortunately we have no observations forthcoming except for the last year. I have received for the last year a long series, but unfortunately these cannot be reconciled with the single observation of July 1878 which I received from Prof. Pritchett, which, however, is not a direct observation, but is merely an inference. If this is correct, then the spot had certainly a considerable retardation during last year, and we are not justified in assuming one particular rotation-period. For this reason I should be glad to receive the observations of 1878, which might throw light upon the subject. We have received from Mr. Hirst, in Australia, very good Sketches, which are in the library; but they do not allow one to form any opinion at what exact time the centre of the spot crossed the meridian.

Lieut. T. Preston Battersby, R.A., was balloted for and duly elected a Fellow.

The Meeting adjourned at ten o'clock.

Notes on Double Stars.

MR. GLEDHILL has called attention to several interesting double-star systems in the March number of the 'Observatory,' p. 352. I have measured Σ 547 on six nights since my recently published measures. The mean result of the first four is given by Mr. Gledhill in the communication referred to. The two later observations give

$$P = 16^{\circ}\cdot 1 \qquad D = 2''\cdot 27 \qquad 1880\cdot 05.$$

The small star is apparently at least 12 magnitude of Struve's scale; in two observations I have called it 12·5. If it be assumed that Struve underrated the companion half a magnitude, this, with the greatly diminished distance, would fully account for the failures to see it by Dembowski and myself. The distance is only about one half what it was when measured by Struve; and this would greatly increase the difficulty of seeing it even when both stars are small. I should regard it now about the same as BC of 40 Eridani, so far as magnitudes are concerned, but practically a little more difficult, because the distance of the components is about 1″ less than 40 Eridani.

In my measures of the new companion found to OΣ 159 (15 Lyncis), given by Mr. Gledhill in the same number, p. 353, the position-angle should be 341°·4 instead of 31°·4. The same correction should be made in my published results, one of the figures having been omitted in some way. Later measures are as follows:—

AB	P=1°·1	D=0″·62	1878·84
AC	342°·5	23″·52	1878·84

I have called the small star 13 magnitude. It will therefore probably require 10 or 12 inches aperture to show it.

Chicago, March 17, 1880. S. W. BURNHAM.

The Cosmogony of Laplace.[*]

LAPLACE's celebrated nebular hypothesis was first distinctly stated in his 'Système du Monde'[†]. The reasoning by which it is there sustained is general, and it does not appear that the author made any effort to test his theory by analysis. The law of the conservation of energy was then undiscovered, and hence data, which now seem available for a critical examination, were entirely wanting. Let us consider the hypothesis in some of its obvious aspects.

1. It is assumed by Laplace that nebulous rings were abandoned only in the vicinity of the present orbits of the planets. While I have for many years believed that the matter of the solar system originally existed in a gaseous condition, and hence that *a* nebular hypothesis in *some* form must furnish the true explanation of the planetary motions, I have more than once ventured the opinion that this assumption of Laplace is wholly unwarranted. I make a single quotation from the 'Monthly Notices' of the Royal Astronomical Society for January 1869:—

"The known facts in regard to the zone of minor planets, as well as the phenomena of Saturn's rings, seem to demand a modification of the nebular hypothesis as generally held. No reason has ever been assigned why the solar nebula should not have abandoned rings at distances intermediate between the present orbits of the planets. On the contrary, it seems highly probable that, after first reaching the point at which gravity was counterbalanced by the centrifugal force arising from the rotation of the contracting spheroid, a continuous succession of narrow rings would be thrown off in close proximity to each other, and revolving in different periods according to Kepler's third law."

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[*] From the 'Proceedings of the American Philosophical Society.' Communicated by the Author.

[†] Published at Paris in 1813.

The view thus expressed in 1868 has never been called in question, and I have seen no reason to modify it. The *ring* theory thus seems to require that after matter began to be thrown off at the equator of the revolving mass, the process should have been almost continuous until the nebula became transformed into a close system of rings presenting the appearance of a thin plate or disk. The theory would thus fail to account for the formation of the solar system as it actually exists.

2. But even if we adopt Laplace's theory of ring formation, we at once encounter difficulties no less serious. It is obvious, on the slightest examination, that the mutual attraction of different portions of a zone could have very little influence in bringing its molecules together around a common nucleus. Laplace, it is true, supposed the fragments of a ring to have been thus collected into a single planet. "Almost always," he says, "each ring of vapours ought to be divided into several masses, which, being moved with velocities that differ little from each other, should continue to revolve at the same distance from the sun. These masses should assume a spheroidal form, with a rotary motion in the direction of that of their revolution, because their inferior particles have a less real velocity than the superior; they have, therefore, constituted so many planets in a state of vapour. But if one of them was sufficiently powerful to unite successively by its attraction all the others about its centre, the ring of vapours would be changed into one sole spheroidal mass, circulating about the Sun, with a motion of rotation in the same direction with that of revolution."

In regard to the mutual attraction here referred to, it may be remarked, that two parts of the Neptunian ring on opposite sides of the Sun could produce no sensible perturbation of each other's motion. If, moreover, the fragments of any ring were distributed around the orbit with approximate uniformity, their mutually disturbing effects would nearly destroy each other. That this state of things should have obtained in the case of some of the eight principal planets is extremely probable. The theory, therefore, of planetary aggregation by the attraction between different parts of the rings, requires an indefinite antiquity of the solar system. Let us suppose, then, that the planet-forming process was due to the different velocities of the fragments into which a ring had been broken up. Take, for example, the ring which was transformed into Neptune. Let us assume that two fragments, A and B, differed in longitude by 180°, and that the mean distance of the centre of gravity of A from the Sun's centre exceeded that of B by 1000 miles. It is then easy to show that the corresponding difference of their angular velocities would not bring them together around the same nucleus in 150 millions of years. But even after *all* the fragments had thus been collected, other millions of years—assuming, with Laplace, that the united mass was still in the gaseous form—would be required for the process of condensation. The supposition we have made is not an extravagant one. In Laplace's cosmogony, therefore, hundreds of millions of years are involved in the separate history of a single planet. Is so great an implied age of the solar system admissible?

According to Helmholtz, whose theory is now generally accepted, the Sun's heat is but the transformed motion of its parts condensed or drawn together by the force of gravitation. Now, the law of the conservation of energy enables us to calculate the age of the Sun, knowing (1) the amount of solar heat radiated in a given time, and (2) the amount produced by a given contraction of the Sun's mass. It has thus been found that condensation from the distance of the nearest fixed stars to the Sun's present volume would have kept up a supply of heat equal to the present for about twenty millions of years. This estimate, it will be understood, is based on the assumption that the Sun's density is uniform from centre to surface. If, as is altogether probable, the density increases towards the centre, the age of the Sun may be considerably greater*.

3. The difficulty here presented is one of no small importance. If removed, however, we are immediately met by another perhaps still more formidable. Assuming the increase of Neptune's radius to have been uniform during the time required for the accumulation of the ring around a single nucleus, the daily superficial deposit would be less than one sixtieth of an inch, the density being equal to the present density of the planet. This extremely slow transformation of the nebulous zones would develop little heat; so that the planets would be nearly cold during the process of their formation. Laplace's theory, therefore, obviously fails to account for the origin of satellites.

4. It is easy to show that the period of a rotating nebula in the process of condensation would vary as the square of the radius. If the solar nebula, therefore, rotated once in 164·6 years when it filled the orbit of Neptune, its period when it had contracted to the orbit of Uranus ought to have been 67 years; at the orbit of Saturn, 16·7; at that of Jupiter, 4·94 years, &c., &c. This obvious inconsistency with Kepler's third law† has been noticed by astronomers, and recourse has been had to the additional supposition

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* "On the only hypothesis science will now allow us to make respecting the source of the solar heat, the earth was, twenty millions of years ago, enveloped in the fiery atmosphere of the Sun."—*Prof. Simon Newcomb in the N. A. Review, for July*, 1876.

† Let r, r', and t, t' represent the radii and periods of rotation of the solar nebula at two different epochs; then $t : t' :: r^2 : r'^2$. But by Kepler's third law, $t : t' :: r^{\frac{3}{2}} : r'^{\frac{3}{2}}$.

that the rate of variation of density from surface to centre was continually changing through all the cycles of planetary formation[*]. Till this latter hypothesis—invented to sustain the hypothesis of Laplace—shall itself have been placed on a basis of facts, the superstructure must be regarded as eminently unstable.

CONCLUSION.

It has been shown (1) that the hypothesis of Laplace gives no explanation of the immense intervals between the planetary orbits; (2) that, apart from this objection, the periods required for the formation of planets from nebulous rings are greater than the probable age of the solar system; (3) that it fails to account for the origin of satellites; and (4) that it is apparently incompatible with a known physical law. The conclusion seems inevitable that this celebrated hypothesis must yet be abandoned, or that its principal features must be essentially modified.

Bloomington, Indiana. DANIEL KIRKWOOD.

Double Stars for May.

Σ 1423. R.A. $10^{h}\ 12^{m}{\cdot}6$, Dec. +21° 10′. Mag. 8·6, 9·3.

99°·3	1″·12	1830·9	Σ.
69 ·7	·28	78·2	β.

Change in angle.

OΣ 226 *rej.* R.A. $10^{h}\ 33^{m}{\cdot}8$, Dec. +42° 9′. Mag. 7, 10.

58°·4	17″·89	1878·1	β.

Burnham in 1874 and 1878 estimated the magnitude of B as at least 11·8.

OΣ 230. R.A. $10^{h}\ 48^{m}{\cdot}1$, Dec. +21° 35′. Mag. 8, 12.

13°·4	8″·36	1878·2.

Change in angle.

OΣ 234. R.A. $11^{h}\ 24^{m}{\cdot}3$, Dec. +41° 57′. Mag. 7, 7·8.

This very difficult star was recorded oblong in 1858 and 1861, single in 1866, and oblong in 1870 by OΣ. In 1878 it was examined by Burnham, and the components found "just in contact; distance certainly under 0″·3."

151°·7	0″·27	1878·3	β.

A binary in rapid motion.

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[*] See the able and interesting memoir on the Nebular Hypothesis by Prof. David Trowbridge, in the 'American Journal of Science' for November 1864.

Mr. Burnham has lately divided the principal stars of the following objects :—

Σ 888.	R.A. $6^h\ 12^m{\cdot}2$,	Dec. +28° 31′.	A 7·5.
Σ 1780 *rej.*	13 39 ·5,	−11 49·5.	A 6·0.
Σ 2005 *rej.*	15 59 ·3,	− 5 57·8.	A 6·5.

The second (Σ 1780 *rej.*) is 86 Virginis; and Mr. Burnham has found that both stars are double in the 18½-inch refractor. He writes :—"The attendant to the large star is very easily seen, and, I think, not beyond the reach of my 6-inch; but the companion to Struve's star is very difficult, and requires a large aperture. The components taken together form the closest quadruple system known."

For other pairs see 'Observatory,' vol. ii.

JOSEPH GLEDHILL.

Mr. Edward Crossley's Observatory,
Bermerside, Halifax.

Meteor Notes for May[*].

MORNING observations may be instituted from about the 4th to the 21st, and from the 1st to the 15th and 25th to the 31st in the evenings. On the first few days a shower of Aquariads should be looked for before sunrise; and during the first half of the month, when the sky will be generally free from moonlight, many radiant-points are observable from the region of Draco, Lyra, Serpens, and Hercules. Showers of slow meteors from the points 209° −8° and 229° −4° should be specially looked for. A morning shower, detected by the writer on May 15–17, 1877, at 354° +41° (and confirmed by Heis at 355° +42°, May 20–29), also requires further observation, and there is evidence of a rich shower in Quadrans (233° +47°) on the 3rd and few following nights. A large meteor was seen on May 30, 1877, by Mr. Corder at Writtle, and by the writer at Bristol, from a radiant at 20° +57° (near δ Cassiopeiæ).

Recent Observations.—A fine meteor, > ♃, was seen by the writer at Bristol on March 29, $7^h\ 57^m$. Path exactly noted from 247° +42° to 200° ±0° = 59°, which it traversed with an extremely slow motion in 7 or 8 seconds. The nucleus shone with a very intermittent light; at mid-course it seemed near extinction, but suddenly regained its previous brightness, which was then sustained until the end point. The same meteor was seen by an observer at Hampstead Heath, N.W. ('Nature,' April 1, 1880, p. 515), where the course was from "nearly north to south, passing near ζ Ursæ Majoris, and disappearing suddenly nearly over δ Leonis. Colour greenish white, like burning zinc, with trace of a reddish train, but no track visible afterwards." A projection of the two paths gives a radiant close to ψ Cygni (about 297° +52°); but it is doubtful whether the expression "nearly over δ Leonis" for the point of extinction at Hampstead means *over* that star in altitude or in the line of sight. This meteor is also described in the 'Natural History Journal' for April 15, p. 55, by Mr. John Sterry, Red Hill, Surrey. The time was noted as 7.56 P.M. The meteor's first appearance was very near the pole-star (about 3° below), and it passed over ζ Ursæ Majoris, disappearing gradually after having travelled about 40°. The colour was a beautiful pale blue. The brilliancy of the head was quite = ♃, and the tail was fully 2° long. Mr. Clark, from a sketch by the observer, makes the path from 275° +85° to 190° +25°, or 55° in length. It lasted 4 seconds. The course here given does not accord with the radiant in Cygnus derived from the observations at Bristol and Hampstead.

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[*] For further notes on May showers see 'Observatory,' Vol. ii. p. 31, and Vol. iii. p. 21.

A fine meteor was seen on April 4, $10^h\ 7^m$. At Greenwich the path was noted from 63° +38° to 74° +41°. End point accurately observed at η–ζ Aurigæ, direction towards β Aurigæ. Motion very slow. The meteor was greatly foreshortened, so as to be almost stationary, and left a broad streak in its track. Duration 2 secs., probably more.

At the same time a bright meteor was recorded at Bristol with a course from 293° +69° to 246° +53°. It left a broad comet-like train, and moved slowly. At Leicester four meteors were seen by Mr. F. T. Mott ('Nature,' April 8, 1880, p. 537), between 10^h 5^m and $10^h\ 25^m$ the same night, as follows:—

1. Crossing Cassiopeia at an angle of 45° with the horizon from the direction of Ursa Minor. Path 20°. Duration 2 secs. Train.
2. From 10° E. of Capella to 10° below Regulus, nearly parallel with the horizon, passing below Castor and Pollux and above Procyon. Path about 80°. Duration 5 secs., leaving a train, which broke up into several fragments before its final disappearance, the fragments travelling one behind the other in the same direction.
3. From 10° W. of Castor and Pollux to 5° W. of them, leaving a train. The trains of these three meteors were all of the same bluish-white colour.
4. In exactly the contrary direction, just above Castor and Pollux. Course about 5°, leaving no perceptible train.

On April 7, $8^h\ 17^m$, a 1st-mag. meteor was observed by Mr. Dunkin at Blackheath[*]. It passed across Monoceros from 95° +5° to 125° −5°. Duration about 2 secs. No train. This meteor, together with that of April 4, $10^h\ 7^m$, observed at Greenwich and Bristol, appears to have radiated from the E. region of

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[*] Seen also near Oxford at $8^h\ 18^m$ by a correspondent of the 'English Mechanic,' who says it "travelled an immense distance across the sky in a wavy line," but unfortunately omits to give any particulars of its path amongst the stars.

Aries, where certainly no shower had hitherto been detected. The position is on the ecliptic some 25° following the Sun's place; and it is worth mention that the comet of 1702 (supposed to be associated with the *Taurids* of November) near its ascending node on April 22 passed close to the Earth; and its radiant-point at 43°·5 +11°·5, as calculated by Prof. Herschel, falls extremely near that of the new meteor-shower, some indication of which is to be found in the meteor-lists of foreign observers. A magnificent bolide was seen in Italy, April 3, 1869, at 9^h, slowly passing from 79° +47° to 131° +46°. On March 30, 1872, at about 8^h, Prof. Serpieri, at Urbino, noted a fine meteor with a path from 61½° −9½° to 67° −16°. Mr. Main, at Oxford, on March 30, 1870, $8^h\ 20^m$, described a meteor > ♃ falling from the zenith to a point near the horizon a little south of east. These several observations, in conjunction with the recent large meteors of April 4 and 7, indicate the region between Aries, Musca, and Taurus as the diverging centre of a conspicuous meteor-shower, visible soon after sunset on the first few nights of April; and its curious position on the W.N.W. horizon, and the fact that it may be allied to the comet of 1702 (and is therefore merely a revival of the Taurids of November), gives it additional importance, and leads to the hope that it will be attentively looked for in future years.

Prof. P. F. Denza, of the Observatory at Moncalieri, has forwarded the writer a list of 175 meteor-paths recorded at Italian stations in the months of January and February, and of 200 registered in March. These include several fine meteors comparable with Jupiter:—

1880.	Time. h m	From ° °	To ° °	
Jan. 4..	5 30	212 +65	254 +55	Very slow; long streak.
Feb. 4..	10 6	0 +88	24 +67	Very slow; train.
6..	10 17	171 +44	196 +45	Slow; train.
14..	10 36	148 +19	131 +10	Slow; train.
Mar. 1..	9 8	177 +15	196 + 4	Slow.
3..	9 51	192 +54	147 − 4	Slow; long streak; 12 seconds.

On March 17, $8^h\ 5^m$, a bright 1st-mag. meteor was seen by the writer at Bristol with a slowish course from 209° +43° to 237° +52°; and on March 18, 8^h, Mr. Corder at Chelmsford saw an emerald-green meteor = Sirius almost stationary above γ Virginis for perhaps 3 or 4 secs. Its path was from 188° +2° to 189° ±0°. These meteors were evidently members of a shower from close to β Leonis (= Greg's No. 28 at 175° +14°, Jan. 8–March 31, *Virginids* I.), which has been independently found by many observers, and supplies some brilliant slow-moving meteors in March.

W. F. Denning.

CORRESPONDENCE.

To the Editor of 'The Observatory.'

The Markings on Mars.

SIR,—

I beg to send you a new memoir on the planet Mars. I see, in No. 36 of the 'Observatory,' p. 369, which I have just received, that a discussion arose at the Meeting of the Astronomical Society, on March 12, on certain details in the map of Mars. My memoir therefore comes opportunely, and I think it may be useful to send you in this letter some reflections on the subject of this discussion, which may perhaps interest your readers.

I see by this discussion that Mr. Green is inclined to receive with extreme distrust all detail which relates to the continents of Mars. It must be admitted, however, that these details *must* exist and that the principal progress of Areography will consist in discovering and fixing them. I do not find it rational to consider *à priori* and to reject as atmospheric all the peculiarities of which traces have been established on the continents of the planet. I see with pain that Mr. Green has not drawn all the details of this kind which he has observed. The smallest appearances of this sort ought to be catalogued. For my part I do not think that simple differences of tint could give rise to the appearance of a dark line—of a feature like the canals of M. Schiaparelli—at the boundary of these tints.

An eminent observer, Schröter, observed Mars for 18 years and persisted in asserting that we do not perceive the surface itself of the planet, and that all the details remarked are purely atmospheric. He asserted that if identical spots reappeared in the same region, it was because there was a disposition there to reproduce identical atmospheric conditions. Let us beware of doing now for the details which we are certainly on the way to discover on the continents, what Schröter formerly did for the general configurations. Let us rather follow the promising line of research opened by M. Schiaparelli*.

I regret that I do not know with sufficient certainty which were the bands in the neighbourhood of the Kaiser Sea, referred to by Mr. Knobel at the Astronomical Society, for I should certainly have some information to give on this point.

I am, Sir, yours faithfully,
F. TERBY.

Louvain, Belgium,
1880, April 11.

White Spots on Venus.

SIR,—

As Dr. Terby of Louvain is undertaking a study of the planet Venus similar to that which he so ably made for the planet

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* Mr. Trouvelot writes to me that he has seen the canals Scamander, Xanthus, Alpheus, Læstrygonum, Indus, Ganges, Eosphoros, Gigantum, and Titanum.

Mars, perhaps it is worth while to anticipate a memoir on Venus which I contemplate publishing some time this year. I began my studies on Venus in the fall of 1875, and have continued them till now, having made, so far, nearly 400 observations of this planet and 49 drawings. Several of the observations are interesting, and abundantly prove the existence of an atmosphere of great density and variability.

From Nov. 13, 1877, till Feb. 7, 1878, two remarkable white spots, strongly reminding me of those seen on Mars, have been observed on the opposite limbs, near the extremity of the cusps. The southern spot, which always appeared the brightest, became very prominent from Jan. 16, 1878, till Feb. 5, and appeared then to be composed of a multitude of bright peaks forming on its northern border a row of brilliant star-like dots of light. After the inferior conjunction, which occurred a few days later, the white spots were no more visible. This phenomenon has been independently seen by Mr. Seagrave of Providence, with his 8-in refractor.

I am, Sir, yours very truly,

Cambridge, Mass., March 20, 1880. L. TROUVELOT.

The Red Spot on Jupiter.

SIR,—

Since my first observation of the "Red Spot" on Jupiter, on September 25, 1878, I have made 137 drawings of this planet, 46 of which represent the red spot in various positions. It was last seen by me on February 22, 1880, at $6^h\ 6^m$, G. M. T., when its preceding end was on, or very near, the central meridian. During this period, the red spot has gone through many changes of form, but when last observed it was almost perfectly elliptical, and was surrounded by a luminous ring. Its mean period of rotation, as obtained from my observations, is $35738^s{\cdot}6$; but evidently it had a proper motion, as shown by the great irregularity of its movements. This and other particulars concerning this spot and other markings on Jupiter I intend to discuss in a future memoir.

I am, Sir, yours very truly,

Cambridge, Mass., March 20, 1880. L. TROUVELOT.

The Total Eclipse of the Moon, 1880, *June* 22.

SIR,—

Be good enough to allow me to direct the attention of foreign and colonial astronomers to the total lunar eclipse of June next.

In this eclipse the Moon may disappear completely from view. Observations of such eclipses have been very rare. Within the last 700 years there are records of only six, and of all of them it was observed that during the eclipse "nothing of the Moon was seen." These eclipses all occurred in April, May, or June.

I am, Sir, your obedient Servant,

Dulwich, April 12. B. G. JENKINS.

Brilliant Meteor.

SIR,—

Yesterday evening, about $8^h\ 15^m$, a very brilliant meteor appeared just under Sirius and rushed eastward parallel to the horizon, vanishing at about S.S.E.

I am, Sir, your obedient Servant,
ANNA STURGES BOURNE.

Crookham Knoll, Farnham,
1880, April 8.

The Nebula in the Pleiades.

SIR,—

With reference to the remarks made by Mr. Common at the last meeting of the R. A. S. on the nebula in the Pleiades, I may say that I looked for this nebula on many nights during the past winter, using my 10-inch (Calver) mirror, and I cannot say that I ever saw any thing very like what either Mr. Common or Mr. Hall has drawn; I merely caught at times a very faint glow close to Merope, chiefly north and south of it, but could not make out any thing more definite, and did not see a tail of light stretching in any direction. Of course Mr. Common's great aperture has enormous advantage in this kind of work, and we cannot hope to see all he sees or compete with him in any way. All interested in this group should read Mons. C. Wolf's very interesting paper on it, published some two or three years ago in the 'Annales' of the Paris Observatory*. Mons. Wolf sees nebulous matter almost entirely surrounding the group. I may add that I see many small stars which are not in Mons. Wolf's map, though I understand that he inserts therein all the stars he sees with at least 12 inches aperture. There is one little star especially which it seems curious he missed; it lies close to the little triangle *p* Alcyone, *sp* the most *p* star of the triangle. Mr. Johnson, of Birkenhead, has also seen this faint star, without previous knowledge of its position, with his 9-inch mirror.

I remain, Sir, yours faithfully,
JAMES L. McCANCE.

Rathfern, Rayners Road,
Putney Hill, 1880, April 16.

Occultation of ζ Cancri.

SIR,—

This was well observed here on the evening of April 17, the atmospheric conditions being peculiarly favourable. The occultation presented some special points of interest, on account of the star being double (really triple) and also as a means of determining whether a near approach (as described in the N.A.) or an actual occultation would take place.

The difference in latitude is about 5 minutes south of Greenwich. With a view to obtain the best possible definition of the dark edge of the Moon a low power (60) was used, and with this the division of the star was very distinct.

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* 'Annales de l'Observatoire de Paris,' Vol. xiv.

The following particulars were observed:—When Star No. 1 touched the Moon's edge, the light *gradually* went out, occupying apparently one second to a second and a half, then an interval of a second, when Star No. 2 touched the edge, and a gradual fading again occurred lasting perhaps half a second, followed by the sudden extinction usually observed in these cases.

The angle at which the stars met the Moon's edge was exceedingly acute, indeed nearly a parallel line; and the gradual fading of Star No. 1 may have been partly owing to this, and partly to the effect on the eye of the light thrown by No. 2. The sudden extinction of No. 2 after a momentary fading was owing to meeting a prominence on the Moon's edge.

But still it is difficult to believe that these can have been true causes, and an atmospheric hypothesis would more easily account for that which is, at any rate to me, a most unusual phenomenon.

The exact time of the occurrence was $10^h\ 40^m\ 30^s$ G.M.T.

Possibly this occultation may have been witnessed by others on or near my latitude; and if so, I shall be curious to hear with what result.

My instrument is a 6-inch equatorial by Cooke.

I am, Sir, yours faithfully,

Audley, Bath, 1880, April 19. J. L. STOTHERT.

SIR,—

Since writing the foregoing I have examined ζ Cancri, and find that the star described as No. 1 is the double (A, B), and No. 2 the single (C) of the trinary star.

My instrument elongates but does not clearly divide No. 1 (A, B).

The centre line joining the component parts of No. 1 is roughly at right angles to the ecliptic; and consequently A would entirely disappear before B touched the Moon's edge, and this would present some degree of inequality.

This combination, together with the momentary retention of the star's image on the eye, may to some extent account for the gradual extinction of light, but not wholly so.

I am, Sir, yours faithfully,

Audley, Bath, 1880, April 24. J. L. STOTHERT,

The Secular Acceleration of the Moon.

SIR,—

With reference to my investigation of the value of the Acceleration of the Moon's mean motion, the following deductions may be interesting.

In the Moon's orbit $1'' = 6000$ feet very nearly. In a century, therefore, the Moon is accelerated 60,000 feet. In the first year the acceleration is 6 feet; in the second year 18 feet additional, and so on. This is additive to the computed longitude whether before or after the epoch.

In a century the Moon's distance is changed by $\frac{60,000}{t}$ feet. Now t for 100 years is 8400, and the Moon's distance is changed in a century by $\frac{60,000}{8400}$ feet, or 7·14 feet; in one year it is changed by 0·0714 of a foot, or less than an inch. This change proceeds uniformly; for every year before the epoch the distance is additionally greater than the computed distance, and for every year after the epoch it is additionally less.

Wherever Bb occurs, we may use the value found thus:—In a century, δv or ht^2, or $-Bb\,.\,t^2$, or $-Bb\,(8400)^2 = 60,000$ feet. Therefore Bb (without respect to sign) $= \frac{60,000}{8400^2}$ feet $= \frac{6}{(84)^2}$ feet $=$ 0·123 inch.

I am, Sir, yours faithfully,

G. B. Airy.

Royal Observatory, Greenwich,
1880, April 24.

NOTES.

The Limit of the Solar Spectrum in the Ultra-Violet *.—On a former occasion ('Observatory,' No. 16, p. 127) we gave an account of M. A. Cornu's work on the ultra-violet end of the Solar spectrum. He has since continued his researches, which were primarily intended to determine the law of decrease of brightness of the Solar spectrum towards the violet and thus to throw light on the difficult question of the temperature of the Sun's photosphere; but which have been perforce directed to a study of the absorptive effect of our atmosphere—an effect so energetic that it cuts off almost abruptly the greater part of the ultra-violet in the spectrum.

M. Cornu's method of determining this atmospheric absorption is to observe the limit of the spectrum on photographs taken at different hours of the day. By adopting the same photographic process (bromo-iodized collodion and iron developer) and the same exposure throughout, strictly comparable results are obtained. As a matter of fact, the composition of the collodion and the length of the exposure have very little effect on the limit of the spectrum. From four photographs taken on the same plate M. Cornu finds that an exposure of 1^s gives 3065 tenth-metres as the limit of the spectrum, 5^s gives 3010, 20^s gives 2985, and 100^s 2975. From these results it would follow that an exposure of 400^s would only extend the limit 5 tenth-metres. Thus in clear weather there is no occasion to prolong the exposure beyond two or three minutes.

In discussing his results M. Cornu lays them down on a curve, in which the abscissæ are the wave-lengths of the limit of the spectrum and the ordinates the log sin . Sun's altitude. The thickness of

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* Comptes Rendus, vol. lxxxviii. No. 22 and vol. lxxxix. No. 20.

atmosphere traversed being inversely proportional to the sin . Sun's altitude, the curve for any one day should be a straight line, which is found to be the case. Taking results for the same altitude at different seasons, the transparency of the atmosphere for the ultra-violet seems to increase in September and October, and to continue to do so even in December and January; but this last increase may be due to the circumstance that only days of exceptional transparency are available in winter. From more than 100 photographs at various hours of the day M. Cornu gets the empirical formula, $\sin h = 0{\cdot}49 \times 10^{-0{\cdot}0036175\,(\lambda - 3000)}$, where h is Sun's altitude and λ the wave-length (in tenth-metres) of the limit of the spectrum. The extreme limit ever reached was 2930, and that was only attained twice.

In order to determine the effect of elevation above the Earth's surface on the limit of the spectrum, M. Cornu took several series of photographs at the Riffel (2570 metres about the sea) and also on the Righi (1650 metres). Considering the vaunted purity of the air on mountain heights, he was disappointed to find but a very slight extension of the spectrum, though the result agreed with the conclusion he had arrived at *à priori*. From the observations in Switzerland, it appears that the extension of the limit of the solar spectrum is only 10 tenth-metres for every 900 metres of altitude, a result quite disproportionate to the difficulties to be overcome in obtaining photographs at elevated stations. There would thus seem to be little prospect of extending to any appreciable extent our knowledge of the ultra-violet end of the Solar spectrum. The spectra of metals have been mapped very far beyond the point at which our atmosphere cuts off the spectrum of the Sun.

PHOTOGRAPHS OF SPECTRA OF STARS AND PLANETS [*].—Dr. H. Draper has, like Mr. Huggins, for some years past been devoting his attention to photography of star-spectra, and he has obtained photographs of the spectra of Vega, Arcturus, Capella, α Aquilæ, Jupiter, Mars, Venus, the Moon, &c. The spectra of Arcturus and Capella are so like that of the Sun that Dr. H. Draper has not so far detected any material differences on his photographs; but those of Vega and α Aquilæ are totally different. In the case of Vega the photograph shows eleven lines, only two of which are certainly accounted for; two more may be calcium, the remaining seven, though bearing a most suspicious resemblance to the hydrogen lines in their general characters, are as yet not identified. Dr. H. Draper remarks that it would be worth while to subject hydrogen to a more intense incandescence, to see whether photographs of its spectrum exhibit any trace of these lines, which extend to wave-length 3700. In this research Dr. H. Draper used a spectroscope with two heavy flint prisms mounted on his 12-inch Clark refractor, the 28-inch reflector not being available at the time. He remarks that a refractor has an advantage for this work which does not appear at first sight. Since it is not perfectly achromatic, if the slit be placed at the focus for the ultra-violet rays, the focus for F and G will fall within, and thus a spectrum may be formed tolerably wide for F and G, and which gradually diminishes in width towards H, and becomes almost linear for M. Thus the effect of atmospheric absorption is corrected by the diminished width towards the ultra-violet, and the same exposure will do fairly well for all parts of the spectrum from G towards H and M.

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[*] American Journal, 1879, December.

THE RELATIVE BRIGHTNESS OF LINES IN THE SPECTRA OF HYDROGEN AND NITROGEN [*].—It may be remembered that some years back Mr. Huggins made some experiments to determine whether the spectrum of nitrogen might not, when sufficiently diminished in brightness, be reduced to the double line with which he had found the principal line in the gaseous nebulæ to be sensibly coincident. M. Fievez, of the Brussels Observatory, has recently made some further experiments in the same direction. Throwing an image of a vacuum-tube by means of a condensing-lens on the slit of a spectroscope with dispersive power of from two to ten prisms, and reducing the pencil by means of a diaphragm, either variable in size or movable between the lens and the image, he finds that in the case of hydrogen the lines *h* and C successively grow shorter and fade out as the spectrum becomes fainter, the F line alone remaining visible. Similarly with nitrogen, the bands i, iii, v, ii (Plücker's notation), and the lines of the band iv fade out successively, leaving only the double line corresponding to the nebular line. Thus the spectra of hydrogen and nitrogen may, when faint, each exhibit only a single line; and from this M. Fievez draws the conclusion that the gaseous nebulæ contain nitrogen and hydrogen, and that the relative invisibility of the other lines is to be explained by absorption in interstellar space. It is to be noted that the nebular line is single and the nitrogen line is double, and that Mr. Huggins has more recently found the nebular line to be coincident with one of the nitrogen lines (the less refrangible).

THE GREAT SOUTHERN COMET [†].—Dr. B. A. Gould has made a valuable series of observations of this comet at Cordoba, extending from Feb. 5 to Feb. 19. On the last-named day it could only be recognized as a scarcely perceptible whiteness in the field of the large equatoreal of 28½ centimetres (11¼ inches) aperture; and on Feb. 20 it could not be detected. No nucleus was at any time to be seen, the head appearing cloud-like and filmy, and elongated in the direction of the tail, which it did not very much surpass in brightness. Dr. Gould remarks on the great faintness of both tail and head, and the inordinate length of the tail, which was 35° long on Feb. 2, and not less than 37° long when last seen on Feb. 14. It was then seen with great difficulty, and was of nearly the same brightness throughout. It was brightest on Feb. 7 or 8, when its length was 40° and its breadth 1½°, but its brightness not superior to that of the Milky Way in Taurus. Careful drawings of its position were made on each night independently by two observers.

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[*] Bulletins de l'Académie Royale de Belgique, 2^{me} série, tome lxix. No. 2, 1880.

[†] Astronomische Nachrichten, Nos. 2307, 2308.

From the positions observed on Feb. 6, 12, and 18, Dr. Gould deduces a set of elements agreeing closely with those found by Mr. Hind from rough positions ('Observatory,' No. 36, p. 390), and bearing a striking resemblance to those found by Hubbard for the comet of 1843.

With reference to this, Dr. Gould remarks that though Prof. Hubbard's exhaustive discussion of the observations of the comet of 1843 resulted in an ellipse of 533 years, he called especial attention to the large changes in the major axis which might be made without doing violence to the observations. He also gave with the final values of each of the elements the coefficients of its variation for a given change in the eccentricity. Applying these to the case of a period of 175 years upon the hypothesis of its identity with the comet of 1668, he found that this only implied an increase of the probable error of a single observation from $\pm 8''\cdot44$ to $\pm 11''\cdot32$. Dr. Gould finds that if the same coefficients be used with the variation of the eccentricity corresponding to a period of 37 years, the probable error of a single observation is increased to $\pm 39''\cdot05$; and, furthermore, the discordances are clearly systematic. But he does not consider this a fatal objection, as systematic discordances ought to be expected if the point observed was not the comet's centre of gravity. Prof. Hubbard also had called attention to this source of uncertainty.

Dr. Gould remarks on the suspected identity of the comets of 1668, 1702, and 1843, that while the period between 1668 and 1702 was a few days less than 34 years, the average time of revolution between 1702 and 1843 would be 35¼ years, and the period has now increased to 36·9 years. He supposes that actual friction of one side of the comet against the Sun itself, or the dense portion of its atmosphere, which could so largely modify the major axis, would also affect the other elements. In conclusion, Dr. Gould suggests the inquiry whether the observations of the second comet of 1806 are compatible with such an orbit as the present one. There are also records of a comet in 1538 and of several earlier ones which, he thinks, favour the hypothesis of identity with the recent comet and of a gradually increasing period.

In view of the difficulty in accepting a short period for the comet of 1843, it is not inconceivable that there may be two comets, such as those of 1843 and 1880, moving in the same orbit. Similarity in the orbits is not conclusive of identity.

Ragoonatha Chary.—On Thursday evening last (Feb. 5) there passed away from amongst us an unassuming and distinguished native gentleman, Chintamanay Ragoonatha Chary, F.R.A.S., Head Assistant in the Madras Observatory for the last seventeen years. Attached to that institution for a period of over thirty-five years, he served in succession in every grade under the late Major W. S. Jacob, Colonel W. K. Worster, Colonel J. F. Tennant, R.E., and the present Astronomer, and won the esteem and regard of each by his intelligence, assiduity, and attachment to the pursuit he had adopted. His strict honesty and ready skill as an observer, combined with accuracy and speed in computation, and a fair and useful amount of self-acquired mathematical knowledge, rendered him, until disabled by impaired health, invaluable in the Observatory; and the chief share in the Catalogue of Stars in hand, with the Transit Circle, since 1862, comprising already over 38,000 separate observations, is due to his personal exertions, besides many other special researches of a nature not often undertaken by ordinary assistants in observatories. His first paper communicated to the Royal Astronomical Society of London, "On the Determination of Personal Equation by Observations of the Projected Image of the Sun," was dated 25th April, 1859, and was followed by other occasional contributions highly creditable to their author. He was elected a Fellow of the Royal Astronomical Society in January 1872, upon the joint recommendations of his immediate chief, the Government Astronomer, and Mr. E. B. Powell, C.S.I., then Director of Public Instruction. He was twice successfully engaged in observations of total eclipses of the sun: on the first occasion in August 1868, at Vunpurthy, in the Nizam's dominions, in independent charge of a branch expedition for the purpose; and on the second, in December 1871, at Avenashy, in the Coimbatore district. He was the first and only native of India who has yet entered the lists as a discoverer of new celestial objects, having detected two new variable stars, viz. R Reticuli in 1867, and V Cephei in 1878. As editor of the astronomical portion of the 'Asylum Press Almanac' and of a native calendar published on his own responsibility, he has been before the public for the past twelve years; and he latterly took great interest in delivering public lectures on Astronomy, with a view to enlighten his countrymen upon the subject, and to convince them of the absurdity of their notions in regard to celestial phenomena, by familiar explanations, in simple terms, of the true principles of the science, as opposed to the ignorant superstitions and rough predictions of Hindoo astrologers and empirics of the old school.—*Madras Mail.*

NEW COMET.—J. M. Schaberle, Assistant at the Ann Arbor Observatory, discovered a comet with a tail 3′ long, on April 6, at 11^{h} Washington M.T., in R.A. $7^{h}\ 20^{m}$, Dec. 84° 25′ N. The comet was observed at Pola, Strassburg, and Vienna on April 10, 11, and 13 respectively; and a circular of the Vienna Academy, dated 1880, April 15, gives the following elements and ephemeris, calculated from these observations by Dr. J. Holetschek and K. Zelbr:—

Comet 1880 *b*. Per. Pass. 1880, June 11·7666 Berlin M.T.
Long. Per. 25° 51′ 19″ Inclination 123° 40′ 13″
Long Node 254 17 15 Log. Per. Dist. 0·28667
Mean Equinox 1880·0.

Ephemeris for 12^h *Berlin M.T.*

1880.	R.A.	Dec.	Log. Δ.	Log. *r*.	Brightness.
April 30..	$6^h\ 14^m\ 43^s$	67° 37′·8 N.	0·3513	0·3019	0·86
May 4..	6 15 38	65 19·2	0·3616	0·2992	0·83
8..	6 17 2	63 9·2	0·3718	0·2968	0·80
12..	6 18 45	61 7·6	0·3816	0·2946	0·77

The brightness on April 10 is taken as 1·00.

From observations on April 26 Major Tupman finds that at that time the ephemeris required a correction of $+10^s$ in R.A. and −8′·2 in N.P.D. The comet was of about 9½ mag., with a nucleus and a tail 6′ long.

The Rev. E. Ledger will deliver lectures at Gresham College, Basinghall Street, E.C., at 6 P.M., as follows:—May 3, 4, and 5, on Jupiter's Satellites; May 7 on Saturn. The lectures will be illustrated by the lime light. EDITOR.

Astronomical Memoranda, 1880, *May*.

Sun. May 1, sets $7^h\ 21^m$, rises $16^h\ 31^m$; May 15, sets $7^h\ 43^m$, rises $16^h\ 8^m$; May 31, sets $8^h\ 4^m$, rises $15^h\ 50^m$.

Equation of time:—Sun *before* Clock, May 1, $3^m\ 5^s$; May 11, $3^m\ 48^s$; May 21, $3^m\ 35^s$; May 31, $2^m\ 31^s$.

Sidereal Time at Mean Noon:—May 1, $2^h\ 39^m{\cdot}2$; May 11, $3^h\ 18^m{\cdot}6$; May 21, $3^h\ 58^m{\cdot}0$; May 31, $4^h\ 37^m{\cdot}4$.

Moon.	*rises.*			*sets.*			*sets.*	
	h	m		h	m		h	m
May 1..	13	43	May 11..	10	15	May 21..	14	18
2..	14	2	12..	10	59	22..	14	50
3..	14	18	13..	11	34	23..	*rises.*	
4..	14	34	14..	12	2	24..	8	53
5..	14	52	15..	12	26	25..	9	58
6..	15	9	16..	12	44	26..	10	47
7..	15	29	17..	13	1	27..	11	20
8..	*sets.*		18..	13	19	28..	11	46
9..	8	22	19..	13	36	29..	12	7
10..	9	23	20..	13	56	30..	12	25
						31..	12	41

Last Quarter, May 1, $1^h\ 53^m$; New, May 8, $18^h\ 16^m$; First Quarter, May 16, $22^h\ 24^m$; Full, May 23, $18^h\ 39^m$; Last Quarter, May 30, $10^h\ 53^m$.

Mercury is a morning star, moving from greatest elongation west (on April 25) towards superior conjunction (June 2). He is in conjunction with Saturn (41′ S.) on May 6, and with Venus (25′ S.) on May 19.

Venus is a morning star approaching superior conjunction. She rises half an hour before the Sun. The illuminated portion of her disk is 0·961 on May 16, and 0·991 on June 15, and the corresponding diameters 10″·2 and 9″·7.

May 1,	R.A. $1^{h}\ 22^{m}{\cdot}8$,	Dec. 7° 4′ N.,	tr. $22^{h}\ 44^{m}$,	rises $16^{h}\ 2^{m}$
May 31,	3 45 ·7,	19 3 N.,	23 9	15 21

Mars is an evening star, moving from Gemini to Cancer.

May 1,	R.A. $6^{h}\ 50^{m}{\cdot}5$,	Dec. 24° 33′ N.,	tr. $4^{h}\ 11^{m}$,	sets $12^{h}\ 35^{m}$
May 31,	8 6 ·7	21 43 N.,	3 29	11 34

Jupiter is a morning star in Pisces. There are no phenomena of the Satellites favourable for observation at Greenwich.

May 1,	R.A. $0^{h}\ 26^{m}{\cdot}0$,	Dec. 1° 36′ N.,	tr. $21^{h}\ 44^{m}$,	rises $15^{h}\ 31^{m}$
May 31,	0 48 ·4	3 54 N.,	20 8	13 45

Saturn is also a morning star in Pisces.

May 1,	R.A. $1^{h}\ 23^{m}{\cdot}4$,	Dec. 6° 21′ N.,	tr. $22^{h}\ 41^{m}$,	rises $16^{h}\ 5^{m}$
May 31,	1 36 ·1,	7 32 N.,	20 56	14 14

Uranus is an evening star, stationary in Leo.

May 1,	R.A. $10^{h}\ 28^{m}{\cdot}2$,	Dec. 10° 27′ N.,	tr. $7^{h}\ 48^{m}$
May 31,	10 28 ·6,	10 23 N.,	5 50

Phenomena.

	G. M. T. h	m	
May 1	3		*Venus* ☌ *Saturn*, *Venus* 47′ N.
1	7		*Neptune* in conjunction.
6	2		*Mercury* ☌ *Saturn*, *Mercury* 41′ S.
16	10	47	*h* Leo Oc. D. 351°.
May 18	7	57	p^{2} Leo Oc. D. 62°.
19	16		*Mercury* ☌ *Venus*, *Mercury* 25′ S.
23	12	26	[5253] Oc. D. 78°.
	14	53	[5286] Oc. D. 32°.
25	9		*Uranus* in quadrature.

EDITOR.

Publications received:—Sir G. B. Airy, *Theory of Errors of Observations*, 3rd edition, revised (Macmillan, London, 1879)—Prof. C. A. Young, *Measures of the Equatoreal and Polar Diameters of Mars* (Amer. Journ. 1880, March)—Prof. George Forbes, *On Comets and Ultra-Neptunian Planets*—F. Terby, *Mémoire à l'appui des remarquables observations de M. Schiaparelli sur la planète Mars* (F. Hayez, Bruxelles, 1880)—*Memoirs R. A. S.* Vol. xli., Observations made during Total Solar Eclipses, collated by A. C. Ranyard—*Reports on Telescopic Observations of the Transit of Mercury*, 1878, *May* 5–6, Washington Observations 1876, Appendix II. (Washington, 1879)—D. P. Todd, *Observations of the Transit of Mercury*, 1878, *May* 5–6 (Proc. Amer. Assoc. 1879, August)—Ormond Stone, *On the Extra-meridian determination of Time* (Cincinnati Society of Natural History)—O. Lohse, *Ueber einige Eigenschaften der Gelatine mit Rücksicht auf ihre Verwendung zur Herstellung photographischer Schichten* (Potsdam, 1880)—W. T. Lynn, *Supplementary Note on Changes in the Error of Hansen's Lunar Tables* ('Monthly Notices,' vol. xl. No. 5)—*American Journal of Science*, 1880, April (J. D. and E. S. Dana, Newhaven, Conn.)—*Ciel et Terre*, Nos. 3 and 4 (F. Hayez, Bruxelles, 1880, April 1 and 15)—*Memorie degli Spettroscopisti Italiani*, 1879, Sept.–Dec. (Palermo)—*British Quarterly Review*, 1880, April (Hodder and Stoughton, London).

EDITOR.

The present position of the planet is thus determined. The mean of the four dates of aphelion disturbance by the hypothetical planet is the year 1160 A.D., and the mean longitude of the planet at these four times is 287°. From the year 1160 to 1880 it must have passed over 258°, thence its present position in its orbit is in longitude 185°.

Fairly good results have been obtained from the hypothesis which was first chosen, viz. that the planet moves in a plane inclined to the ecliptic. But even from general reasoning it is probable that if the dates fit in for an orbit inclined 53° to the ecliptic, they will do so equally well for other orbits. It is right, therefore, to see what would be the present position of the planet if it was on the ecliptic, and if it attracted each comet at the time when it came nearest to the aphelion-position of the comet. Thus we deduce a motion of 1° in 2·96 years, and get the following dates and longitudes of the planet. The latitudes of aphelia are also given, as those nearest to the ecliptic are likely to give the surest indications as to the position of the planet; the period is also given.

Comet.	Period in years.	Aphel. date.	Aphel. lat.	Aphel. long.	Planet's long.	Diff.
		A.D.	°	°	°	°
I. 1840 iv ..	350	968	−38	220	225	− 5
II. 1843 i ..	376	1655	−36	100	98	+ 2
III. 1846 vii..	400	1248	+29	340	320	+20
IV. 1861 i ..	415	409	+32	37	37	0
V. 1793 ii ..	422	1582	−47	241	..	..
VI. 1861 ii ..	419	1651	+30	96	97	− 1
VII. 1855 ii ..	493	1609	− 9	79	82	− 3

The only comets which do not fit in fairly are Comets III. and V., of which the latter has a very great latitude. It is really a bad argument, unless, indeed, we suppose it to have been influenced by the planet about 700 B.C. It seems better to reject this comet. We are certain that Comet IV. dates as far back as 409, and this gives us an excellent means of determining the rate of motion of the planet.

If, then, at the mean date of the three Comets II., VI., and VII., 1638 A.D., the planet was in the longitude there determined, viz. 92°, and if it has since travelled at the rate of 1° in 2·96 years, it must now (1880) be in the position—

Long. 174°, R.A. 11^h 40^m, N.P.D. 87°.

Having now examined the two hypotheses, both of which have wonderful coincidences, and considering the strong evidence in favour of each, Prof. Forbes proceeds to test them by examining which, if either of them, agreed with facts in the case of such a planet as Neptune, which has six comets associated with it.

Comet.	Aphel. dist.	Ω.	*i.*	π.	μ.	Date.	Period in years.	Date of Aphel.	
		°	°	°					
I.....	32	346	41	43	+	1852 iv	70	1817	
II.....	33	253	74	92	+	1812	71	1777	
III.....	34	83	44	149	+	1815	70	1780	
IV.....	34	78	85	70	+	1846 iv	72	1810	
V.....	35	310	19	79	+	1847 v	75	1810	
VI.....	35	55	18	305	−	1835	75	1798	Halley's.

Omitting Halley's comet, which has returned so often that it cannot be included in the present research, we have the following Table:—

Comet.	Period in years.	Aphel. date.	Aphel. lat.	Aphel. long.	Planet's long.	Diff.
			°	°	°	°
I. 1852 iv ..	70	1817	−33	215	..	..
III. 1815	70	1500	−39	321	329	+8
II. 1812	71	1635	+ 9	256	256	0
IV. 1846 iv ..	72	1810	−12	260	259	−1
V. 1847 v....	75	1810	−15	260	260	0

No plane passing through the Sun includes many of these positions, hence the first hypothesis must be given up.

Comets IV. and V. have about the same longitudes and dates of last aphelion. They were probably affected by Neptune on the last possible occasion; and although this is not certain, it is the best hypothesis to start from. Now from the aphelion-distances we would assume Neptune to be about 30 Earth's radii from the Sun, which gives a yearly motion of about 2°·2.

Comet II. must have been in aphelion in the years 1706 and 1635. But if in 1810 the planet was in longitude 260°, and moved over about 2°·2 a year, it would in 1635 have been in longitude 235°, which is not very far from the longitude of aphelion, viz. 256°. By assuming the planet to have a yearly motion of 2°·08, it would have been at the right spot in 1635 to influence Comet II. Comet III. was in aphelion at the dates 1780, 1710, 1640, 1570, 1500 In 1500, according to hypothesis, the planet was in longitude 329°, and the comet's aphelion longitude is 321°. Comet I. does not seem to fit in.

From 1810 to 1880 is 70 years, which, with a yearly motion of 2°·08, gives the longitude of Neptune in 1880 equal to 45°. Its longitude, deduced from the 'Nautical Almanac,' is 48°.

This remarkable agreement makes it probable that the second hypothesis (the one which assumes the planet to move in the plane of the ecliptic) is the correct one, and that the present longitude of the new planet is about 174°.

It was impossible to test the method by means of the Jupiter-group of comets, because they are not generally visible to the naked eye, and in consequence we cannot guess when they were first affected; but the behaviour of Lexell's comet would certainly have indicated the whereabouts of Jupiter.

Prof. Forbes has also, to a certain extent, studied the conditions of the more distant planet (about 300 Earth's radii). But the investigation cannot be looked upon as likely to be very accurate, since the elements of these comets of long period are not known with sufficient accuracy. There is an orbit which would embrace four of these aphelion-positions out of six, while a fifth is not far from it. This orbit has the longitude of ascending node=265°, and the inclination of its orbit=45°. This is nearly the same plane as found on the first hypothesis for the other planet; and had it not been seen from the analogy of the Neptunian comets that the second hypothesis was the correct one, this would have been taken as evidence that for these external planets a new plane supplants the ecliptic. The present position which is deduced for this external planet is:—R.A. $22^h\ 0^m$, Dec. 39° N.; but this is not at all likely to be an accurate determination, and it is only with respect to the other planet that Prof. Forbes feels confident as to the present position.

He has searched through a number of star-catalogues to see whether any star has been observed in the position of the nearer of these two planets, and which has not since been observed. The only star as yet found fulfilling these conditions is No. 894 of the 'Greenwich Seven Year Catalogue,' 1860. Two observations were made both of R.A. and N.P.D., from which its position was—R.A. $11^h\ 19^m\ 14^s{\cdot}35$, N.P.D. $85^\circ\ 3'\ 23''{\cdot}57$, whilst from the cometary calculations the hypothetical planet would have been in R.A. $11^h\ 8^m$, N.P.D. 85°. If, then, the star above mentioned be the planet, the cometary calculations are in error to the extent of 11^m, or nearly three degrees, which is quite possible.

Prof. Forbes concludes:—"I think that, even although we may not be absolutely certain that the position of the nearest planet is at present in R.A.$=11^h\ 40^m$ and N.P.D.$=87^\circ$, still we may feel very confident that these two planets do exist; and this consideration in itself is of great interest. The light of the Sun must take 15 hours to reach the nearest of the two planets, and 45 to reach the outer one; and α Centauri is only 750 times further from the Sun than the furthest planet, and 2240 times further than the nearest one. Considering the probably enormous mass of the stars, it is nearly certain that they must influence the motion of these two planets; and if we have the good fortune to observe either of them, a new field wherein to test the extent to which the law of gravitation holds good will be immediately opened to astronomers. Our ideas of time are in the same way extended when we think of these two planets revolving in periods, the one of 1000 and the other of 5000 years; and when we consider that some of the comets introduced by the most distant planet were influenced by that planet tens of thousands of years ago.

"In conclusion I have to say a few words about the principles of the method which has been adopted. There are two principles involved:—First, we assume that when a comet is influenced by a planet, so as to cause it to move in an ellipse, the planet tends to destroy that part of the comet's motion which is towards or from the Sun, thus causing this position to be its aphelion-position. Even were the investigation of Prof. H. A. Newton, which supports this view, found to be defective, however, we should still have the analogy of the Jupiter groups of comets, of the Neptune group, and one comet and several meteor-streams connected in the same way with Uranus. I admit, however, that the theory of the influence of planets on comets is well worthy of further investigation.

"The second principle relates to the relative position of planet and comet at the time of influence. The hypothesis to which I have given greatest weight is that which supposes the planet to be in that part of its orbit which is nearest to the aphelion-position, or the position occupied by the comet when it was so influenced. Other hypotheses might have been adopted; and considering the great distances, in some cases, between the aphelion-positions and any point in the planet's orbit, it is difficult to understand how this hypothesis agrees so exactly as it undoubtedly does with the motions of a planet at that distance from the Sun. Here again, however, in default of a complete theory of planetary influence on comets, I have felt justified in working on the hypothesis stated, because of the analogy of Neptune. By working out this hypothesis with the Neptune group, I immediately deduced the true position of Neptune to within 3°, or almost with the same precision as Adams deduced it by his splendid research on Uranus. I think it only right here to say, that when making the computations I was ignorant of the position of Neptune, or, rather, I had a notion that the planet was in quite a different part of the heavens; and I was disappointed with my work, because I thought it gave an erroneous result, until I consulted the 'Nautical Almanac,' and then I found my result correct within 3°."

On the Origin of Planets[*].

If Laplace's hypothesis of the formation of planets and satellites from nebulous *rings* cannot be sustained[†], we may conclude that each planet, at its origin, was separated from a very limited arc of the equatoreal protuberance; or, in other words, that instead of the separation of a ring, the centrifugal force produced a rupture at the point of least resistance in the equatoreal belt. From the chasm thus formed a nebulous mass was thrown out, which in process of time was transformed into the outermost planet[‡]. The tendency to separation around the equator would thus be relieved, and the ellipticity of the spheroïd temporarily diminished. Further condensation, however, would again increase the centrifugal force until another rupture or outrush similar to the first would necessarily result. The formation of planets from these nebulous masses may thus be explained without the necessity of supposing such matter to have been slowly collected from continuous rings.

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[*] Read before the American Philosophical Society, April 2, 1880. Communicated by the Author.

[†] Proc. Amer. Phil. Soc. vol. xviii. p. 324. 'Observatory,' No. 37, p. 409.

[‡] It is now believed by astronomers that the phenomena of temporary stars, such as those of 1572, 1866, and 1876, are produced by enormous outbursts of incandescent matter.

The origin of satellites is also very obviously accounted for. In short, where the ring hypothesis is encumbered with difficulties well nigh insuperable, the theory here proposed seems less open to objection. Not improbably, however, the ancient orbits of the secondary system, and perhaps also of some of the primary planets, may have differed to a considerable extent from their present dimensions, as is shown by Mr. G. H. Darwin in his "Tidal Theory of the Evolution of Satellites"[*].

DANIEL KIRKWOOD.

Double Stars for June.

FOR the most important pairs see the 'Observatory,' Vol. iii. p. 53.

Σ 3123. R.A. $12^h\ 0^m{\cdot}0$, Dec. +69° 22′. Mag. 7, 7.

Elongated by Σ and Mädler; single to De in 1862 and to OΣ in 1867. In 1832·20 Σ found it thus:—289°·7, 0″·3, elongated.

Σ 1602. R.A. $12^h\ 1^m{\cdot}1$, Dec. +69° 44′. Mag. 7·5, 9.

179°·1	14″·49	1867·5	De.
178 ·8	15 ·13	79·3	Cincinnati Obs.

Angle unchanged; distance increasing.

Σ 1608. R.A. $12^h\ 5^m{\cdot}5$, Dec. +54° 5′. Mag. 7·5, 7·7.

223°·0	11″·36	1869·1	De.

Angle unchanged since 1830; slight increase in distance.

Σ 1659. R.A. $12^h\ 29^m{\cdot}5$, Dec. −11° 23′. Mag. 8, 8·1.

AB	351°·6	27″·24	1867·6	De.
	350 ·1	27 ·39	79·3	Cincinnati Obs.
AC	69 ·2	34 ·31	67·6	De.
	69 ·8	35 ·07	79·3	Cincinnati Obs.
BC	113 ·2	39 ·01	67·6	De.
	111 ·3	40 ·08	79·2	Cincinnati Obs.

AB probably unchanged. In AC the angle has not changed, but the distance has increased about 5″ since 1832. In BC slight decrease in angle and increase of 4″ in distance since 1832.

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[*] 'Observatory,' No. 27, p. 79.

Σ 1669. R.A. $12^{h}\ 35^{m}\cdot 1$, Dec. $-12^{\circ}\ 21'$. Mag. 6·5, 6·5.

$307^{\circ}\cdot 2$	$5''\cdot 71$	1866·3	Secchi.
304 ·4	5 ·73	79·2	Cincinnati Obs.

Slight increase in angle since 1828: the measures of distance do not agree well; they range from $9''\cdot 2$ to $5''\cdot 44$, the former being by H_2 and the latter by Σ.

61 Virginis. R.A. $13^{h}\ 12^{m}\cdot 2$, Dec. $-17^{\circ}\ 38'$. Mag. 4·5, 10·5.

$345^{\circ}\pm$	$73''\cdot 0$	1783	H.
22 ·6	169 ·3	1862	Knott.

Large proper motion in A; Flammarion gives it $-0^{s}\cdot 075$ in R.A. and $+1''\cdot 04$ in N.P.D.

JOSEPH GLEDHILL.

Mr. Edward Crossley's Observatory,
Bermerside, Halifax.

*Meteor Notes for June**.

THE first half of the month is chiefly available for observations in the absence of moonlight. A radiant of large meteors near α Scorpii should be looked for on the 6th–7th; and the 12th and 29th–30th are also dates on which fireballs have been recorded with more than usual frequency. The Draconids (282° +60°), Aquilids (299° −7°, and 301° +8°), Cepheids (317° +62°), Lyrids (280° +34°), and Coronids (235° +23°) form the major showers of the month, and there are radiants in Lacerta, Pegasus, and Aquarius visible in the morning hours.

Recent Observations.—A large meteor was seen at Leicester by Mr. T. Brewin on May 2, $9^{h}\ 29^{m}$ G.M.T.; brilliancy when first seen about equal to a 4th mag. star, rapidly increasing and attended with train, until, at the time of disappearance, it exceeded Venus at maximum. Time of flight 3 sec. ±. Colour bluish white at disappearance. Mr. Brewin gives a diagram of its path from about 201° +70° to 65° +78°. The same meteor was observed by Mr. Woodd Smith at Matlock, Derbyshire, May 2, $9^{h}\ 28^{m}$, P.M. He writes:—"I was startled by the light, and looking up saw the meteor pass through the Great Bear between γ and δ, but nearer to γ, and across α, disappearing soon afterwards." The projected tracks give a radiant just below the equator in R.A. 217°, near μ Virginis, and close to the centre of the large fireball of May 12, 1878, at 214° −7°.

Mr. D. W. Barker reports† meteors unusually numerous on July 28 and 29, 1879. Between 0^{h} and 4^{h} A.M. on the 28th average number 3 in a minute, and on the 29th, in a similar interval, 2 per minute. He gives a few of the observed paths, which sufficiently indicate that the shower of Aquariads was in considerable activity, and his notes are important as confirming the marked abundancy of meteors at this particular epoch pointed out by the writer (see 'Observatory,' Vol. ii. p. 164, and Vol. iii. p. 138).

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* See 'Observatory,' Vol. i. p. 61, Vol. ii. p. 71, and Vol. iii. p. 55 for further notes on June showers.

† Monthly Notices of the R. A. S. Vol. xl. p. 364 (April 1880).

Prof. P. F. Denza has forwarded a list of 80 shooting-stars registered at Moncalieri between April 2–12. The observations include a fine bolide, seen on April 12, $9^h\ 42^m$, with a path from 30° +62° to 29° +45°, leaving a long streak and moving slowly.

Mr. E. F. Sawyer recorded 6 Quadrantids on the evening of Jan. 2 (7^h to 8^h P.M.), giving a rather diffuse radiant at about 227° +48°. The meteors were bright, long, and generally rather slow. Major Tupman's shower (No. 3) at 145° +5°, Jan. 4–11, 1870, was also confirmed on Jan. 5–10. 7 meteors (5 of which were seen during half an hour's watch on the 7th) gave a well-determined position at 140° +7°. Two showers were slightly traced in February, at 130° +22°, Feb. 6–8th, and 155° +20°, Feb. 8–9th.

The writer at Bristol watched for the return of Major Tupman's *Aquariads* on the morning of May 3, $2^h\ 15^m$ to 3^h. Only two meteors were seen, one of which, very exactly recorded, belonged to this shower. It ascended between the stars ϵ Pegasi and ζ Cygni, and indicated the radiant at a point slightly E. of Mr. Corder's position, at 334° −5°, for this shower.

A 1st mag. meteor was seen at Bristol on April 30, 10^h, path from 233½° +8° to 227° −10°, length 19°, not very swift; streak. Radiant near β or δ Draconis. On May 14, between 11^h and $11^h\ 45^m$, 4 meteors were seen, of which 3 were conformable to a radiant at 245° +64°.

W. F. Denning.

CORRESPONDENCE.

To the Editor of 'The Observatory.'

The Red Spot on Jupiter.

Sir,—

When lecturing upon Jupiter at Gresham College last February I mentioned a point in connection with the red spot and its rotation-period which, it has been suggested to me, may possibly be of some interest to your readers. I was comparing the observations and drawings of Lord Rosse in the early part of 1873, recorded in the 'Monthly Notices,' Vol. xxxiv., with the recent observations and drawing of the spot. It is well known that Lord Rosse speaks of and delineates a red portion of the surface extending from about 250° to 280° of longitude, and in a latitude which appears not to be very different from that of the recent spot. I have not, however, seen any suggestion of a possible connection between the rotation-period of this recent spot, and the coincidence of the longitude of its centre, in or about August 1879, with that of the red spot seen in the early part of 1873 by Lord Rosse, both being about 250°. The interval is about 6½ years. A difference in the rotation-period from that ordinarily assigned to the planet, sufficient to produce a change of 360° in the apparent position of the spot (or, in other words, a difference of one whole rotation in 6½ years), would give the spot the same longitude at the two epochs to which I have referred.

360° in 6½ years is, roughly, equivalent to between 55° and 56° per annum. I think the interval 6½ years is probably rather too long and that the 56° may perhaps be more nearly 60°, which would be more in accordance with the apparent proper motion of the spot as seen in 1879.

A difference in apparent Jovicentric longitude of 60° per annum would be nearly equivalent to a difference in rotation-period of 7 seconds of time, as may easily be calculated. Thus, 7 seconds is about $\frac{1}{5100}$ part of the rotation-period of Jupiter; but Jupiter rotates about 880 times in one of our years; consequently the difference of Jovicentric longitude caused by 7^s difference of rotation-period would in a year amount to $\frac{880}{5100}$ of 360°, or, roughly, to 62°.

When making this rough calculation in my lecture I ventured to compare the 7 seconds thus suggested with the 7·9 seconds difference of rotation-period calculated by Mr. Pratt, in his recent communication to the Royal Astronomical Society.

It is this comparison which I have been asked to bring before your notice.

I am, Sir, your obedient servant,
E. Ledger.

Gresham College, 1880, April 22.

Sir,—

Having enjoyed the privilege recently of looking through a note-book of old observations by that admirable man and most careful observer the late Rev. Cooper Key, M.A., of Stretton Rectory, Hereford, I found a number of exceedingly careful drawings in black and white of his early experiences as to the optical performances of various small telescopes, both refracting and reflecting. The subjects were most frequently stars, especially double stars; but there were several planetary sketches amongst them, and one of these was a view of Jupiter with not only the dark belts admirably drawn, but between them, in stronger black colour, a long oval spot. This spot, too, was so precisely the shape and size of the red spot which has of late been attracting the surprised attention of observers, that I could not but jump to the almost self-evident conclusion of their both referring to the same body, appearance, or phenomenon.

Mr. Cooper Key indeed described it as a black spot; but the telescope he observed with, only "a 4-inch Gregorian by Adams of 18 inches focal length," could not be expected, when taxed to the utmost it could bear of magnifying-power, to show any symptons of various local colour in a celestial object, after the manner of the large-apertured silver-on-glass reflectors of the present day—instruments which he was destined to have so large a hand in introducing, and rendering popular throughout the nation, afterwards.

His whole note, the drawing excepted, runs thus:—"June 4th, 1843. Jupiter had a dark horizontal black spot in the light space between his two principal belts: the latter are not well defined with less than a power of 158."

Though so short, the mention is pithy; and when we know the skill and conscientious care of the man who made it, such a note may be quite enough to raise the much-speculated-on appearance of the present "red spot" out of the category of things new, unexampled, and tending to the catastrophic, into that of long, well-ordered phenomona with a regular cyclical series of dates for appearing and vanishing again.

I am, Sir, yours faithfully,
Piazzi Smyth.

15 Royal Terrace, Edinburgh,
1880, April 26.

σ Aurigæ.

Sir,—

I have recently discovered a minute companion to σ Aurigæ of about the twelfth magnitude. The mean result of three accordant measures is as follows:—

$$P=170^{\circ}\!\cdot\!9 \qquad D=7''\!\cdot\!91 \qquad 1880\cdot14$$

Two other new pairs, each less than 2" in distance, were also found in the immediate vicinity.

I am, Sir, yours faithfully,
S. W. Burnham.

Chicago, 1880, April 17.

The Secular Acceleration of the Moon.

Sir,—

My note of April 24, printed in the 'Observatory' of May 1 (No. 37, p. 419), terminates with an error so conspicuous that I cannot imagine how it escaped me.

The value of B*b* is 0·0102 inch. Apparently I had omitted one divisor 12.

I am, Sir, yours faithfully,
G. B. Airy.

Royal Observatory, Greenwich,
1880, May 3.

The Nebula in the Pleiades.

Sir,—

In reply to Mr. McCance's very courteous communication on the Pleiades, inserted in No. 37 of the 'Observatory,' p. 418, I have the honour to send you a list of 36 stars of the thirteenth or fourteenth magnitude, which I have added (in November 1878) to my chart of the Pleiades group.

Stars added in November 1878 to the Map of the Pleiades.*

m. s.	′	m. s.	′	m. s.	′
−2 3	− 0·3	−0 10	+32·0 R.	+2 23	+ 2·8
−1 57	−11·5	−0 7	+ 5·0 R.	+2 26	0·0
−1 53	− 9·0	−0 7	+30·5 R.	+2 33	−13·3
−1 23	+12·5 R.	−0 2	+12·2 R.	+2 33	− 5·5
−1 15	+ 3·3 R.	−0 1	+21·0 R.	+2 34	− 2·1
−1 9	+ 6·0 R.	0 0	+ 0·6 R.	+2 42	− 3·4
−1 4	+ 4·0 R.	+0 2	+13·6 R.	+2 45	+ 1·6
−0 24	− 3·6	+0 9	+14·1 R.	+3 28	−12·0
−0 18	+37·1 R.	+0 49	− 4·5	+3 44	− 7·2
−0 17	+ 4·0 R.	+0 50	+ 0·4	+3 59	− 6·7
−0 16	+39·0 R.	+1 7	− 7·0	+4 13	− 8·0
−0 14	− 0·6(*a*).	+1 33	+ 2·5	+4 30	−12·5
−0 13	+ 4·3 R.				

In this list, the star noted by Mr. McCance is probably that which I have marked (*a*). Those which I marked R have been seen with the great reflecting telescope of 1·20 metre, the others with the equatorial of 0·31 metre aperture, which I made use of for my description of the group.

I have no doubt that further examination will lead to the discovery of other errors. There is, however, in this connection a rather curious fact which I think I may point out. When I examined the group of the Pleiades for the first time with our great reflector, I expected to see—thanks to the power of this magnificent instrument—a much larger number of stars than the reflector of 0·31 metre had shown me. My expectation was completely disappointed. Beyond a few small stars which the refractor would doubtless have shown me on a clear night, since they are not much fainter than the other stars already seen, the great mirror has shown me nothing new in the central region of the group.

I am led to conclude from this that the space-penetrating power of an object-glass of 0·31 metre (about 12 inches) is sufficient to attain the limit of the visible universe in that region of the sky—a conclusion which is further supported by the fact that the background of the sky appears there perfectly black in the middle of the nebulosity which surrounds the group of the Pleiades.

I have never seen the nebulosity in the neighbourhood of Alcyone of which Mr. Common speaks. I am, yours faithfully,

Paris Observatory, 1880, May 8. C. Wolf.

Sir,—

In a letter on "The Nebula in the Pleiades" in your May Number, Mr. McCance refers to a little star which "lies close to the little triangle *p* Alcyone, *s p* the most *p* star of the triangle." This star was picked up, some sixteen years ago, by the late Rev. H. Cooper Key, of Stretton Rectory, Hereford, with a 12-inch silvered-glass reflector, and was at first suspected by him to be nebulous, an idea which further observation did not, I believe, confirm. In consequence either of a letter from Mr. Key, or of an indirect notification through the Rev. T. W. Webb, I observed the star on February 6, 1864, and estimated it of about 13½ magnitude. By careful scrutiny I found that I could glimpse it suspiciously with an aperture of 5 inches. Tempel's Merope Nebula I believe I caught with my 7⅓-inch refractor on January 5, in the same year. It was very faint, stretching in a *s p* direction from Merope. I was a little puzzled by its direction as figured by Mr. Hall in the January number of the 'Monthly Notices,' till I noticed that his chart was erect, not inverted.

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* [The coordinates (R.A. and N.P.D.) are reckoned from Alcyone.—Ed.]

I may perhaps just recall the fact that M. Goldschmidt, the keen-eyed and indefatigable discoverer of minor planets, observed an extensive nebulosity surrounding the Pleiades group, and that his autograph representation of it was figured in Leverrier's 'Bulletin' of December 4, 1863.

I am, Sir, yours faithfully,
GEORGE KNOTT.

Knowles Lodge, Cuckfield,
1880, May 10.

The Markings on Mars.

SIR,—

In the 'Observatory,' No. 37, p. 416, M. Terby expresses uncertainty as to which bands on Mars I referred to in my remarks at the March Meeting of the Royal Astronomical Society.

The particular band is depicted by Mädler in a drawing, No. 12 in M. Terby's 'Aréographie,' and is further referred to at p. 101 of the same memoir:—

"Déjà en 1854 le capitaine Jacob dessine une sorte d'isthme très-indécis séparant la tache principale de ce prolongement (mer de Tycho). Cet isthme ne figure pas sur la carte de Mars, mais a été dessiné très-nettement par M. Knobel en 1873." (*Vide* Sketches Nos. 11, 12, and 13, 'Monthly Notices,' 1873.)

It may be as well to add that when my sketches were published I had never heard of Captain Jacob's drawing.

I am Sir, your obedient Servant,
E. B. KNOBEL.

Bocking, Braintree,
1880, May 12.

New Stars, or hitherto unobserved Variables.

SIR,—

Besides the new star in Canis Minor discovered November 25, 1859, I now announce the following:—

1. Star in Gemini, discovered January 28, 1880, when it was about 8·7 magnitude; it soon afterwards began to diminish in

brightness, and on May 4th it had fallen to 12·9 magnitude. It precedes B.D. 1655 +13° about $37^s \cdot 5$, and is $7' \cdot 3$ north.

2. In Boötes, discovered March 12, 1880, when it was about 9·4 magnitude. On March 19th its magnitude was 9·2. It began to diminish in the first week in April, and the rate of diminution gradually increased, so that on the 14th of May its magnitude was only 12·7. It precedes B.D. 2955 +18° about $87^s \cdot 2$, and is $0' \cdot 4$ south.

I am, Sir, yours faithfully,

Joseph Baxendell.

The Observatory, Birkdale,
Southport, 1880, May 15.

The Tails of Comets.

Sir,—

I have the honour to beg you to place the following lines in your periodical.

My division of the tails of the comets into three distinct types, the idea of which I have already published in 1876 ('Annales de l'Observatoire de Moscou,' Vol. iii. 1 livraison, p. 37), is ever more and more confirmed. I have just finished the calculations of the numerous observations of the comets of 1680, 1744, 1769, and 1880, which had enormous tails, and consequently are advantageous for the inquiry as to the numerical value of the repulsive force, and I have obtained the following results:—

The tail of the comet of 1769, observed principally by Messier, is represented by a curve, computed with the repulsive force $1-\mu=12$.

The comet of 1744, observed by Cassini, Cheseaux, Heinsius, &c., had two tails: the principal and *longest* tail (0·7 of the distance, Earth–Sun) requires likewise for its construction the force $1-\mu=13$; in the secondary tail, much *shorter*, the Newtonian force is *destroyed* by the repulsive force; that is to say, that the latter is nearly equal to 1 (unity), and the resulting effective force is equal to 0.

The tail of the comet of 1680, observed by Dörfel, Kirch, &c., is exactly constructed with the repulsive force $1-\mu=0{\cdot}95$.

Finally, for the tail of the great southern comet of 1880, observed by Messrs. Ellery, Gill, Gould, Liais, &c., I have obtained the force $1-\mu=1{\cdot}2$; that is to say, that the repulsive force in this case a little exceeds the Newtonian force.

It is nearly the same value as that which Messrs. Bond and Norton have found in 1862 for the axis of the tail of Donati's comet (1858, V), the value which is given in some outlines of astronomy, and which the calculations have given me more lately for several comets of the *second* type—for instance, for the comet of 1874.

According to the principles of mechanics it is evident that for this type (but only for it), in which the effective force approaches to 0, the hyperbolic orbits, convex or concave to the Sun, described by the particles of the tail approach in their turn to the tangents to the orbit of the comet; and these particles approximately are moved only by the velocity acquired at the moment of their separation from the nucleus.

The calculations *in extenso* of these four comets are now in the press for the first part of Volume vii. of our 'Annals.'

I am, Sir, your obedient servant,
TH. BREDICHIN.

Moscow Observatory,
1880, May 9.

NOTES.

TRANSATLANTIC LONGITUDES.—During the years 1878 and 1879 an important chain of longitudes was determined by a party of American observers under the command of Lieut.-Commander F. M. Green, U.S.N. The object was to establish the longitudes of important points in South America by means of the South Atlantic cable from Lisbon to Brazil, the telegraphic chain being Greenwich – Lisbon – Madeira – Porto Grande – Pernambuco—Bahia – Rio de Janeiro – Monte Video and Buenos Ayres, and Pernambuco—Para. For the section Greenwich – Lisbon, Porthcurnow and Carcavellas (the two ends of the submarine cable) were used as repeating stations. The comparisons Greenwich—Lisbon were made on four days, viz. 1879, June 25, 26, 27, and 29; and the separate values for the longitude of the Royal Observatory, Lisbon, found by Commander Green, are:—$36^{m}\ 44^{s}{\cdot}75$, $36^{m}\ 44^{s}{\cdot}61$, $36^{m}\ 44^{s}{\cdot}73$, and $36^{m}\ 44^{s}{\cdot}64$. The resulting longitude of Lisbon Observatory is thus $36^{m}\ 44^{s}{\cdot}68$, being $8^{s}{\cdot}54$ larger than that hitherto accepted. The discordance is, as Commander Green remarks, surprisingly large; but he feels sure that within reasonable limits his work is substantially correct. The wave and armature time was $0^{s}{\cdot}06$ between Greenwich and Porthcurnow, and $0^{s}{\cdot}43$ between Porthcurnow and Carcavellas. Commander Green has also determined the longitude of Funchal, Madeira (English Consulate), which he finds to be $1^{h}\ 7^{m}\ 40^{s}{\cdot}97$ W., being $1^{s}{\cdot}97$ larger than that determined by chronometers and adopted by the English Hydrographic Department. The complete results of Commander Green's expedition will be given in his Report, which is to be published this summer.

THEORY OF ERRORS OF OBSERVATIONS.—Sir George Airy's valuable work on this subject is so well known to Geodesists and Astronomers that we need hardly do more than mention the issue of a third edition. In this the author has substituted Thomson and Tait's elegant investigation of the law of Frequency of Errors for Laplace's. Though objection has been made to this as well as other proofs of this law, in that it postulates the existence of a law of frequency symmetrical with respect to a certain value, and that this condition might possibly not hold in some of the cases to which the principle has been applied, there can be little question of its validity in astronomical observations. A practical verification of the law in such cases is given in the Appendix, where a comparison is made of the actual numbers of errors of different magnitudes in 636 observations of N.P.D. of Polaris from 1869–1879 with the theoretical frequency. The general accordance between the actual and theoretical value is very close.

PROF. C. A. F. PETERS.—It is with regret that we have to record the death, in his seventy-fourth year, of Dr. Christian August Friedrich Peters, Professor of Astronomy in the University of Kiel. Born on 1806, Sept. 7, he became at an early age a diligent student in mathematics and astronomy, and in 1826 and following years he was attached to the Altona Observatory, assisting Schumacher principally in his pendulum and geodetical observations in the Duchy of Holstein. In 1834, Dr. Peters became an assistant in the Hamburg Observatory, and in 1839, at the request of W. Struve, he joined the staff at the Pulkowa Observatory. During his residence at Pulkowa he published his two valuable memoirs, 'Numerus Constans Nutationis,' &c. and 'Recherches sur la Parallaxe des Étoiles Fixes,' for which jointly he was awarded the Gold Medal of the R.A.S. In 1849, Prof. Peters received the appointment of Professor of Astronomy at Königsberg, and shortly afterwards the directorship of the Observatory, in succession to Bessel. In 1854, on the death of M. Petersen who had succeeded Schumacher in the charge of the Altona Observatory, Dr. Peters left Königsberg to fill his old master's place, and to carry on the publication of the 'Astronomische Nachrichten,' which had been for many years the central organ for astronomical science in Germany. Here he remained till 1872, when the observatory was transferred to Kiel, of which he still continued the director, filling also the office of Professor of Astronomy in the University. Prof. Peters was one of the foremost astronomers of our time, and he is the author of several important astronomical and mathematical investigations, besides his two standard memoirs to which we have referred. For many years he was a most diligent observer, in proof of which we have only to examine the pages of the 'Astronomische Nachrichten,' the 'Memoirs' and 'Proceedings' of the Academy of Sciences of St. Petersburg, and other scientific publications. Prof. Peters died on May 8, after some months' illness resulting from an attack of paralysis. He is succeeded in the editorship of the 'Astronomische Nachrichten' by his son, Dr. C. F. W. Peters.

Two Minor Planets have been discovered:—No. 215 Œnone, by M. Knorre on April 7, and No. 216, by M. Palisa, on April 10. The planet discovered by Prof. Peters on March 18 turns out to be the old planet Althæa No. 119. EDITOR.

Elements and Ephemeris of Comet b 1880 (*Schäberle's*)*.

By H. Martin.

Per. Pass. 1880, June 29·17 Berlin M.T.
Long. Per. 40° 12′ 27″ Inclination 123° 9′ 4″
Long. Node 256 52 57 Log. Per. Dist. 0·26302.
Mean Equinox 1880·0.

Ephemeris for 12^h *Berlin M.T.*

1880.	R.A.	Dec.	Log. Δ.	Brightness.
May 31 ..	$6^h\ 28^m\ 57^s$	52° 9′·7 N.	0·4109	0·75
June 4 ..	6 31 15	50 37·8	0·4171	0·73
8 ..	6 33 31	49 10·1	0·4227	0·72
12 ..	6 35 46	47 46·4	0·4276	0·71
16 ..	6 38 0	46 26·1	0·4319	0·70
20 ..	6 40 10	45 8·9	0·4354	0·69

From Major Tupman's observations on April 26, 27, and May 8, it appears that the error of this ephemeris is small.

In the 'Science Observer' for April, Mr. Lewis Swift describes the comet on April 17 as appearing at first sight to be all tail and no head and only 4′ in length, and he had no difficulty in seeing it in the presence of a nearly half-moon. Writing on May 5, Mr. W. R. Brooks, of Phelps, N.Y., states that he had been unable to find it with a 5-inch Newtonian. Another observer, Mr. Butler, picked it up with a 9-inch refractor, and found that it was barely visible with a 4-inch. It was discovered by Mr. Schäberle with an 8-inch silvered-glass reflector.

Mr. Hind has also computed an orbit for this comet which represents Major Tupman's observations with great exactness (*cf.* p. 433).

Astronomical Memoranda, 1880, *June.*

Sun. June 1, sets $8^h\ 5^m$, rises $15^h\ 49^m$; June 15, sets $8^h\ 16^m$, rises $15^h\ 44^m$; July 1, sets $8^h\ 18^m$, rises $15^h\ 49^m$.

Equation of time:—Sun *before* Clock, June 1, $2^m\ 22^s$; June 11, $0^m\ 33^s$; *after*, June 21, $1^m\ 35^s$; July 1, $3^m\ 38^s$.

Sidereal Time at Mean Noon:—June 1, $4^h\ 41^m{\cdot}4$; June 11, $5^h\ 20^m{\cdot}8$; June 21, $6^h\ 0^m{\cdot}2$; July 1, $6^h\ 39^m{\cdot}7$.

Moon.	*rises.* h	m		*sets.* h	m		*sets.* h	m
June 1..	12	58	June 11..	10	29	June 21..		
2..	13	16	12..	10	50	22..	8	35
3..	13	36	13..	11	7	23..	9	16
4..	13	58	14..	11	24	24..	9	47
5..	14	29	15..	11	40	25..	10	10
6..			16..	11	58	26..	10	30
7..	*sets.*		17..	12	19	27..	10	47
8..	8	57	18..	12	45	28..	11	5
9..	9	35	19..	13	19	29..	11	21
10..	10	7	20..	14	8	30..	11	40

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* Astronomische Nachrichten, No. 2310.

New, June 7, 9^h 55 ; First Quarter, June 15, $9^h\ 52^m$; Full, June 22, $1^h\ 46^m$; Last Quarter, June 28, $21^h\ 57^m$.

A Total Eclipse of the Moon, invisible at Greenwich, occurs on June 22, $1^h\ 32^m$ to $2^h\ 9^m$ G.M.T.

Mercury is in superior conjunction on June 2, after which he is an evening star, setting at the end of the month in the N.W. about $1^h\ 30^m$ after the Sun.

Venus is a morning star near superior conjunction, rising shortly before the Sun.

Mars is an evening star, moving from Cancer to Leo.

June 1,	R.A. $8^h\ 9^m{\cdot}2$,	Dec. 21° 35′ N.,	tr. $3^h\ 28^m$,	sets $11^h\ 32^m$
July 1,	9 23·2	16 37 N.,	2 43	10 16

Jupiter is a morning star in Pisces.

June 1,	R.A. $0^h\ 49^m{\cdot}0$,	Dec. 3° 58′ N.,	tr. $20^h\ 5^m$,	rises $13^h\ 42^m$
July 1,	1 5 ·7	5 35 N.,	18 23	11 51

Saturn is also a morning star in Pisces.

June 1,	R.A. $1^h\ 36^m{\cdot}5$,	Dec. 7° 34′ N.,	tr. $20^h\ 52^m$,	rises $14^h\ 11^m$
July 1,	1 46 ·1,	8 22 N.,	19 4	12 17

Phenomena.

	G. M. T. h	m	
June 1	14	1	45 Pisc. Oc. R. 255°.
2	0		*Mercury* super. conjunction.
	14	40	J. iii Oc. R.
12	14	17	J. ii Tr. E.
15	14	25	J. i Ec. D.
19	14	15	J. ii Tr. I.
20	13	3	22 Scorpii Oc. D. 150°.
22			Ecl. of Moon, invis. at Greenwich.
June 22	12	32	26 Sagitt. Oc. R. 254°.
24	14	22	J. i Oc. R.
26	15	35	κ Aquarii Oc. R. 231°.
27	14	36	κ Pisc. Oc. D. 146°.
	15	43	κ Pisc. Oc. R. 279°.
	15	45	9 Pisc. Oc. R. 314°.

Abbreviations :—J. i, Jupiter's 1st Satellite.

Oc. Occultation.	D. Disappearance.	Tr. I. Transit Ingress.
Ec. Eclipse.	R. Reappearance.	Tr. E. Transit Egress.

The angles are reckoned from the *apparent* N. point towards the right of the Moon's inverted image.

EDITOR.

Publications received :—Warren De La Rue and Hugo Müller, *Experimental Researches on the Electric Discharge with the Chloride of Silver Battery*, Part iii. (Phil. Trans. Part i. Vol. 171)—G. H. Darwin *on the Analytical Expressions which give the History of a Fluid Planet of small Viscosity attended by a Single Satellite* (Proc. R. S. No. 202, 1880)—*Science Observer*, No. 26, 1880, April (Boston Scientific Society)—*American Journal*, 1880, May—*Sirius*, 1880, April—*Popular Science Monthly*, 1880, May—*Ciel et Terre*, No. 5, 1880, May 1—*Kansas City Review*, 1880, May—*The Analyst*, 1880, May.

EDITOR.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

No. 38. JUNE 1. 1880.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, 1880, May 14.

J. R. Hind, F.R.S., *President*, in the Chair.

Secretaries: W. H. M. Christie, M.A., and
J. W. L. Glaisher, M.A., F.R.S.

The Minutes of the previous Meeting were read and confirmed.

Mr. Christie announced that 54 presents had been received since the last Meeting of the Society, including a book in MS., by the Rev. S. J. Johnson, containing charts and projections of Eclipses of the Sun and Moon from 1880 to 2000.

The thanks of the Meeting were voted to the respective donors.

The Astronomer Royal. Before reading the paper which I am about to do, I would ask permission to make a remark upon the paper which I produced at the last Meeting of the Society. Just as I was starting for this Meeting I received a note from Prof. Adams in which he points out one error in my paper, and I confess that I am quite wrong and Prof. Adams is quite right in that matter. It happens that it was in the last sentence which I wrote, and, as usually happens in last sentences, there is a greater chance of error than anywhere else. But with regard to the general treatment of the subject, I may say I have written the paper in no polemical spirit whatever. I have found a method of investigating small terms in the Lunar theory which I believe to be more accurate and more powerful than any other that has been made, and I was anxious to show to the Society its power with regard to that interesting matter, the acceleration of the Moon's mean motion. If I should discover any error in it, I shall not be slow to avow it; but I have not found any, and do not expect any. The external factor may require examination.

The Astronomer Royal then read a paper on the preparations to be made for the observation of the Transit of Venus on the 6th of December 1882.

Premising that the scientific world would not be satisfied without observations of the Transit of 1882, which would be much facilitated by the experience gained by observers in 1874, and by the extension of telegraphs, he pointed out that an important condition of success is a sufficient elevation of the Sun, say 20°, or, at any rate, 15°.

For Ingress Accelerated, stations in the Cape Colony from the Cape Observatory to Durban with an elevation of the Sun ranging from 33° to 46°, and a parallax factor of 0·65, should be occupied. As there is now complete telegraphic communication with England, it may be expected that the longitude of the Cape will be quickly ascertained, relying on that of Aden determined in 1874.

For Ingress Retarded the Astronomer Royal had formerly proposed to rely on stations on the coast of the United States and Canada; but as the elevation of the Sun would be only from 15° to 18°, he now prefers the whole chain of the West-India islands, with elevation 23° to 43°, parallax factor 0·75 to 0·85. The longitudes have been determined with great accuracy by the Americans. Bermuda with elevation 25°, parallax factor 0·90, is also very favourably situated. This phase is also visible in England; but the Sun is too low, except perhaps at the Land's End and Valentia, where the elevation is about 14° and the parallax factor 0·65.

All the stations just mentioned are equally suitable for Egress Accelerated. At Bermuda the Sun's elevation is 13°, at Charleston 22°, on the West-India islands 25°, and on the coast of North America 35°.

For Egress Retarded, although Melbourne (with elevation 10°, parallax factor 0·96), Sydney (elevation 14°), and New Caledonia (elevation 22°) are suitable, the Astronomer Royal would rely chiefly on New Zealand, elevation 30° to 35°, par. factor 0·83. Major Palmer's determination of the longitude in 1874 might be supplemented by chronometer-runs in default of telegraphic communication.

With regard to instruments, he would propose an Altazimuth for determination of time and latitude by zenith-distances of stars about 90° apart in azimuth, a 6-inch Equatorial, and a detached telescope of 4 in. or 5 in. aperture, with special provision of solar eyepieces.

In conclusion, he urged that the history of the phenomena should be carefully perused by the intending observers. Attention had hitherto been concentrated on the observations of the Transit of 1769, and those made in 1761 had been neglected, though this Transit had been more extensively observed, and perhaps by abler observers. Amongst the records would be found mention of the ring of light which had puzzled observers so much in 1874.

Mr. Glaisher read a note by *Prof. J. C. Adams* on the Astronomer Royal's investigation of the theoretical value of the acceleration of the Moon's mean motion.

The Astronomer Royal. I would rather not enter into any details of this, because it is extremely difficult. Unless Prof. Adams will go term by term into the whole formation of what I call my factorial table, it would be impossible to give an answer to it. I have taken the greatest pains to secure accuracy, and I have no doubt that I have secured accuracy. Prof. Adams relies upon methods which proceed by powers of m; I have no power of m whatever. My theory is purposely intended to avoid any dependence upon powers of m. This shows that there is a total divergence of methods, and it would be impossible, without discussing it for some hours between ourselves and a great many hours before such a Society as this, to make the comparison. I am disposed to give my theory to the world and to rely upon it. I do not find fault with Prof. Adams; and if he finds fault with me, I shall not answer.

Mr. Neison. The matter which Sir George Airy has brought before us is too important to Astronomy to be left in any uncertainty. It is not a subject upon which two rival views might be placed before the Astronomical world, for them to judge which they thought was most likely to be true. It is essential that we should know what is the real and true value; and as there is such a difference of opinion between Prof. Adams and Sir George Airy, it is a subject which would invite the attention of mathematicians to enable us to know which was really the truth. So far as I can see, Prof. Adams's paper turns upon this point:—He says he gets his correction from certain terms in the motion of the Moon, and Sir George Airy in his investigation has neglected those terms, and that that accounts for the difference between the two results. That is, of course, a subject which really requires the investigation of a mathematician to ascertain whether Prof. Adams's view is correct or not.

Mr. Freeman. I was not quite clear whether the Astronomer Royal in his investigations has taken only the numerical value of the term-coefficients, or whether he has adopted the analytical form of literal coefficients. To mathematicians I should suppose that no result would be perfectly satisfactory which did not deal entirely with the literal forms of expression that occur in the representation of the disturbances in the mean motion of the Moon. There would not be any check of a safe character on the accuracy of the result without going through a careful combination of the literal expressions that are required in the investigation of those disturbances.

The Astronomer Royal. The general principle of the method which I have adopted is not to investigate the value of any term calculated, but to investigate the alteration that is made in the value of terms by an alteration in some of the coefficients. The quantity which I have to deal with is, in any case, not a large one. In treating, for instance, the correction to be made in the large coefficient of the evection or any such term of large magnitude, the coefficient does not enter into the final operation at all, but only multiples of the alteration to be made in it. But this is to be determined by factors of the corrections of the coefficients, and those factors have been prepared with the greatest care. In those factors there is no omission of the terms which Professor Adams has pointed out. Some of those points are printed in rather a confused state, and a small part has to be supplied from manuscript; but if any Member of the Society wishes to see more of that part of my process I shall be happy to supply him in some way or other. The characteristic difference between the method which I have used and the method everybody else has used is that I do not determine a coefficient positively any more; but I take the coefficients as far as they go, and I make the subject of investigation to be, not the coefficient itself, but the correction which is required to that coefficient, which is always a small quantity, and which never requires higher powers than the first. These remarks do not apply to the external or solar factor.

Mr. Neison read a paper "On the Determination of the Solar Parallax from the Parallactic Inequality in the Motion of the Moon," containing the results of a good deal of work by himself and Mr. J. Campbell:—Parallactic inequality is a term in the motion of the Moon which has been used for the purpose of determining the Solar parallax, and if it were not for certain difficulties in the way, it would be the most satisfactory means that we have, because in this case we have the Solar parallax multiplied by a very large coefficient; so that if we could determine the value of this parallactic inequality within half a second of arc, we should be able to determine the value of the solar parallax within about one fifteenth of that amount. In the present paper we propose to investigate the results obtained by previous astronomers, and to examine whether there may not be some imperfections in the method by which they have investigated the problem which would account for the discrepancies. We have gone through the modern results with the view of ascertaining what the errors of them may be. The principal results are those by Sir George Airy, by Prof. Hansen, by Mr. Stone, and by Prof. Newcomb. As to the results of Sir George Airy, we find that there is no objection to be urged to the method, and the results obtained ought to be regarded as perfectly satisfactory, only the observations used, being many of them made at very early periods—at Greenwich from 1750 to 1851—the results are not equal in accuracy to those obtained of late years.

In the tables used there are certain small errors which will tend to make the parallactic inequality too large for many years to come; and unless those errors are corrected by proper means, the resulting value obtained will be too large for certain periods and too small for others. The paper investigates what imperfections in the tables will exert any effect upon the determination of the real value of the parallactic inequality. Then we go on to consider the results obtained by Prof. Hansen, with which nothing can be definitely done, as he has not published the methods he employed or the data he used, but simply the results.

As to Mr. Stone's result, he sent a paper to the Society in which he reduced a considerable number of observations. Unfortunately that paper was not printed, and only an abstract of it is to be found, so that we do not know the exact details; but sufficient has been published for us to show that there are systematic errors of a most important nature involved in the result obtained by Mr. Stone. The observations used were founded on the assumption that the errors of the tables of the Moon would be the same at midnight as at 6^h and 18^h.

We show that, owing to the errors in Burckhardt's tables, this is not the case; that we have large systematic errors introduced in this manner, and therefore the result obtained by Mr. Stone requires to be considerably reduced to allow for the effect of those errors.

We have taken Prof. Newcomb's results, and pointed out that there are also corrections in Newcomb's value; and his theoretical basis seems to be of insufficient accuracy.

We then proceed to a fresh discussion of all the observations made at Greenwich between 1862 and 1876. We determined the value of the parallactic inequality by the same method and on the same assumptions as Prof. Newcomb and Mr. Stone. In our critical examination of this result we found that the errors which on theoretical grounds we pointed out must exist, practically do exist. When we examined the separate results for each year, we found enormous discordances amounting to 20″ of arc, much greater than can possibly exist. Therefore, instead of adopting the approximate methods used by Prof. Newcomb and others, we adopted a more extended method, in which we allowed for all the errors likely to be in the tables. Instead of considering the errors in the table due to this parallactic inequality by itself, we assume that there may be errors due to other terms and other inequalities, and we consider what their effect will be upon the parallactic inequality. We find that our results, instead of being extremely discordant, become remarkably accordant, and we get satisfactory values. The great objection to this method is that the irradiation at the Moon's limb is greater the darker the sky; and we calculate the effect of the irradiation upon the observations, and we find values which enable us to determine the true value of the parallactic inequality. Then we calculate what the true value of the solar parallax will be, and we find that it amounts to 8″·843, with a probable error of 0″·007. That result taken by itself may be regarded as extremely satisfactory. All this goes upon the assumption that there are no errors in theory which would be likely to introduce systematic errors in the parallactical inequality. So far as we are aware no such errors are supposed to exist; but we show that, owing to the influence of the planets on the Moon, there may be such errors, and we proceed to determine their effect on Sir George Airy's results and on the other results. Sir George Airy has pointed out that there may be an inequality of about forty-six years; but he remarked that such an inequality seems extremely improbable, and that there seems no physical ground for it. Later investigations enable us to show that there are such inequalities in the motions of the Moon, one of forty-eight years and another of forty-four years.

We have thus got the following comparison of the values of the parallactic inequality, uncorrected and corrected for the forty-six-years' inequality:—

	Uncorrected.	Corrected.
Airy.............	124″·53	124″·13
Stone	123 ·84*	124 ·01
Newcomb	124 ·42	123 ·96
Neison............	125 ·03	124 ·03

We are able to draw the conclusion, that if this term really exists then the solar parallax must be 8″·773, which is almost identically the same value as the value obtained by Mr. Gill from his observations of Mars. But which value is right I cannot tell until the inequalities have been investigated.

The course hitherto pursued has been to observe the limb of the Moon; and the whole difficulty of determining the parallactic inequality arises from the fact that the irradiation at the limb varies, and no one ever knows what semidiameter to apply. We are now observing a certain crater near the centre of the Moon, and we get rid of all variations in the semidiameter, and reduce the whole problem to a very simple one indeed; and it is thus possible from far fewer observations to obtain a much better value of the parallactic inequality.

The Astronomer Royal. I would ask whether the proposed method is subject to the effect of libration.

Mr. Neison. Libration can be completely eliminated, so that it has no practical influence upon the determination of the parallactic inequality.

The Astronomer Royal. I have always been a little unwilling to enter into inferences from the parallactic equation, because I conceive it to be in any case subject to much doubt of a constant character, from the circumstance that the great majority of observations of the Moon are taken between six hours on one side of opposition and six hours on the other side; and in both cases the parallactic inequality and the variation combine with the same sign, and that represents the uncertainty of the ultimate determination of either of the coefficients.

Mr. Stone. Might I ask whether Mr. Neison has allowed for the different values of the term 3(☽ — ⊙) used in the different Lunar Tables? I cannot conceive how Mr. Neison has got a probable error amounting to 0″·007. The determination of the semidiameter of the Moon is a point of very great difficulty indeed. I am sure every practical astronomer who has observed the Moon or attempted to discuss the results of observations has arrived at that conviction. I may have deceived myself with reference to Mr. Neison's correction for irradiation; but it seems to me not to explain the discordances at all. His value of the Solar Parallax is uncommonly like my result. I can hardly say what the difference is. At the time I brought it forward I felt very great difficulty in knowing what value of the semidiameter to adopt for the reduction of observations, and this is the chief cause of uncertainty in my result. As to the value which Prof. Newcomb has brought forward, he has applied some considerable corrections which have sensibly reproduced my result.

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* [*Note by Mr. Stone.*—My value is 125″·36, and not that given by Mr. Neison.]

Mr. Neison. With regard to this correction of the semidiameter, formerly alluded to by me, I mentioned at the time that it was purely empirical, and only meant to represent certain observations; but nothing of that kind is used in the present investigation. Sir George Airy's remark about the parallactic inequality and the variation having the same sign is perfectly true; but it will be seen that we have completely eliminated any error due to that. What Mr. Stone says about the term 3(☽ – ☉) struck us at once, and we point out the absolute necessity of taking this term into account. I was under the impression that Mr. Stone had not taken it into account in his paper, as nothing of the kind is mentioned in the abstract that is published; but if not it will introduce a systematic error. In our paper we take into account not only the error resulting from that term, but from half a dozen more. Then, as to the semidiameter, the effect of irradiation is determined from the observations themselves, when those observations are corrected for the errors in Hansen's tables, in one case depending upon a very important term which does not appear in his tables at all, but which has since been discovered.

There are several terms not included in Hansen's tables which produce an amount of systematic error in the diameter of the Moon; but when these are taken into account the results agree with wonderful accordance. The other questions which have been put by Mr. Stone he will find completely answered in the paper, when he has read it. Sir George Airy too will find that the liability to error from libration which he has pointed out has been taken into account, and the most effectual means taken to guard against not only those errors, but some half a dozen more which have not been alluded to, and which, unless taken into consideration, would produce very grave discordances.

The President read a paper giving the elements of Schäberle's Comet 1880 *b*. He said:—It is a comet of somewhat unusual interest, because it has been discovered rather early in its appearance. It is not unlikely that we may see it until January next; and there will be a very excellent opportunity of working out the nature of the orbit, although the perihelion-distance is not less than 1·8. After the perihelion passage on July 1 it will again come under observation early in August in the north-east before sunrise. The brightness will increase from the beginning of August till about November 3, when the comet will arrive at its least distance from the earth (1·6).

I have been greatly assisted in getting out these elements, which appear to be unusually precise, by an observation made by Major Tupman on May 8, when he was fortunate enough to witness the passage of the comet within about 45″ of a star well observed by Argelander and Lalande. The observations are quite exceptionally accurate; and I can rely on these elements to give the Society something like an outline of what the future course will be.

I will now refer, in connexion with the Great Southern Comet, to the comet of 1695, which was supposed to be identical with the great comet of 1843, and my attention was called to it again by Prof. Winnecke.

Mr. Christie read a letter from me at the last Meeting, in which I took an opposite view; but I find, after the work I have gone through since, I must rather qualify that opinion, and that the comet of 1695 should be considered one which may possibly be found to have similar elements to those of the great Southern Comets of 1843 and 1880.

Mr. Christie. I should like to ask the President what his opinion is with regard to the comet of 1689, because three orbits have been computed for that comet, which differ considerably; and it seems conceivable that the true elements might agree sufficiently well with those of the comet of 1843. I should like to know which of the orbits he considers to be the most reliable.

The President. The comet of 1689 presented the same appearance as the great comet of 1843; but in consequence of the matter remaining very unsettled, I asked my former colleague, Dr. Vogel, to go through the discussion of the observations of that comet; and he obtained an orbit which differed entirely from that of the comet of 1843. But on looking at it lately I find there is reason to think that the places were not correct, and that they require further examination. I do not think there is as great plausibility in identifying it with the comet of 1843, as there is in the case of the comet of 1695. There is another comet, of which, unfortunately, we know very little, which appeared in 1618 under the same circumstances as the comet of 1843 and the Southern Comet of 1880. It became suddenly visible, with a very long tail; but the nucleus was lost in the solar rays, and after a few days it vanished. These have been the peculiarities of both those comets; but the book in which these observations of the second comet of 1618 are supposed to be contained has not been discovered so far as I know.

Mr. Downing read a paper on the possible ten-month period of variation in latitude:—Some years ago Prof. Maxwell examined the Greenwich observations of *Polaris* 1851–54 to see whether he could detect a variation in the resulting latitudes with a ten-month period. He states that there were slight indications of such a variation, but that more observations would be required to establish the reality of its existence. In the present paper the Greenwich observations of *Polaris* for the ten years 1868–77 are discussed, with the object of determining both the magnitude and phase of this variation, assuming its period to be 306 days. The co-latitude is formed from the observations of each month, and the method of least squares applied to determine—r, the distance between the pole of the axis of rotation and that of the axis of figure; θ, the angle which the great circle joining these poles makes with the meridian of Greenwich at the epoch 1872·0; and z, the correction to the assumed co-latitude $38^\circ\ 31'\ 21''\cdot90$. The values found for these are $r=0''\cdot075\pm0''\cdot015$, $\theta=205^\circ\pm6^\circ$, and $z=-0''\cdot089\pm0''\cdot011$. This value of θ indicates that the latitude of Greenwich was a maximum about 1872, Oct. 12, so that the result agrees remarkably with that found by Prof. Peters from the Pulkowa observations of *Polaris*. On the other hand Dr. Nyrén has found discordant results from three series of Pulkowa observations, and concludes that there is no constancy of magnitude or phase in this variation of apparent latitude.

Mr. Neison. So far as I understood Mr. Downing's paper, it means that the north pole of the Earth has a movement round the real axis of revolution in a circle 6 feet in diameter, with a period of ten months. But may I ask Prof Cayley or Mr. Downing if it has ever been investigated whether there are other terms due to the combined action of the Sun and Moon which may produce similar effect?

Prof. Cayley. So far as I know they cannot have any thing to do with it.

Mr. Marth. As regards the history of the subject, before the late Prof. Clerk Maxwell's paper was printed, twenty years ago, a Cambridge friend asked me about it. Prof. Clerk Maxwell thought it was quite a new thing; and he deduced from the Greenwich observations of 1851 to 1854 a variation in the latitude of something like half a second of arc. I replied that this question was of far too difficult a character to be decided by such observations without taking into account corrections which had not been applied. The first inquiry on this point was made by Bessel in 1820, and he satisfied himself that this term was less than a second of arc. The next was by Peters, whom Mr. Downing mentioned; he gives something like Mr. Downing's value. The pole of the figure might possibly shift round in a circle of a radius of 20 feet. Then came Clerk Maxwell. I confess I am extremely puzzled to find out how Mr. Downing found such a small probable error as $0''\cdot015$.

Mr. Christie. I think we must be pleased to find that the possible variation to which the latitude is subject is so small. If the distance of the pole of rotation from the axis of the figure is determined within a foot or so, I do not think we have much to complain of. At any rate, the results of these years cannot be alleged to be so rough as Mr. Marth would imply; and we hope that things are in a better state than he imagines.

Mr. Stone. Some years ago I tried the same question with the Greenwich observations; but I do not recollect the year, nor do I recollect the result; but it came to something very small. I cannot recollect whether mine had the same sign or not.

Mr. Marth presented an ephemeris for physical observations of Jupiter, and pointed out that if observers would note the time when a spot bisects the chord (representing its apparent path across the disk) no correction would be required on account of the spot's latitude; whilst if the time of passage across a line bisecting the interval between two lines touching the limb at the equator and parallel to them were noted, a correction would be necessary.

Mr. Christie read a paper by *Dr. Henry Draper* on a photograph of Jupiter's spectrum, showing evidence of intrinsic light from that planet. The photograph was taken on 1879, Sept. 27, $9^h\ 45^m$ to $10^h\ 45^m$ New York Mean Time. Mr. Marth had informed him (Mr. Christie) that the red spot was not visible on the disk at that time.

Mr. Ranyard. I should have liked some more details with regard to the length of the slit. On looking at the photograph it would appear that the breadth of the spectrum of Jupiter does not correspond with the breadth of the planet's disk. The edges of the spectrum are not sufficiently shaded off. They look as if they were too sharply cut. One would also like to know some further details about the instrument with which it was taken, and whether it could possibly be an optical effect instead of a physical effect. I do not see from the argument in Dr. Draper's paper that it is necessarily due to the emission of rays from the planet, and not to absorption by the atmosphere of the planet.

Mr. Christie. As regards the details, nothing is mentioned as to the height of the slit: the only thing mentioned is that there was an exposure of 50 minutes; and from the statement that the photograph was taken with a telescope of 183 inches focal length I infer that it was Dr. Draper's refractor. I believe his reflector is not at present in use.

Captain Abney. I think we must have a little more information before we can accept Dr. Draper's statement that Jupiter emits light. As you are aware, if you have an emission of light, it would first affect the rays of lower refrangibility and not those of higher refrangibility; but that is not the case in the photograph, I think. Another thing is that when a refractor is used, the focus of the telescope has a great effect, because the part which you usually focus is the most visible portion of the spectrum, viz. the part in the green or the blue, and beyond G there will be a spreading-out of the spectrum and an overlapping of one part of it. Also that will account, to a great extent, for the darkening around G when it was out of focus. Anybody who takes spectrum photographs will be aware that there is very often a variation in the intensity of the light of an image of a celestial body. Without referring to Dr. Draper's view, that Jupiter emits light, I think that the phenomenon shown on the plate is principally due to an optical and photographic defect.

Lord Lindsay. If I remember right, the instrument which Dr. Draper uses for photographing is the achromatic, corrected for the photographic rays by the addition of a third lens. I do not think that the argument that the light is from a different part of the spectrum would apply in this case, because, so far as I recollect, the photographic focus of the telescope is brought as near as possible to the visual focus.

Mr. Christie. Dr. Draper explains how in his photograph of the stellar spectra he arranges that the rays about H shall come to a focus, so that the part of the spectrum about G is spread out very much, and the intensity being reduced, the same exposure would do for different parts of the spectrum, counteracting in that way the different actinic effects. He relied upon this want of achromatism as an advantage. I think this photograph of the spectrum of Jupiter was taken under the same conditions as the photographs of the stellar spectra. Of course these details are not very explicitly stated in the paper; but this is an inference from his previous paper.

Captain Abney. The phenomenon seen in that photographic spectrum of Jupiter is by no means a rare one, because I have often got in it photographic spectra of the Sun with a real image of the Sun upon the slit. It has nothing to do with the spectrum itself; it is simply attributable to a photographic defect in focusing the object. If I had seen the photograph, I should have said at once that the phenomenon was a defect, and should have rejected it in consequence. If another photograph had been taken during another hour very likely that phenomenon would not have been observed.

Mr. Ranyard. The effect is similar to an over-exposed photograph. One can understand that the foci of the different rays would be in different planes and the slit of the spectroscope would not be in focus all over the spectrum. If, for example, the rays of higher refrangibility had not come to focus, the central part of the spectrum would be illuminated by light that had come from the top of the slit and from the bottom of the slit, and so would be more intense. But in this spectrum the edges are brighter at one end, whilst at the other end the centre is brighter. One would think that the same effect ought to be produced at either end. One cannot conceive that it is the effect of over-exposure leaving a more intense edge to the spectrum, because at the end of the spectrum the intensity of the light decreases. I have seen the same effect of brightness in the centre produced by solarization.

Mr. Brett. The shadows of Jupiter's satellites are usually said to be black; and though I do not pretend to assert that black does not emit rays of light, still I presume that a plate must be remarkably sensitive in order to record the spectrum emitted from a part of Jupiter shielded from the Sun's rays where his intrinsic light alone would be visible.

Captain Abney. I think the effect is not due to solarization at all. I think it is an optical photographic defect, because if it were due to solarization it would be about the line G, and not above *h*, as is seen there.

Mr. Ranyard. You could not explain how the edges of the spectrum ought to be more intensely acted upon.

Captain Abney. That is all produced, I think, from the same causes as in the solar spectrum. It is from want of proper focusing.

Mr. Christie. Any one who looks at the spectrum of a star will see that it runs off in a trumpet shape towards the violet end. That might account for the phenomenon of the edges being bright. And at the blue end the brightness in the middle may be accounted for by the intrinsic brightness of the equatoreal part of Jupiter.

Mr. Glaisher read three papers by *Mr. Tebbutt*:—(1) "Notice of the Great Southern Comet;" (2) "On the Longitude of the Observatory, Windsor, New South Wales;" and (3) "On the Variability of B.A.C. 2472."

Also a paper by the *Rev. S. J. Johnson*, "Central Solar Eclipses in Great Britain during 1000 years."

Also a paper by *Mr. D. P. Todd*, of the Nautical Almanack Office, Washington, "On a Mechanical Attachment for Equatorial Mountings to facilitate sweeping in R.A."

Mr. Common. It would be interesting to know whether the principle brought forward by Mr. Todd has priority over the principle brought out by Severne and published in the 'Times' last year. The practical application was shown at the Conversazione of the Royal Society a few weeks ago. Two pieces of platinum wire were so hung that a compass-needle touching either of them would show where there was any deviation from the course. It is called a tell-tale compass, and a great deal was made of it. This is identically the same thing. It tells you when there is a certain deviation of the vernier of the circle on either side. It appears Mr. Todd invented this in 1877, and Severne's was published as a new principle in the autumn of 1879.

Lord Lindsay. Is there any thing new in having a check put upon the telescope in one direction?

Mr. Common. It is not a check; it is a tell-tale.

Mr. Christie. In this case it is simply the application of that principle to this particular work of sweeping in R.A., and the suggestion of a practical improvement. There is no particular novelty in the principle.

Mr. Bosanquet exhibited and described the working of a precessional globe which he had himself constructed for calculating the effects of precession for every particular star. He was not aware till that evening that a globe of a similar character was in the possession of the Society.

Mr. Christie. We have also a similar globe at the Greenwich Observatory which the Astronomer Royal had made some thirty or forty years ago; and we have often found it useful when we are troubled with questions on Babylonian and Egyptian astronomy, as to the heliacal rising and setting of stars, and so on. Now that Mr. Bosanquet has made this globe, we may hope that these questions will be addressed to him, as he has entered so fully into the subject, and is so much more competent to answer inquiries.

The President. I ought not to leave the Chair without mentioning the most serious loss which the Royal Astronomical Society has sustained in the death of Prof. Peters, the Director of the Royal Observatory of Kiel, and for so long a time the editor of the 'Astronomische Nachrichten.' He died, after a long illness, on Saturday last; and it is only right that from this Chair some expression of regret should be made to the Fellows of the Society at the loss to Astronomy from his death.

Lord Lindsay. Would it be in order for the Meeting to request the Council to express their sympathy with his widow?

The President. If it is the pleasure of the Meeting, the Council will send an expression of regret and condolence to Prof. Peters's widow.

Lord Lindsay. Might I ask if any reliable information has come from the Cape about the great Southern Comet?

The President. There have been some observations from Melbourne, but the best series of observations are those made by Dr. Gould. We must rely mainly upon his observations for elements in the present year.

The following gentlemen were balloted for and duly elected Fellows of the Society:—T. Buckney, L. A. Eddie, T. Gullon, and N. Perini.

The Meeting adjourned at ten o'clock.

On Comets and Ultra-Neptunian Planets[*].

THE object of the present communication is, first, to show reasons for a belief in the existence of two planets whose orbits are greater than that of Neptune; and, secondly, to indicate the probable positions of these planets.

The whole of this research is founded upon the theory of the introduction of comets as permanent members of the solar system, which is now generally held. According to this theory, comets are bodies of size, composition, and character, which we need not at present discuss, but which move through interstellar space subject to the laws of gravitation. Every time that such a comet becomes sensibly attracted by any star, such as we have reason to suppose our Sun to be, it is attracted towards it, and tends to describe about it an orbit sensibly parabolic.

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[*] Abstract of a Memoir read before the Royal Society of Edinburgh, by Prof. George Forbes. From an advance copy.

Let us call the distance of the Earth from the Sun one Earth's radius. The orbits of comets thus attracted into the Sun's neighbourhood, though on the numerical average parabolic, may have these orbits transformed, by planetary perturbation, either into ellipses or hyperbolæ. If the comet approach a planet in such a manner that its velocity is increased, it then will describe a hyperbolic orbit in future, and will never again return to the Sun. But if the action of the planet be such as to reduce the velocity of the comet, it will then for the future revolve in an elliptic orbit round the Sun, which may have its elements altered by planetary perturbations, and which may eventually be actually in this way driven out of the solar system altogether, but which, in default of these accidental occurrences, must become a permanent member of the solar system.

It has long been known that the aphelion-distances of comets are grouped in classes at definite distances from the Sun. Thus we know that there is a large group of comets whose aphelion-distance is about the same as the distance of Jupiter from the Sun. Jupiter's distance is 5 Earth's radii, and there are 11 periodic comets whose aphelion-distance is between 4 and 6 Earth's radii. Neptune's distance is 30 Earth's radii, and there are six comets whose aphelion-distances vary from 32 to 35 Earth's radii.

Aphelion distances of all the Comets revolving in elliptic orbits.

Comet.	Aph. Dist.	Comet.	Aph. Dist.	Comet.	Aph. Dist.
Encke's	4·1	1852 iv	32·0	1811 ii	181·4
Pons'	4·8	1812	33·4	1807	285·2
1844 i	5·0	1815	34·0	1858 vi	303·8
1743 i	5·3	1846 iv	34·5	1769	322·8
1766 ii	5·5	1847 v	35·0	1840 ii	359·3
1819 iii	5·5	Halley's	35·4	1827 iii	377·4
Brorsen's	5·6	1862 iii	48·6	1846 i	388·2
Lexell's	5·7	1683	65·5	1811 i	420·7
1846 iii	5·7	1857 iv	74·9	1825 iv	533·6
D'Arrest's	5·7	1845 iii	78·9	1822 iv	617·0
Faye's	6·0	1840 iv	96·7	1680	620·0
Biela's	6·2	1843 i	100·0	1851 iii	624·0
1783 i	7·8	1846 vii	108·2	1763	754·3
1846 vi	9·4	1861 i	110·3 *	1849 iii	823·6
1858 i	11·0	1793 ii	111·0	1830 i	2971·3
1866 i	16·8	1861 ii	111·2	1780 i	3209·9
1863 v	27·6	1855 ii	124·2	1844 ii	4275·6

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* [With regard to this group of seven comets, it is to be remarked that for the Comet of 1843 Prof. Hubbard deduced an aphelion-distance 173 from *all* the observations, and 131·4 from the micrometer measures alone (period 533 years), and not 100 (period 376 years) as stated here and elsewhere. If, as

On tabulating the aphelion-distances of all the known elliptic orbits of comets*, it was found that in no case was there any grouping of aphelion comet distances which did not agree with the distances of planets, except that beyond the distance of Neptune there were two groupings of comet aphelion-distances, one at 100 Earth's radii, the other at 300 Earth's radii, approximately.

Taking Prof. H. A. Newton's theory with respect to the introduction of comets into the solar system, it would follow that the disturbing planet must, at the time when a comet was so introduced, have been somewhere near the position of the comet's aphelion. Two hypotheses then present themselves:—(1) We may suppose that the planet must have been extremely close to the comet when it influenced it, in which case it would be necessary to prove that the aphelion-positions of a fair proportion of these comets lay in one plane which passes through the Sun. In this case we could determine the date when the planet was in some definite positions, and so might predict its present position. (2) We may suppose that the planet revolved in some orbit close to the ecliptic, and assume that it attracted the comet into the solar system, when it was most near to the comet's aphelion-position.

From the elements of the seven comets ranging from Aph. Dist. 96·7 to 124·2, the longitudes (measured along the ecliptic) and latitudes of the aphelion-positions were calculated (see Table, p. 443).

These points were marked by wafers upon a celestial globe, and it was immediately evident that the four Comets, I., II., IV., and VII., have their aphelion-positions almost exactly on one great circle. The exactness of the coincidence is so remarkable that there were strong reasons for believing that the planet must have been close to each comet at the time when it deflected it from a parabolic orbit. Consequently this plane was examined on the supposition that it is the plane of the planet's orbit. The longitude of its ascending node is at 250°, and it cuts the ecliptic at an angle of 53°. This is a high inclination; but when we think of the satellites of Uranus and Neptune, which have high inclinations and retrograde motion, and of the divergence of Neptune from Bode's law, we lose confidence in a belief of the maintenance of all the general features characteristic of the nearer planets in those orbits which are near the limits of the solar system. In fact, if, as we may suppose, there are at least two planets beyond Neptune,

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appears probable, this comet be identical with the great southern comet of this year, its period must be 37 years instead of 533 years. Also for Comet 1793 ii D'Arrest found that the observations (extending over 2¼ months) were equally well satisfied by a parabola. For Comet 1855 ii Schulze deduced an ellipse with an aphelion-distance of 57 instead of 124·2 as found by Donati. As regards the other four comets, the aphelion-distances deduced by other calculators agree pretty closely with those here given.—Ed.]

* Those in G. F. Chambers's 'Astronomy' have been adopted, as his catalogue is an impartial selection of the best orbits computed.

it is quite a possibility that there may be a special plane in which they move, and which in those vast orbits supplants the ecliptic.

We now have to see whether the motion of a planet about 100 Earth's radii from the Sun with a period of about 1000 years could bring it into the position of these aphelia at the dates when the comets were there, either in the last revolution of the planet, or at least the last but one. [If the comet was caused to move in an elliptic orbit earlier than the last revolution but one, it would have returned so often to the neighbourhood of other planets that probably it would have met with planetary perturbations which would have altered its orbit entirely, and might have reduced the orbit to one like those of Comets III., V., and VI., which have their aphelion-positions not in the plane of those of the Comets I., II., IV., and VII.]

Such a planet would come into juxtaposition with the four aphelia in the course of *two* revolutions, arriving at each one at the time when the corresponding comet may have been expected to have been in aphelion, as is shown in the following Table:—

Comet.	Period in years.	Aphelion date.	Aphelion long.	Planet's long.	Difference.
		A.D.	°	°	°
IV. 1861 i ..	415	409	29	19	−10
I. 1840 iv..	493	968	203	218	+15
VII. 1855 ii ..	376	1608	82	87	+ 3
II. 1843 i ..	344±8	1655	115	105	−10

The period of revolution of the planet is taken as 1006 years.

According to this hypothesis, Comets VII. and II. were affected by the planet when it was last in these positions, Comets IV. and I. in a previous revolution of the planet. We have to suppose, then, that Comets VII. and II. have only once appeared in the neighbourhood of the Sun, that Comet I. has appeared three times altogether, and Comet IV. so many as four times. Comet I. has been supposed to be identical with Comet 1490 A.D., and it ought to have appeared before in the year 1140 A.D.—giving a period of 350 years, and dates of aphelion-passage in the years 1668, 1318, 968 A.D. Comet IV. has a calculated period of 415 years, which gives for the dates of aphelion-passage the years 1654, 1239, 824, 409 A.D.

As to Comet IV. it was visible to the naked eye in 1861, and ought to have been seen in the years 1446, 1031, 616 A.D. As a matter of fact, we find from historical accounts that comets were seen in the years 1444, 1032, and 617 A.D. All these comets were seen at about the same time of the year (midsummer), *i. e.* when the Earth was in the same position, and they were all seen to pass near to the star β Leonis. It is then shown that in the July position of the Earth we should see the Comet 1861 i pass close to the star β Leonis, and the identity of the four comets is thence concluded.

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No. 39. JULY 1. 1880.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, 1880, June 11.

J. R. Hind, F.R.S., *President*, in the Chair.

Secretaries: W. H. M. Christie, M.A., and
J. W. L. Glaisher, M.A., F.R.S.

The Minutes of the previous Meeting were read and confirmed.

Mr. Christie announced that 61 presents had been received since the last Meeting, including a photograph of the Moon by Professor Pritchard, and two photographs of the Oxford University Observatory.

The thanks of the Meeting were voted to the respective donors.

Prof. Pritchard read a paper "On the first Results of Researches connected with the Lunar Physical Libration made at the Oxford University Observatory," in the course of the reading of which he remarked on the fact that hitherto but very little attention had been paid in the Oxford Observatory to lunar photography in a purely pictorial sense, but he referred to the beautiful photograph of the Moon which he had presented to the Society as an instance of what might still there be done in that direction, and in other observatories, with improved instruments.

After describing the instruments used in the Libration research, he referred to the laborious and delicate nature of the operations required; and although absolute accuracy might not be attainable, still he considered that he had already reached to a very fair approximation to the law and the amount of the Moon's rotational inequalities. On the resumption of the Meetings of the Society in November he hoped to be able to complete the final numerical results, as well as an account of the general mathematical methods pursued in a much more extended series of computations with regard to both the spots selected on the Moon; these two spots will thus become a mutual check, and may serve to secure the truth of the case, and with the truth alone he was really concerned. "We possess," he added, "about 1200 photographs, extending through upwards of four years; I propose to calculate some of them through at least two years further. If any astronomer, competent to the task, desires to extend the computation still further, and he does not become heartily tired of the task, I do not doubt our photographs may be at his disposal. But I think the upshot of the case would be, that the ultimate result would not differ considerably from that already obtained, viz. that the Moon's rotational inequalities exist as a physical fact, and that they arise in the manner and, generally speaking, to the amount indicated by theory, viz. from the gravitational action of the Earth on the Moon, the figure of which is very approximately spherical."

Mr. Neison. I should not rise to make any remark upon Prof. Pritchard's paper, if it were not that I thought I might make some suggestions which would be of use to him perhaps in the course of his reductions. He would be in a position before completing his labours to ascertain how far the objections which I have to urge have any effect upon his investigation. First, I would remark that when Wichmann made his long series of investigations, he found that the great objection to the method was that there were irregularities at different parts of the limb; and he found that though the measures from different points on the same day agreed fairly, yet that in the results for different days the discrepancy then came into play. He found that the inequalities in the limb, which were brought into view by libration, and then moved out again, so affected the resultant measures as to render his results almost valueless. I want to point out further that there is a relation between the period of the physical libration and the periods in which these elevations and depressions are produced, which has a systematic effect for years at a time. Supposing we have an elevation and a depression alternately upon a portion of the limb for two or three years at a time, the existence of that inequality will cause an amount of physical libration in the Moon amounting to about 1″ or 1½″ of arc, just such a quantity as has been actually observed.

Prof. Pritchard remarks that he takes two points on the Moon's disk. I would point out, if there is any systematic error arising from inequalities in the limb, it would be the same at both points, with hardly any appreciable difference. The accordance of his results from the two points would be no test of accuracy, and no proof that there are not systematic errors due to irregularities on the limb. Prof. Pritchard remarks that results hitherto obtained have been in accordance. I would remark that this inequality that I draw attention to has a period of nine years. It is, in fact, the period of rotation of the lunar perigee. Supposing we take Prof. Pritchard's case, which we assume is about right, his observations are about 1878. Supposing we go back exactly four revolutions, or 36 years, we come to 1842; that is, to the epoch of Herr Wichmann's measures. The same systematic error which affected the one result will affect the other.

Prof. Pritchard. Mine were taken in 1876, I think.

Mr. Neison. That would not make any difference. However, these observations were made in 1841 and 1842. Prof. Pritchard alluded to the results obtained by Bouvard and Nicollet. But supposing we go back 36 years more (it is not quite 36 years, for the lunar perigee is not quite nine years), we come to 1806, the very time that those measurements were made by Bouvard and Nicollet; so that, if there is any error, it will affect all three. Let us take another series of measures, which Prof. Pritchard does not allude to; that is, that which was made by Nicollet in 1815 or 1816, just one period from that, bringing every one of the four under the same systematic error as I draw attention to in this case of Prof. Pritchard, so that, if there is any libration, or any inequality of the limb, that libration would be of the same kind, and have the same effect, throughout the whole four series of observations *.

Mr. Marth. If the last were in 1876, there would be three revolutions and a half instead of four revolutions.

Mr. De la Rue. I am sure that in what Mr. Neison has said he has no intention of decrying all attempts at determining this very interesting problem of lunar libration; because, unless some attempts are made from time to time with the methods that are at our disposal, we shall never be able to arrive at any conclusion respecting it. But it appears to me that Mr. Neison is in some error as to the methods of determining the distance of a lunar crater from the limb of the Moon. There are no direct measures made from any one point of the lunar periphery to a spot, and therefore the measurements are not affected by slight inequalities. It is the general circumference of the Moon, as in measurements of solar photographs, that is always dealt with. The general contour of the moon in reference to a spot is determined by the rotation of the micrometer, and the gentlemen who take the trouble of observing it will find that there are means of doing that, and then from the centre determined by the rotation of the moon, the micrometer enables us to determine the distance of any other spot from the centre of the general circumference of the Moon.

I was going to ask Prof. Pritchard whether, in his determinations, he got the libration that ought to exist in the same direction or whether in the opposite direction, and how many of the observations confirm his general result, and how many give negative quantities. Because, if there are a great number of negative quantities, and his result happens to be a happy mean of the difference between positive and negative, of course the result has not the same value as if a far greater number of the results were of a positive character. All I wish to say, as an old photographer of the Moon, is, that the photographs taken at Oxford have remarkably good definition. Naturally I have taken great interest in investigating the possibility of making measurements to a very small fraction of an inch; for example, measurements made, without any bias, by myself, and by another observer, come out one after the other, certainly to within the $\frac{1}{5000}$th of an inch. I might say, in many cases, to the $\frac{1}{10000}$th of an inch; that is to say, the $\frac{1}{10000}$th part of the lunar diameter. All I can say is, I congratulate Prof. Pritchard on the remarkable zeal, industry, and perseverance which he and his two observers and computers, Mr. Plummer and Mr. Jenkins have evinced in this laborious undertaking. (Applause.)

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* [As a matter of fact, Bouvard's observations (reduced by Nicollet) were made in 1808–1810 (a few only in 1806); Nicollet's in 1819–1820 ('Conn. des Tems,' 1822, 1823); and Wichmann's in 1845 (Astr. Nachr. No. 630).—Ed.]

Prof. Pritchard. I have been accustomed to hear exaggerations, but I declare that, in all my experience, I have never seen any thing so exaggerated as that caricature of the Moon's disk now drawn upon the board by Mr. Neison. On the wall you see Mr. Rutherfurd's beautiful photograph of the Moon, and underneath it you see mine. No human hands have touched either the one or the other. Mr. Neison's drawing on the board is the work of a human hand. Now, first of all, Mr. Neison is mistaken if he supposes there exists any known term in the physical libration having for its period the time of revolution of the Moon's perigee. Every lunar inequality plays its part in the rotational inequalities, and the man does not live who can say what the entire period for the periodic return of all these inequalities may be.

Again, the photographs were not taken in 1878; they were taken in 1876. It was in 1878 that Dr. De la Rue's micrometer arrived in Oxford, and I am at a loss to understand the connexion of the period of the revolution of the perigee with either facts, even when combined with Wichmann's measures in 1845*.

As to the average rotundity of the Moon, the photographs themselves, now hanging on the wall, our own repeated measures at Oxford, and Wichmann's measures at Königsberg, place the question, I should say, beyond a doubt. Surely nothing is gained in the quest of truth by exaggerations such as those now exhibited by Mr. Neison on the board; but if he can throw any new light upon a difficult subject, or if he will indicate any really improved method, I shall be most thankful to adopt it. It would be well for us all not so much to search for objections, as to search for and to communicate improvements.

Mr. Neison. As to the 9 years which Prof. Pritchard struck out, I stated that this inequality arises from the inequalities in the Moon, which vary in the same period as the revolution of the Moon's perigee, which is 9 years, unless it has changed within the last few days.

Prof. Pritchard. That is exactly what I deny.

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* [*Note by Prof. Pritchard.*—On calculation it appears that the perspectives of the Moon's disk (from libration) during the taking of the Oxford photographs happened accidentally to be as widely different from those when Wichmann's measures were made, as possibility admits.]

Mr. Neison. If Prof. Pritchard will deny that the apparent libration of the Moon, which depends upon the equation of the centre of the lunar orbit, does depend upon the revolution of the perigee, then I am willing to admit that my methods are insufficient. He seems to think that I am alluding to little tiny hills in the Moon. I am alluding to ranges of mountains which extend along the Moon's limb for several hundreds of miles at a time, and the peaks which have been measured amount to four miles in height. These irregularities are not small things—there are peaks of all heights, from a mile and a half to four miles, extending in ranges for 50° or 60°. These mountains will produce inequalities in the apparent diameter of the Moon amounting to 3″ or 4″ or 5″ of arc. It is those which have this effect, and, I maintain, Prof. Pritchard cannot eliminate them by taking eight or nine or ten measures from different parts of the limb. Wichmann points that out as a weak point of his own method. I suggested this to Prof. Pritchard so that he could introduce into his investigation an inequality of unknown value, and he might be able to determine whether it did or did not amount to any thing, and show that the resultant value that he obtained for the libration of the Moon was thoroughly trustworthy, and obtained without neglecting any precautions whatever.

Prof. Pritchard. I have been now engaged four years upon this subject, and I absolutely deny that the period does depend upon the equation of the centre. The equation of the centre gives us about 40″ as the coefficient of an argument; but the whole effect of this is so small, that Nicollet neglected it. It depends much more upon the annual equation, the effect of which is about eight times as large as that due to the equation to the centre. There is no such period as nine years connected with lunar physical libration that I know of.

Mr. Christie. Prof. Pritchard mentioned that the distances to eight or nine points of the limb have been measured. Mr. Neison talks about these points as if they all lay within a small arc of the Moon—an arc of some 50° or 60°, which I think Mr. Neison considers his mountains may extend over. The question is whether these measurements were made within a limited arc, or whether they were distributed round the circumference. Because, of course, the ordinary law of probability of error would show that if they are distributed around the circumference we shall eliminate the effect.

Prof. Pritchard. They are distributed all round the circumference of the Moon.

Mr. Neison. Not on one photograph.

Prof. Pritchard. Surely the Moon has more limbs than one!

Mr. Christie. I confess I feel a difficulty in understanding how the heights of mountains which extend over 60° can be determined. How are we to fix what the general level is, or what the true limb of the Moon is? Mr. Neison speaks with great confidence of deviations from the circle. How are they to be determined? If we knew that, then they could be applied as corrections. But perhaps they will be eliminated by taking points sufficiently distant from each other. That is a question which we want elucidated.

Mr. Neison. There is no difficulty in determining the heights by measuring the distance of a crater from the limb on different days when the libration is different, the libration being easily calculated. The only difference between me and Prof. Pritchard is due to the fact that he is referring to periods of inequality in the real libration; I am referring to periods of inequality in the apparent libration.

Mr. De la Rue. Reference has been made to Prof. Wichmann's method and to his desire to obtain photographs to measure from. That arose from this circumstance: in 1858 I passed through Königsberg on my way to Russia, and made the acquaintance of Prof. Wichmann; I was then taking photographs of the Moon, one of which I gave him; and he was so struck with their sharpness and the facilities that they would afford for determining the lunar libration, that he was very anxious to obtain a number of them. At that time I contemplated staying in Russia; and he then had recourse to the Astronomical Society to know whether good photographs were obtainable, in order that he might have them; but he very shortly afterwards died.

Prof. Pritchard read a paper on the construction of a new duplex micrometer, adapted to measuring the angle between two objects not simultaneously visible in the same eyepiece. The duplex micrometer was exhibited at the meeting.

Lord Lindsay. Over a large field it would not be perfectly flat, and I think there would be distortion. I want to know how you manage in the field of your telescope to get your eyepiece over stars at equal distances from the edges of the field of view so as to get equal distortions.

Prof. Pritchard. A most fair question. That is a difficulty which has occurred to Lord Lindsay, and would occur to any skilled observer like himself. What we have done is this. Before we applied this instrument to actual research we have examined the field of view in the focal plane of the large refractor and of the De la Rue reflector. We took two pairs of stars, each at about the distance of 20′ from each other. We then placed each of these pairs in various positions in the field of view; and then finding the measured distance of the components of each pair unaltered thereby, I fairly concluded that the field of view to the extent of a diameter of 20′ was sensibly accurate for measurements therein. If it were not so, it would be fatal to all sorts of instruments.

Mr. De la Rue. I think Prof. Pritchard has overlooked the heliometer at the Radcliffe Observatory.

Prof. Pritchard. I beg pardon. I was once for fifteen months in charge of that institution, and I know that heliometer well; and all I can say is that I consider it has never yet been effectively used; but if any man can use it I am quite sure that Mr. Stone is the man who can and will. I fully expect that Mr. Stone will have perseverance and skill to turn that magnificently-intentioned instrument to very good practical effect; but I am thankful that I have not the charge of it. Mr. Stone will do with it quite as much as we could expect from any living astronomer.

Mr. Marth. I should not rise if it were not in obedience to Prof. Pritchard's summons. The first observer who used a ruled glass plate was, I think, Tobias Mayer; and, indeed, his catalogue of the stars in the Pleiades was made with such a glass scale, which he had constructed with his own hands and poor tools. This was before 1750, perhaps in 1748. At that time there lived at Augsburg a clever mechanic (Brander), who took up the subject properly, and constructed Brander's glass micrometers, which in the latter half of the last century were in great repute and much used for different purposes. A description of some of them was published by Lambert about 1770, in which he proposed for large distances the use of two eyepieces; and I think Brander made several micrometers of that kind. I have never seen one; but I remember having seen it stated that at Augsburg, where Brander lived, the museum contains a collection of some of his instruments; so that to any gentleman present who may travel that way I should suggest to look at them and to let us know what they are. Of course these instruments may not be comparable with the present one in beauty of detail; but I mention them merely as the first instances of the principle adopted.

Dr. Wentworth Erck. I had a micrometer of very much the same class of construction made about ten years ago, for the purpose of measuring the positions of Sun-spots. In my telescope the diameter of the solar disk is about an inch. Mine is a great deal simpler than the instrument which Prof. Pritchard has been describing. I had the glass divided by squares instead of by single lines, so that there were ten squares in the radius of the Sun. The whole of this scale was movable by a micrometer-screw; but it had not the spider-lines to move over a fixed plate, as Prof. Pritchard described. Instead of having two eyepieces to move, it had only one eyepiece, which was moved in a groove in the plate, which had an independent motion round about on the axis of the telescope, so that you kept moving the eyepiece rapidly from one to the other.

Prof. Pritchard. The difficulty is to be quite sure that the telescope does not move while you shift your eyepiece.

Dr. Wentworth Erck. With a good driving-clock you can move from one to the other, and you need not take more than two seconds.

Prof. Pritchard read a paper describing a chronograph which had been made for the University Observatory at a cost of £7, and with which the time could be measured as accurately as with one costing a very much larger sum. It was, in fact, an obvious adaptation of Morse's recording-telegraph; and in some form or other he has since heard it had been long ago adopted in some Continental observatories. Mr. Grubb of Dublin constructs it.

Lord Lindsay. As Mr. Marth answered for Germany in the last case, I will answer for Scotland. If Prof. Pritchard will do me the honour of a visit, I will show him an instrument I used in Mauritius. It is a Morse instrument with two levers and two sets of magnets, one for the seconds' taps and the other for the observations. The instrument is perfectly satisfactory; but if the taps are not read off pretty soon afterwards, the needle-punctures will, from changes in the hygrometrical state of the paper, be gradually effaced. My chronograph cost a great deal more than £7.

Mr. Christie. The great desideratum is a cheap instrument. The Morse costs much more than £7.

Prof. Wiedemann was introduced to the Meeting by the President, and at his request *Mr. Schuster* gave the substance of a paper by him on a Method of Determining the Pressure on the Solar Surface. The general principle is this:—If we try to produce interference-bands by means of two different lights, we shall not succeed; and so also if we try to produce interference between a light as it is now and as it will be after a short time, we shall not succeed, because, according to the molecular theory of gases, the molecules of gas suffer a sudden disturbance during that time; so after a short time the same light will behave as two lights. Suppose one single molecule flying about through the gas, as long as it vibrates freely, we shall be able to produce interference; but as soon as it knocks against another molecule, there is a sudden change of phase, which will produce a sudden disturbance in the interference-bands. Wiedemann suggests that we should obtain a ray of light as homogeneous as possible by means of the spectroscope and then produce interference-bands. By counting the number of these bands which we are able to see, the mean time between the two encounters of molecules can be calculated, and by this again the pressure of the gas on the solar surface, in a comet, or even in a nebula can be determined. I think the principle an exceedingly hopeful one; only I would suggest not to count the number of interference-bands, but simply to measure their intensity, because of the enormous differences in the intensity of the light.

Mr. Christie. The question which Prof. Wiedemann endeavours to solve is one of more importance to astronomy than might appear at first sight. A short time ago the President asked me whether any information could be given as to the pressure at the Sun's surface with reference to the retardation or friction that would be produced on a Comet such as that of 1843 or the great southern Comet of the present year. So that it is not merely as a matter of physics, but as a matter of gravitational astronomy, that this question comes in with great importance. I think we ought to be very grateful to Prof. Wiedemann for suggesting a method by which the effect of the solar atmosphere in retarding a comet's motion can be ascertained.

Mr. Glaisher announced the receipt of a paper by *Prof. Adams* on the Constant of Lunar Parallax; by *Mr. Bosanquet* and *Prof.*

Sayce on Babylonian Astronomy; by the *Rev. S. J. Johnson* on the Coincidence of the Sun-spots and Auroræ in olden time; by *Herr Wagner* on the Star No. 894 in the First Greenwich Seven-Year Catalogue; and by *Herr Lohse* on the Refractive and Dispersive Powers of various kinds of Glass.

Mr. Marth, in presenting an addition to the ephemeris for physical observations of Jupiter, said:—I may mention two matters in connexion with my paper. I have received from Mr. Trouvelot, of Cambridge, Massachusetts, some observations on the red spot on Jupiter taken in 1878. He states that during that year the spot had gone through more changes of form than in 1879 or 1880; so that very likely greater irregularities in its motion will be found in that year. At present the question is left in some uncertainty. I hope observations may still be forthcoming not to leave any doubt about the early history of the spot; and at any rate I hope that many observers will take part in future observations, and watch and note the times of the beginning, middle, and end of the red spot crossing the central meridian. The other matter refers to observations connected with the figure of Jupiter itself, for the correct reductions of which the paper supplies the proper data. It would be very desirable that the existing uncertainty about the true dimensions and ellipticity of Jupiter should be reduced by first-rate observations.

Mr. Glaisher announced an ephemeris for the Satellite of Neptune by *Mr. Marth*, also a paper by *Major-Gen. Baillie* on a simple method of finding a Meridian Line.

Mr. Common. This is a description of a method of finding the meridian and getting the sidereal time by observing with the naked eye the reflection of two stars in a basin of mercury by means of cross wires placed above it. The only possible use it might have would be in the case of a scientific traveller landing on a desolate place.

Captain Noble. Years ago I remember a paper being read before the United Service Institution describing such an instrument.

The President. The Fellows will be glad to hear that the whole of Mr. Gill's Ascension work has been received from him. Mr. Christie has received a letter from him, and will read one or two extracts.

Mr. Christie. Mr. Gill has deduced from his heliometer observations of Mars at Ascension twenty-five equations of condition from which he determines the solar parallax; and he finds that, confining himself to the cases of evening and morning observations of the same star, he gets a solar parallax of 8″·782, with a probable error of ±0″·012. If, on the other hand, he takes in all the observations, including those in which different stars were observed morning and evening, and making use of the fundamental places which he has obtained for these stars, he finds for the solar parallax 8″·783, with a probable error of ±0″·026, twice as great as in the former case. Mr. Gill considers that the only systematic error to which his result is liable is that which might arise from the effect of chromatic dispersion of the atmosphere on the limb of Mars—an effect to which his attention was called by Sir G. B. Airy on his return from Ascension. To test this effect, he had discussed the observations at greater and less zenith-distances separately, and had deduced the values 8″·786 and 8″·780 respectively, the agreement being as close as could be desired. The individual results from each day's observations range from 8″·62 to 8″·90; and from a comparison of these with the discordant results obtained by different astronomers from the Transit of Venus, Mr. Gill concludes that a single day's observation of Mars, evening and morning, will give as trustworthy a value of the Solar parallax as a large number of Transit of Venus expeditions. He, however, considers that heliometer observations of minor planets would afford a still more trustworthy result, as not being affected by chromatic dispersion of the atmosphere.

Captain Noble. May I ask whether the observations of the coming transit of Venus are to be only on the method of Delisle?

Mr. Christie. I imagine that they will certainly be reduced by that method.

Captain Noble. Being at the Royal Observatory at Greenwich on Saturday, I saw a most astounding paragraph, that Commander Green of the United States Navy had found that the longitude of Lisbon was 8ˢ wrong. Some years ago, before the last transit of Venus, I called the Astronomer Royal's attention in this room to some observations of Prof. Asaph Hall with reference to the value of Moon culminations. He said that the longitude of San Francisco as determined by 201 Moon culminations was 4ˢ in error; and he gave a variety of other examples, whence he deduced the curious result that from no number of Moon culminations could you possibly get trustworthy longitudes. If we find that the longitude of such a place as Lisbon is 8ˢ wrong, and we are going to determine the Sun's parallax by a method the result of which has been contradicted by Mr. Gill's results, it seems to me that we may just as well not send out an expedition at all.

Mr. Christie. I am not aware that the longitude of Lisbon had been really thoroughly well determined. These determinations by Moon culminations depend upon delicate points in their treatment. Observers usually get a great preponderance of observations of the first limb because it is much more convenient to observe the first limb than to sit up into the early morning to observe the second limb; and if we give equal weight to every observation, any error in the adopted diameter of the Moon will affect the result. With regard to the longitude of San Francisco, there may have been something of that kind; but I suppose we must admit that we have had an error of 3ˢ or 4ˢ in longitude. But that does not at all contradict what I understand the Astronomer Royal stated originally, that the probable error of longitude would be about a second. With a probable error of a second you may in extreme cases get an error three or four times that amount. Probable error does not mean extreme error. I think a second is not a very unfair estimate of the probable error of a longitude determination by Moon culminations. With regard to the result of the last transit, I must say that, having looked into the matter pretty closely, the thing that is forced on one's mind is that the error has arisen not from any uncertainty of longitude, but from the great uncertainty in the observation of contact, in which we are liable to an error of 10^s to 20^s. Capt. Noble has spoken of a discordance between this result of Mr. Gill's and the official result of the Transit of Venus in 1874. There is a great doubt as to what is the official result. There was one result published by the Astronomer Royal in his Parliamentary paper which gave 8″·76 or 8″·75. There was another result published afterwards by Capt. Tupman which gave 8″·86 or 8″·85, differing by $\frac{1}{10}$ of a second. The same longitudes were used throughout: it was simply that a different interpretation was put upon the observations; and the difference in these two results was due to uncertainty in the observation of contact. Perhaps if observers had expressed themselves more clearly, and if the different astronomers who have discussed the observations had fixed on their interpretation before they knew any thing about the result of the observations, we might have got more consistent results; but the uncertainty and the discordance from Mr. Gill's results is not in the least dependent upon the uncertainty of the longitude. If you even allow 4^s in the longitude of any place, that will produce a comparatively small effect upon the parallax. The estimates that were made before the transit by Mr. Proctor and others of the probable error of observation of contact were utterly fallacious. The probable error is three, or four, or five times as great. In one case it is really a question of a minute; and we are not quite certain whether we are justified in altering it or not. And when we have an uncertainty of a minute in the observation of contact, an error of three or four seconds in the longitude is comparatively insignificant*.

The President. The longitude of Lisbon in the 'Nautical Almanack' was adopted in 1872 on the authority of General Foque, who held a similar position to that of the Director of the Ordnance Survey in England. I have no particulars as to how it was obtained. About ten months afterwards I received from the Lisbon Observatory a correction of their longitude amounting to a little over 3^s in the same direction as Commander Green's correction. But it was stated in a letter that the telegraphic connection of the observatory at Lisbon with Madrid would probably be effected. It is not desirable to make frequent alterations in the 'Nautical Almanack' for a port like Lisbon; and I thought it better to keep to the old longitude, which I did until this telegraphic determination was made by the American observers.

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* [For a discussion of the possible effect on the result for the Sun's parallax of errors in the adopted longitudes, *cf.* 'Observatory,' Vol. i. p. 172.—Ed.]

Mr. Chambers. Will you kindly state, Sir, for the information of the Society, what are the terms or conditions which are involved in the issue of the circulars announcing comets and other interesting discoveries? I ask this because it may have some bearing upon the effort made by Lord Lindsay in that direction. I am very glad that the Council should imitate Lord Lindsay's efforts and extend them if they think fit; but I think it would be interesting to know what is to be the policy of the Council with regard to those notices.

The President. It has been suggested that a great many of the Fellows would not care to receive those notices. The idea has been to send them to those Fellows who are provided with telescopes competent to observe comets, and who express a desire to receive them. I do not know that there is any exact limit as to the number that are to be sent out; but it depends upon the number of applications that we receive.

Mr. Chambers. May I ask whether it is a different form of circular from that sent out by Lord Lindsay, or whether the Fellows should make a request both to the Royal Astronomical Society and to Lord Lindsay also?

The President. I apprehend that the Royal Astronomical Society really takes action by itself: it is quite a new thing.

Mr. Christie. The Council have taken it up without wishing to interfere with the plan which Lord Lindsay has adopted of issuing circulars. Dun Echt is so far off that there is a loss of something like three days in the post; and comets are thus lost altogether. Unless you can send out information at once of the discovery of a new comet, it is of very little use sending it out at all. The information generally comes in the first instance to Greenwich, and we can save a post and get them printed at once, so that they may go out on the same day, and thus give a better chance of finding new comets.

Mr. Chambers. It is hardly fair to Lord Lindsay and hardly creditable to this Society merely to supplement his Lordship's efforts. I think the Society ought to do the thing sufficiently well to supersede the efforts of a private Fellow. I hope there will be a combined arrangement between the Society and Lord Lindsay, so that those who are working Members of the Society may at first hand receive the fullest information without Lord Lindsay's liberal resources being unreasonably taxed to do what ought many years ago to have been effectually done by the Society for the benefit of Astronomy.

The following gentlemen were balloted for and duly elected Fellows of the Society:—Frederick Hall, G. Kilgaur, Lieut. D. F. M'Carthy, R.E., F. H. S. Orpen, Lieut. Sidney Smith, R.N., Capt. R. W. Sterry, and Capt. Peter Thompson.

The Meeting adjourned shortly after ten o'clock.

Measures of the Polar and Equatoreal Diameters of Mars, made at Princeton, New Jersey, U.S.[*]

THE polar compression of Mars has never yet been satisfactorily determined, the results of different observers ranging from that of Sir W. Herschel, who made it $\frac{1}{16}$, to that of Bessel, who found it insensible. The value $\frac{1}{62}$, deduced by Main at Oxford, from his measures in 1862–3, has probably been of late more generally accepted than any other, though by no means without reserve.

Hartwig, in his recently published investigation upon the diameters of Venus and Mars, gives $\frac{1}{96}$ as the result obtained by combining all the double-image measurements made at Königsberg, Leiden, Oxford, Berlin, Paris, and Strassburg.

Either of these values, however, is apparently irreconcilable with the planet's known mass and period of rotation, if we admit the presence of water upon its surface, as the polar "snow caps" seem to indicate, except upon the almost absurd assumption of a density rapidly increasing from the centre toward the surface.

It has seemed to the writer quite possible that the difference of illumination of the limbs of the planet, caused by phase, may lie at the bottom of the difficulty. Except on rare occasions, there is phase enough, even at the moment of oppposition, to produce a notable difference of appearance between the fully illuminated edge of the planet's disc and that opposite, a difference which can hardly fail to be felt in micrometric measurements. Unexceptionable observations for determining the polar compression can therefore be made only when the planet reaches opposition and its node together. This was so nearly the case last season, that on the night of November 12, an observer on the planet would have witnessed a Transit of the Earth. At this time, and for a few days before and after, the phase was extremely small, and an opportunity was presented for determining the planet's ellipticity such as will not be available again for nearly half a century.

The measures were made by myself with a filar position-micrometer attached to the nine and one-half inch Equatoreal of the School of Science Observatory. The object-glass of this instrument (by Clark) is constructed substantially upon the Gaussian curves, and is of the highest excellence. During the past year it has shown repeatedly both of the satellites of Mars, the two outer satellites of Uranus, and the Saturnian satellite Mimas, the last, for my eye, which is not extremely sensitive, being just at the limit of visibility. The telescope doubles distinctly, and on fine nights easily, the little companion of α^2 Capricorni, nor has it yet failed upon any of the objects usually considered tests for twelve-inch instruments.

While perfectly aware that measures by a filar micrometer are subject to a considerable constant error, the measure of an illuminated disc being always excessive, I have thought they might safely be used in determining a difference of diameter in different directions. The wires were dark upon an illuminated field. The magnifying-power throughout the observations was 398. The value of one revolution of the micrometer-screw at 50 °F., as determined by about a hundred star transits, is 17″·860±0″·003. The screw appears to be a very perfect one without sensible errors of periodicity or run.

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[*] The substance of a paper in the 'American Journal of Science,' 1880 March. Communicated by the Author.

Marth's ephemeris, in the 'Monthly Notices' for June 1879, was used in setting the position-circle, and in computing the minute phase-corrections. Each measure of a double diameter consisted of twenty settings of the micrometer wire, five with the movable wire on one side of the fixed wire, ten with it upon the other side, and finally five more in the first position. The correction for thickness of the wires was determined by a full set of readings at each of the three different fixed wires, one at least in each evening's work.

After each measurement of either diameter, the wires were turned 90° by the position-circle, and the other diameter was measured. In some cases, however, after finishing a set of four measures (two of each diameter) at one of the fixed wires, the movable wire was transferred to another of the fixed wires, and the same diameter was then measured again as the first of a new set. The measures thus come in pairs, each measure of the polar diameter being matched with an equatoreal immediately preceding or following. The total number of micrometer-readings, exclusive of those for determining the thickness of wires, was 1140.

Had not cloudy weather prevented, the series would have been much more extensive. The nights of November 11th and 13th were entirely overcast; on November 12th only a single pair of measures of small weight could be obtained, and on the 14th only two pairs and an odd one. On all the other nights there were also occasional periods of atmospheric disturbance or entire interruptions. It is especially to be regretted that more measures were not obtained at considerable western hour-angles, as they would have gone far to eliminate the effects of atmospheric dispersion and astigmatism of the observer's eye. The observations have been very carefully reduced under my direction, by my assistant, Mr. M. McNeil, who deserves and has my warmest thanks for his interest and help in the matter.

If we apply the corrections for refraction, phase, and reduction to opposition, and group the results according to the angle made by the observed diameter with the vertical, it at once becomes evident that a further correction is needed, horizontal lines being systematically measured smaller than the same when vertical. Thus, if we take the twenty measures of the equatorial diameter made when its inclination to the vertical was more than 45°, we get for the mean (by weights) 20″·687±0″·028 at a mean angle of

$72^\circ\cdot4$. The nine nearly vertical measures give $20''\cdot780\pm0''\cdot044$ at a mean angle of $19^\circ\cdot3$. Similarly the ten nearly horizontal measures of the Polar diameter give $20''\cdot608\pm0''\cdot031$ at a mean angle of $73^\circ\cdot8$, while the nineteen nearly vertical measures give $20''\cdot706\pm0''\cdot026$, at a mean angle of $16^\circ\cdot9$. No sensible difference appears between results obtained when the measured diameter inclines to the right, and those obtained when the inclination is toward the left.

It is not certain what causes produce this difference between horizontal and vertical measures; but it is probably due in part to atmospheric dispersion, and partly to a known vertical astigmatism of the observer's eye. In either case the effect would be proportional to the cosine of the inclination, and we may therefore assume with sufficient approximation that the correction required to an observation is $I=\alpha\times\cos\beta$; β being the angle with the vertical, and α a constant to be determined from the observations themselves.

Approximate values of α can be obtained from the data given above; thus, from the two groups of equatorial measures (regarding each group as a single measure) we get $\alpha=0''\cdot149$. The two polar groups give $\alpha=0''\cdot146$; a coincidence which strongly attests the reality of the correction.

It is not, however, strictly correct to treat these groups as single observations. The more legitimate course is to regard each observation (corrected for refraction and phase and reduced to opposition) as furnishing an equation of condition of the form

$$\textit{measured diameter}=\textit{true diameter}+\alpha\cos\beta.$$

This has been done, and from the equations of condition, using the weights given, normals have been formed, in several different ways.

Thus we may take as the unknown quantities π, ϵ (the polar and equatorial diameters) and α. Or calling $c=\epsilon-\pi$, we may take as the unknowns , π and c, or α, ϵ, and c. Or finally, putting $m=\frac{\epsilon+\pi}{2}$, we may form our normals with m, c, and α. Whichever of the four sets of normals we use, we get for the unknown quantities involved the following values, weights, and probable errors, viz.

ϵ (equatoreal diam., Nov. 12, 1879) $=20''\cdot634\ \pm0''\cdot034$; wt. 8·92
π (polar diam., „ „ „) $=20\ \cdot552\ +0\ \cdot043$; wt. 5·85
$m=\frac{\epsilon+\pi}{2}$ $=20\ \cdot593\ \pm0\ \cdot035$; wt. 8·85
$c=(\epsilon-\pi)$ $=0\ \cdot0818\pm0\ \cdot0348$; wt. 8·75
α coefficient of inclination correction $=0\ \cdot168\ \pm0\ \cdot050$; wt. 4·17

The introduction of the correction I reduces the sum of squares of residuals multiplied by weights from 1·3994 to 1·2612.

The values of the diameters are of course not very reliable, being subject to the considerable constant error which always affects filar micrometer measures. If we accept Hartwig's determination, 9″·352*, for the diameter at distance unity, a value which depends upon the whole body of heliometer and double-image micrometer measures up to 1877, we get for the opposition diameter of 1879, 19″·128. Comparing this with my mean, we find 1″·46 as the constant error of my filar micrometer measures, a value rather unexpectedly large, but not unprecedented.

This constant error can, however, have but very slight, if any, influence upon the measured difference of diameters, and we find for the ellipticity of the visible periphery of the planet the value $\frac{0''\cdot0818}{19\cdot128}=\frac{1}{234}$.

As the pole was not on the periphery, but, according to Marth's ephemeris, at a distance of 14°·5 from it, a slight correction is still needed to deduce the true polar compression. The diameter measured as polar was that joining two antipodal points in areographic latitude 75°·5. Recalling the formula for the radius of a spheroid at any latitude, viz.

$$\rho=a\sqrt{\frac{1-2\,e^2\sin^2\phi+e^4\sin^2\phi}{1-e^2\sin^2\phi}},$$

and putting $\frac{\rho}{a}=\frac{233}{234}$, we easily find e, the eccentricity of a planetary meridian, and from it C_0, the polar compression. Performing the solution we get $C_0=\frac{1}{219}$, which may be taken as the final result of the work here detailed.

The limits of probable error extend from $\frac{1}{156}$ to $\frac{1}{364}$, and, so far as can be judged from the observations themselves, the chances are more than two to one that the ellipticity lies between $\frac{1}{139}$ and $\frac{1}{515}$.

The work of reduction was nearly finished before the writer had seen Professor Adams's recent paper upon the orbits of the satellites of Mars, in which he gives $\frac{1}{228}$ as the ellipticity which the planet ought to have if it follows the same law of central density as the Earth. The closeness of the accordance is probably in considerable degree accidental, but it is quite satisfactory.

C. A. YOUNG.

Princeton, New Jersey, Jan. 1880.

Boss's Declinations of Fixed Stars.

PRACTICAL astronomers have for some time been looking forward with interest to the appearance of Mr. Boss's work, rumours of the progress and approach to completion of which have from time to time reached this country. The interest has been intensified from the circumstance that the declinations of the stars given in the American 'Nautical Almanac' for 1881 and following years are taken from Mr. Boss's paper, testifying thereby to the great reputation which the work has already obtained in the United States, Various delays appear to have occurred in the publication of the book, so that it was not until early in March of the present year that the first copies reached England. As is perhaps natural under these circumstances, the first feeling experienced on looking over the paper is one of disappointment; there is a want of uniformity of procedure and of methods of treatment which gives rise to the suspicion that preconceived theories as to the relative accuracy of catalogues are allowed to overweigh the results of actual examination of the catalogues themselves. This more or less patch-work character is to a great extent explained by the history of the work given in the introduction, from which it appears that the star-places were originally intended merely for the determination of latitudes required for the use of the United States Northern Boundary Commission, and that the plan was gradually enlarged as an afterthought by Mr. Boss until at last the work reached its present dimensions. On looking closer into the paper, however, one is struck with the vast amount of industry and of research exhibited almost in every page, and the conclusion is very soon arrived at, that it is by far the most complete and exhaustive work of the kind yet published. The work contains systematic corrections to the declinations of almost every existing catalogue, as well as a catalogue of the declinations of 500 stars for 1875·0, deduced from a thorough discussion of the star-places given in these catalogues. The former is of course by far the most interesting, and perhaps the most important, part of the work; but in addition to these there are some very interesting discussions to be met with in the earlier part of the paper, which is devoted to the examination and comparison of the various catalogues.

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* The mean diameter 20″·593 resulting from my observations, when reduced to distance unity, gives 10″·068; a value sensibly identical with that obtained by Peirce from a discussion of the Washington Mural-circle observations, and used in the American Ephemeris.

The plan of proceeding adopted by Mr. Boss is briefly as follows:—He first forms a list of stars which have been most frequently observed, to serve as a basis for the normal system and as a medium of comparison between the various catalogues. Then comes the formation of approximate positions and proper motions for 1875·0, and the computation of precessions which are necessary for the reduction of the individual declinations to the required epochs, in order that the assumed declinations may be corrected by comparison with the observed values. The declinations of the various catalogues used are first of all modified by the application of certain corrections deemed advisable from an inspection of the constants and methods of reduction. The approximate normal system is then formed from those series of observations which are supposed to give determinations of sufficient independence and weight. By the aid of the approximate corrections to the selected list of authorities, Mr. Boss proceeds, by successive steps, to ascertain corrections to the assumed declination and proper motion of each star, and then to compute definitive corrections and weights for the numerous catalogues, with which final weights and corrections the definitive declinations are computed as they appear in the catalogue.

This series of declinations is unique in one respect, it does not depend to any considerable extent on Bradley's observations as given in the 'Fundamenta,'—Mr. Boss contending that a reliable system of corrections to the various catalogues may be founded on a discussion of recent catalogues alone, beginning with Bessel, 1821.

Mr. Boss's nomenclature for abbreviation of the titles of star-catalogues is peculiar. He uses the first and last letters of the name of the place where the observations were made, followed by the mean year of observation; thus "Gh. 57" means the Greenwich Seven-year Catalogue for 1860. This notation is no doubt convenient to a certain extent, but I confess that I was unable at first to recognize Groombridge's catalogue in "Bh. 10."

It is of course impossible on the present occasion to advert to all the points of interest raised in the examination of the various catalogues; it must suffice to refer briefly to one or two of them. And first with regard to the Greenwich catalogues, 1836–1872 (Mr. Boss having formed a catalogue for 1870 from the observations 1868–72).

It is pointed out that the observations are made with non-reversible instruments, and that the problem of absolute declination is rendered subordinate to the requirements of routine work on a large number of objects. The position of the telescope on each of the mural circles formerly in use was changed at the beginning of each year, the relation of the telescope and circle of the transit-circle is invariable. Mr. Boss considers that "Gh. 57" contains the work of the transit-circle in its best state, before any appreciable wear had resulted from long use, that the instrument gradually underwent slow changes from wear, which are quite sufficient to account for a real difference in the systematic corrections required for this catalogue and for that for the epoch 1864. The author became aware of the large error due to wear of the micrometer-screws during the period 1868–72, too late to make any use of a special correction on that account. Acting on a suggestion of Prof. Newcomb's, an attempt has been made to reconcile the entire series of observations with the transit-circle on the supposition of a uniform coefficient of flexure depending on sin Z. From a discussion of the different catalogues, Mr. Boss finds that if a uniform value of sine flexure $+0''\cdot21$ had been employed throughout the series, we should have had sufficiently good agreement between direct and reflexion observations, and considers that this result throws discredit on the value of flexure derived from opposing collimators, and makes it probable that the change in collimator-flexure between

1865 and 1866 (the time when the cube of the telescope was pierced) was much smaller than has been supposed. A correction derived from the empirical formula a ($\sin^3Z+\sin^3Z'$), where Z' is the zenith-distance of the pole, has been applied by Mr. Boss to "Gh. 70," and the remark is appended that it is probable that a great part of the correction is due to error in the adopted constant of refraction. It appears to me that it would be necessary to correct the observations 1868–1872 for error in the adopted refractions (if such exists) before any corrections depending on flexure or R – D discordance can legitimately be applied by means of such a formula as that given above.

Mr. Boss has devoted much space to an exhaustive discussion of the observations made with the Washington transit-circle. I can only here refer to his results with regard to the adopted refractions. From a discussion of the results of observations of circumpolar stars 1866–1869 it is found that

$$\{\text{Bessel's refractions}\}\times\cdot 99986=\text{Washington};$$

and from a comparison with the polar distances of the Melbourne Catalogue for 1870 (the latter being reduced to those which would have resulted from the use of refractions derived from circumpolar stars alone) there results

$$\{\text{Bessel's refractions}\}\times\cdot 99532=\text{Washington}$$
$$\text{and}\quad\{\text{Bessel's refractions}\}\times\cdot 99267=\text{Melbourne}.$$

In 'Monthly Notices' for 1879, June, will be found a comparison between the polar distances of the Washington Catalogue for 1860 and those of the Cape Catalogue for the same epoch, from which it appears that the Washington observations would be fairly represented by the use of

$$\{\text{Bessel's refractions}\}\times\cdot 99672.$$

It will be remarked that these investigations all indicate the necessity of a diminution of the refractions of the 'Tabulæ Regiomontanæ;' it is, however, doubtful whether the assumption that the refractions at any place can be represented by Bessel's multiplied by a constant is a sound one, as its adoption has generally led to unsatisfactory results, and astronomers are gradually coming to the conclusion that a thorough revision of Bessel's theory, or at all events a redetermination of the various constants employed in the formation of the refraction tables, is necessary. I have often wondered why the Washington astronomers were willing to take their refractions from an "alien authority," and I venture to think that the example of Pulkowa is one which they might advantageously follow in this matter.

In his first approximation to a normal system Mr. Boss has given the greatest weight (10) to "Pa. 45" (Pulkowa) and the next largest (8) to "Ln. 67" (Leiden). On turning to the preliminary corrections to the fundamental catalogues I was surprised to find that the corrections to these two catalogues are larger than those to "Gh. 45," which is credited with weight 2, and to "Gh. 64," which has the same weight, respectively; and the same thing appears in the final table of systematic corrections. I am unable to account for this apparent inconsistency, but have no doubt that there is some good reason for it; at all events it is not likely to affect the definitive declinations injuriously.

I am glad to find that Mr. Boss makes frequent use of hand-drawn curves in his determinations of systematic errors; it is, I am convinced, a far more satisfactory method than the assumption of a formula, unless under very exceptional circumstances. This latter process is, however, too often resorted to throughout the work, the author's practice being to adopt a general expression, such as a (sin Z + sin Z'), or a (tan Z + tan Z'), whenever it was found to represent, approximately, the residuals. This appears to me to be a fault in the work, which is generally entitled to the highest praise, though of course, where so much depends on the individual judgment, differences of opinion would constantly arise on matters of detail.

It must be explained that Mr. Boss's Catalogue extends down to the south pole, all the available southern catalogues being used for the places of stars situated between −30° and −90° Dec. These of course cannot be compared in accuracy with the places of the northern stars, as with our present means of information very little can be known of the systematic corrections required; but they will serve as a basis for future discussions when more southern observations are available.

With regard to the accuracy claimed for the declinations of the northern stars, it is sufficient to say that for 1875 the probable error of position of stars near the equator is estimated to be $\pm''{\cdot}15$, and less than this for any given point between the equator and the north pole. But, using the equatoreal systematic corrections of the 32 catalogues upon which the system is based, it appears that it best corresponds to the epoch 1847·5, its probable error is then $\pm''{\cdot}05$; the author, however, modestly adds that these probable errors may be somewhat too small, since more than half the determinations are reduced with the same refractions.

We naturally turn for comparison of the places of the 500 stars to the latest publication of a similar character, viz. Auwers' 'Fundamental-Catalog für die Zonen-Beobachtungen;' the result is very satisfactory and testifies to the accuracy with which declinations of northern stars may now be found. Column I. of the following Table is derived from a comparison of the Pulkowa *Haupsterne* common to the catalogues. Column II. gives the comparison with Auwers' Mean System (A.N. 1536).

Limits of Dec.		Auwers—Boss. I.	Auwers—Boss. II.
		"	"
+90° to	+80°	+0·07	−0·16
80	70	+0·12	−0·35
70	60	+0·05	−0·35
60	50	+0·07	−0·25

Limits of Dec.		Auwers—Boss. I.	Auwers—Boss. II.
		″	″
+50° to	+40°	−0·03	−0·01
40	30	−0·04	+0·17
30	20	−0·01	+0·10
20	10	−0·20	+0·09
+10	+ 0	−0·15	+0·02
− 0	−10	−0·33	0·00
−10	−20		+0·06
−20	−30		+0·16

In conclusion, I would say that Mr. Boss is to be congratulated on the successful termination of his labours, which have resulted in the production of a work the most complete of its kind yet published, a work which is mainly the result of the application of the industry and energy which is so characteristic of his nation.

A. M. W. Downing.

Double Stars for July.

The principal pairs visible in this month will be found in the 'Observatory,' Vol. iii. p. 86, &c.

Σ 1746. R.A. 13$^{\text{h}}$ 22$^{\text{m}}$·2, Dec. +10° 5′. Mag. 7·7, 10·3.

1868·06	249°·9	27″·98	De.

Angle unchanged; distance has decreased about 2″ since 1829.

Σ 1768. R.A. 13$^{\text{h}}$ 32$^{\text{m}}$·1, Dec. +36° 54′. Mag. 5·7, 7·6.

This is 25 Canum Ven. Binary. Distance less than 0″·5. Period about 124 years.

Σ 1771. R.A. 13$^{\text{h}}$ 33$^{\text{m}}$·5, Dec. +70° 24′. Mag. 7·8, 8·5.

1867·19	73°·4	1″·76	De.
1878·29	75 ·8	1 ·97	Bu.

In 1829 Σ found the angle 69°·9, distance 1″·81. Probably binary.

OΣ 270. R.A. 13$^{\text{h}}$ 41$^{\text{m}}$·6, Dec. +18° 3′. Mag. 4·8, 11·7.

1878·3	351°·9	8″·71	Bu.

A physical pair. The proper motion of the principal star is −0″·49 in R.A. and −0″·05 in P.D. according to Flammarion. The star is τ Boötis. Since 1849 the angle has increased 4° and the distance diminished 1″·5.

OΣ 273. R.A. 13$^{\text{h}}$ 50$^{\text{m}}$·3, Dec. +5° 50′. Mag. 7·5, 8.

1868·59	110°·6	0″·94	De.

The angle has increased about 8° and the distance about 0″·2 since 1843. Binary.

OΣ 276. R.A. $14^{h}\ 3^{m}{\cdot}1$, Dec. +37° 19′. Mag. 7·5, 8·3, 10.

AB	1869·37	194°·3	elongated.	De.
AC	1869·37	74 ·2	9″·57	De.

AB probably binary. In 1843 OΣ has 193°·6, 0″·48.

Σ 3124. R.A. $14^{h}\ 11^{m}{\cdot}9$, Dec. +51° 55′. Mag. 4½, 4½, 8.

AB	1836	elongated in 150°.		Σ.
AC		33° ±	38″ ±	

This is ι Boötis. AB have never been separated (see 'Observatory,' Vol. iii. p. 381). A physical system.

1846. R.A. $14^{h}\ 22^{m}{\cdot}0$, Dec. −1° 41′. Mag. 5·2, 9·7.

This is φ Virginis. Probably a physical pair. Webb could not see the companion in 1854.

1878·73	108°·5	4″·15	Cincinnati Obs.

OΣ 297. R.A. $15^{h}\ 29^{m}{\cdot}8$, Dec. +25° 25′. Mag. 7·5, 11·5.

1867·0	147°·7	10″·23	De.

Angle unchanged; distance decreased about 3″ since 1845.

The following pairs discovered by Mr. Burnham (β) will probably require fine sky, considerable aperture, and some patience:—

β 31. R.A. $14^{h}\ 46^{m}{\cdot}9$, Dec. +19° 13′. Mag. 8, 10·2, 12–13.

AB	187°·8	1″·29	1878·24	β.
AC	161 ·4	9 ·04	·24	β.

β 239. R.A. $14^{h}\ 51^{m}{\cdot}5$, Dec. −27° 10′. Mag. 6, 6.

129°·5	0″·93	1878·36	β.

β 348. R.A. $14^{h}\ 55^{m}{\cdot}8$, Dec. +0° 20′. Mag. 6, 7·5.

121°·4	0″·51	1878·36	β.

"Very difficult." The angle has probably increased since 1875.

β 120. R.A. $16^{h}\ 5^{m}{\cdot}0$, Dec. −19° 9′. Mag. 4·2, 6·7.

AB	3°·7	1″·04	1878·3	β.

In 1873 the distance was estimated 0″·4 by β. The wide pair AB and C is H_1 V. 106.

AB and C	336°·9	40″·78	1874·5	De.
CD	48 ·4	1 ·89	·5	De.

A very fine system.

JOSEPH GLEDHILL.

Mr. Edward Crossley's Observatory,
Bermerside, Halifax.

*Meteor Notes for July**.

The strong twilight which prevails during the brief nights of this month offers an impediment to successful observation, but the epoch is one of note and requires further investigation. During the last few nights of July two rich streams may be observed (with radiants near δ Aquarii and χ Persei), and the horary number of meteors considerably exceeds that of ordinary nights of the year. Fireballs are very numerous, especially on the 28th; they appear to be chiefly directed from showers in Aquarius. The 11th, 20th, and 21st are also dates on which a large proportion of bright meteors have been recorded, and they should be looked for each year with a view to ascertain the radiant-points. It is impossible to specify the almost numberless showers which mark this season of the year; we have, however, in former papers pointed out a few of the more important.

Recent Observations.—Mr. V. Cornish writes that on March 29, $7^h\ 50^m$, a large meteor was seen at Debenham. The sky was rather cloudy, but the meteor looked fully as bright as Venus at maximum. The beginning of its path was in the W. by S., at an altitude of 35°, whence it descended to an altitude of 15° in due S. It travelled very swiftly, leaving a track along the whole length of its path, and disappeared suddenly without any explosion or emission of sparks. Another fine meteor = Sirius was seen at the same place on April 4, $9^h\ 30^m$, with a path from $114\frac{1}{2}^\circ + 8^\circ$ to $119^\circ + 2^\circ$; it left a short train. Mr. Cornish adds that "meteors seemed pretty frequent that evening," thus fully confirming other observations mentioned in the 'Observatory,' No. 37, p. 414.

A large meteor was seen, at $11^h\ 10^m$ on May 27, by the writer at Bristol. The observed portion of its path was from $327^\circ + 30^\circ$ to $357^\circ + 34^\circ$, which it traversed very slowly. The same meteor is referred to by several correspondents of the 'English Mechanic.' It was seen from a place between Leeds and York, where it was estimated as "considerably brighter than Venus. It was almost due east, and, roughly speaking, commenced its visible path about 30° above the horizon, disappearing 20° to 25° above, it having declined about 5° or 10° in its course." Another writer, who omits to give his station, describes the meteor as moving nearly parallel with the horizon, and remaining in sight not less than 4 or 5 seconds:—"Its clear brightness, large size, and slow motion made it a singularly beautiful object." The radiant of the meteor was evidently low in the southern sky and probably near ζ Ophiuchi (or at $247^\circ - 10^\circ$), for another bright shooting-star, >1st mag., was recorded at Bristol on June 8, at $11^h\ 20^m$, travelling from $228^\circ + 10^\circ$ to $223^\circ + 15^\circ$, and conforming exactly with the position alluded to. These meteors may have had their origin in the same stream as that of the large fireball of June 7, 1878, which, however, diverged from a point further S. near Antares.

W. F. Denning.

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* See 'Observatory,' Vol. i. p. 94, Vol. ii. p. 103, and Vol. iii. pp. 88, 89, for further notes on July showers.

CORRESPONDENCE.

To the Editor of 'The Observatory.'

White Spots on Venus.

Sir,—

I read with much pleasure, in the 'Observatory,' No. 37, page 416, that Mr. Trouvelot has made important observations on Venus, and that he contemplates publishing soon a memoir on that planet. The work on which I am myself engaged is too extensive to be finished so soon and will probably not be published before that of Mr. Trouvelot. I shall be, therefore, very happy to see the publication of the Cambridge astronomer completed with the least possible delay, so as to be able to study also the results obtained by this skilful and persevering observer.

The short and interesting letter which he has communicated to the 'Observatory' induces me to insist already on the interest which is offered by these white spots observed near the horns and recalling the polar spots of Mars. Gruithuisen, in 1813, made a great stir, and with reason, about the discovery of these same spots, and the study of them is worthy of the greatest attention.

I am Sir, yours faithfully,

Louvain, Belgium, F. Terby.

The Markings on Mars.

Sir,—

Allow me to thank Mr. Knobel for having given, in the 'Observatory,' No. 38, p. 453, some explanations on the subject of the markings on Mars, about which he had spoken at the meeting of the Royal Astronomical Society. Mr. Knobel does a real service to Areography in recalling attention to this region, still so little known, at the approach of oppositions in which it can be best studied.

It is a question, then, as to the light bands which extend from the Kaiser Sea to the Tycho Sea (Knobel Sea of Green) and which are shown, for example, in the drawing No. 12 of my 'Aréographie,' due to Mädler in 1839, and in figs. 11, 12, and 13 of Mr. Knobel ('Monthly Notices,' 1873); and, I would add, in fig. 31 of the 'Aréographie,' due also to Mr. Knobel. The bands, then, *precede* the Tycho Sea (Proctor). It is necessary to be very precise here, because Mr. Knobel cites a passage of my memoir which refers to another region *following* the Tycho Sea. The isthmus of which I speak in this passage is the bright line which in the drawings Nos. 5, 6, 7, 8, and 9 of Mr. Knobel (*loc. cit.*) separates the Tycho Sea from the prolongation which *follows* it, or separates Knobel Sea from Tycho Sea (Green). It is this isthmus which Capt. Jacob saw in 1854.

Since the publication of the 'Aréographie' our knowledge on this point has not changed; this region was not sufficiently visible in

1875, in 1877, and in 1879. Mr. Green has not been able in tracing his chart to base it on *new* observations on this point, and M. Schiaparelli has only been able to draw there the Nile, which is nothing but a simple vestige of the great oceans situated in those regions.

Meanwhile we cannot give any other explanation of those bands than that which is found in pp. 99–104 of the 'Aréographie;' and the two questions concerning them at p. 104 are still submitted in the same terms to the attention of observers of the next oppositions:—

(*a*) What are the relations of Tycho Sea to Beer Sea and Nasmyth inlet?

(*b*) To study the exact configuration of the two dusky bands which join Tycho Sea and Delambre Sea with the more westerly regions, and to examine *if they are not in reality prolongations of these two Seas only confused on the East.*

The following is a complete list of drawings in which I have remarked the presence of the two bands in question. The observations marked with an asterisk have not been mentioned in the 'Aréographie':—

Kunowsky in 1822.
Beer & Mädler, figs. 1 and 2 of 1837.
Mädler, figs. 2, 3, 8, 9 of 1839.
*Galle, 1839, Public. des Astrophys. Obs. zu Potsdam, vol. i. pt. 2, pp. 129, 130; figs. 12, 14, and 15.
Mädler, 1841, figs. 5, 6, 17, 19, 20, 22.
Jacob, 1854.
Secchi, 1858, figs. 7, 8, 9, 10.
*Wilson, 1871 (unpublished drawings).
J. Schmidt, 1873, fig. 17 of the 'Aréographie.'
Green, 1873, 'Aréographie,' fig. 48.
*Trouvelot, 1873, May 24, 26, and 29 (Annals of Harvard Coll. Obs. vol. viii. part 2, plate 22). Very remarkable drawings from this point of view, and of which a detailed study has appeared in the 'Bulletins de l'Acad. Royale de Belgique,' 2^{me} Série, Tome xl. No. 11, chap. v. "Mers de Tycho et Delambre," p. 569.

I take this opportunity, which shows so well the interest of old drawings, to earnestly solicit of the Royal Astronomical Society the publication of the drawings of Hofrath Schwabe of Dessau, drawings included in the register of observations preserved by the Society (*vide* 'Monthly Notices,' 1876, May)†. These drawings were made in 1830 and 1837, and it is unnecessary to point out the interest which would be taken in the comparison of them with those of Beer and Mädler. This precious manuscript of Schwabe contains also drawings of Jupiter and Venus which may perhaps be of much interest. The views of Jupiter would perhaps serve to elucidate in certain points the important question of the "red spot," so much discussed at the present time amongst astronomers.

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† [Up to the present time Schwabe's MSS. have not been in the actual possession of the Royal Astronomical Society, having remained at Kew Observatory for examination and discussion of the Sun-spot observations from the time when they were brought over to this country.—Ed.]

I am, Sir, yours faithfully,
F. Terby.

Louvain, Belgium,
1880, June 9.

The Nebula in the Pleiades.

Sir,—

It may be interesting to state, in connection with the Nebula in the Pleiades, that M. Tempel, Director of the Florence Observatory, has sent a charming little map or drawing of the group and Nebula to the Royal Astronomical Society; it is done by hand, and is made from observations with a refractor of, I think, about 11½ inches aperture. I believe there is some idea of the Society publishing it, and it is to be hoped it will be decided to do so. The drawing shows the Nebula much as Mr. Hall has drawn it; there is an entire absence of nebulous matter in or about the group, except in a roughly S.W. direction from Merope; the edges running almost directly S. and W. of Merope are well defined, and the Nebula fades gradually away to the S.W. Just S. of Merope there appear two or three little conglomerations, where M. Tempel apparently resolves the Nebula into minute stars. As to the faint stars in the group, M. Tempel has mapped the little one close to the triangle near Alcyone, mentioned by Mr. Knott in the 'Observatory' for June; also a rather brighter star closely *n.* of a pair which lies about 1^{m} *p.* and a little *n.* of the triangle; this star I saw last winter, but it is not in M. Wolf's map, nor among those in his list of 36 added stars, published in the same Number of the 'Observatory.' I have not yet had an opportunity of comparing the two maps completely.

I remain, yours faithfully,
James L. McCance.

Rathfern, Rayners Road,
Putney Hill, 1880, June 10.

P.S.—Since writing the above I have read a translation of the extract from the letter of M. Tempel accompanying his drawing. He considers the nebulosity surrounding the group, which some observers have thought they saw, an optical illusion; and the two "nuclei or nodules" south of Merope are but little more luminous than the rest of the nebula. M. Tempel has not inserted all the minute stars he sees, and the omitted ones may amount, he says, to "some hundreds."

J. L. McCance.

June 12.

Observing Weather at Scilly.

Sir,—

It was mentioned in the Astronomer Royal's paper on the Transit of Venus of 1882, that the Sun would be too low for good observations in Great Britain, except about the Land's End and Valentia, where the elevation is about 14°, and the factor of parallax 0·65.

As the transit would be seen longest at these places in England and Ireland, I endeavoured to obtain some meteorological information, two or three years back, about Valentia and the Scilly Islands at the beginning of December. Concerning Valentia, I could ascertain nothing; but the following was furnished me with regard to the other locality. The return relates to December 6th, at 2^h afternoon, each year (the amount of cloud is given in brackets):—1871 (3); 1872 (4); 1873 (3); 1874 (4); 1875 (4); 1876 (3). The figure 3 would indicate sky three-fourths covered; and 4 signifies overcast. It is well, therefore, that observers should think twice before running the risk of cloudy weather in the most favourable parts of our own country. From my own observations in another locality in England, only on one occasion (1879), for many years back, would external and internal contact at ingress have been obtainable.

Abbenhall Rectory,
Gloucester, 1880, June 3.

Faithfully yours,
Samuel J. Johnson.

NOTES.

Royal Observatory, Greenwich.—The Astronomer Royal presented his Report for the period 1879, May 20, to 1880, May 9, at the Annual Visitation of the Royal Observatory on June 5.

Under the head of buildings and grounds, it is stated that the Admiralty have decided not to proceed with the erection of a new Library in the present year, though the ground has been cleared in preparation.

As regards instruments, the horizontal circle of the altazimuth has been regraduated and an azimuth-mark at a distance of 1700 feet has been set up. The Astronomer Royal has had constructed by Mr. Hilger a double-image micrometer with a sphere of Iceland spar, but has not yet had an opportunity of examining its scale.

The record of observations shows 4164 transits and 3953 zenith-distances of Sun, Moon, Planets, and Stars with the transit-circle. The Moon has been observed 99 times with the transit-circle and 154 times with the altazimuth, being an average of 8·3 and 14·0 per lunation with the two instruments respectively. Occultations of stars and phenomena of Jupiter's satellites have been observed at every available opportunity, and a few drawings of Mars and Jupiter and measures of Deimos and the satellites of Saturn have been made. It is worthy of note, as evidence of the bad weather of last summer, that for a whole month in July no observation of the Sun could be obtained. The reductions are kept up as closely as usual, and the results of the collected observations of last year have been completely deduced.

The Sun's chromosphere has been examined on 37 days, and on 34 of these prominences were seen. A detailed examination of the spectrum of the chromosphere at 24 points of the limb has also been made on four days, and the spectra of Sun-spots have been examined and photographed. The motions of 51 stars in the line of sight have been spectroscopically observed, 21 of these stars being new. As it is found practicable with the Half-prism Spectroscope to apply this method to stars of the fourth magnitude, it is expected that in course of time the motions of about 300 stars may be spectroscopically determined.

Photographs of the Sun were taken on 145 days, and on 64 of these they show a complete absence of spots, whilst in the preceding year there was a similar absence of spots on 121 days out of 150. The epoch of minimum appears to have occurred about the beginning of 1879, and since last October the outbreak of spots has been very marked. The photographs have been measured, and the areas of spots and faculæ and heliographic longitudes and latitudes of spots have been deduced to the end of 1879. The mean areas of spots and faculæ, and the mean heliographic latitude of spots for each rotation from 1873 to 1879, have been computed and printed. The Spectroscopic and Photographic Results are now separated from the Astronomical Results with a view to their more rapid publication; and as regards the complete volume of Greenwich Observations, the Queen's Printers have courteously made arrangements for accelerating the rate of printing.

The rating of chronometers for the Navy and the distribution of Time-signals form an important part of the work of the Royal Observatory. The number of chronometers on hand is 229, 44 of these being entered for the annual competitive trial. The first chronometer of last year's competition is a very good one, only excelled by the first chronometer in 1873. Every chronometer is tested in a temperature approaching to 100° Fahr. for one or more periods of three or four weeks each. In connection with the question of isochronism, some observations have been made of the arcs of vibration of the balance.

The Greenwich time-ball has been regularly dropped at 1^h without a single failure; in the case of the Deal time-ball there have been five failures. As regards the accuracy of the Westminster Clock, its errors exceeded 1^s on 120 days; on 32 of these it was between 2^s and 3^s, on 4 days between 3^s and 4^s, and on 1 day it exceeded 4^s. Under the head of longitude operations it is mentioned that Commander Green, U.S.N., made an interchange of signals between Greenwich and Porthcurno on four nights (1879, June 25 to 29) for the purpose of determining the longitude of Lisbon as part of a chain of longitudes extending from South America to Greenwich.

In the Astronomical part of the establishment, the Observatory has lost the services of a responsible assistant for at least seven months, through delay in filling up the vacancy caused by the retirement of Mr. Lynn; and although, by increase of employment of computers, routine calculations and even (but with some risk) routine observations are maintained, those services that require more matured judgment cannot thus be supplied.

In connection with the Transit of Venus, 1874, Major Tupman's services have been secured till the end of June 1880; and the observations and calculations are completed for the Sandwich Islands and Rodriguez, and nearly finished for Egypt, Kerguelen, and New Zealand, and 125 pages have been printed, including the text and the greater portion of the tabular part of the Honolulu work.

With regard to the Transit of 1882, the Astronomer Royal has lately placed a Memorandum before the Royal Astronomical Society. From the facility with which the requirements for geographical position are satisfied, and from the rapid and accurate communication of time now given by electric telegraph, the observation of this Transit will be comparatively easy and inexpensive. He has attached greater importance than he did formerly to the elevation of the Sun. For the four principal phases (Ingress accelerated, and retarded; Egress accelerated, and retarded) he proposes to rely mainly on:—1st, the Cape Colony; 2nd, the shores of Canada and the United States, Bermuda, and the West-India Islands; 3rd, the same as the 2nd; 4th the eastern shore of Australia, or New Zealand in preference if telegraph-communication be made; and he remarks that it is highly desirable that steps be taken now for determining by telegraph the longitude of some point of Australia. He considers that it will be useless to repeat photographic observations.

Next he proceeds to report on the progress of his own Lunar Theory. The general principle of this is: to adopt for correction the best existing theory; to compute with the severest accuracy the numerical values of the terms produced geometrically by the tabular coefficients, and also the terms really due to the forces which produce them; and to remove the differences between these by corrections of the tabular coefficients, for which corrections proper factors are prepared. It was a special object to avoid the use of powers of m (a symbol well known to lunar theorists), and to give easy means of computing, not new absolute values, but corrections of existing numerical coefficients (a principle which he has adopted extensively in other branches of astronomy), and also of computing the effect of small external disturbances or small changes of force. Both these are obtained by his process. The heaviest part of the work is the severe computation to which allusion has been made; and this is done entirely by junior computers. The calculations had been carried out in every part to the accuracy of 10^{-7}; but for securing the degree of accuracy proposed it was found necessary to extend many parts to 10^{-8}, and some to 10^{-9}. This has caused a very great addition to the labour, but the work has advanced well, and will, the Astronomer Royal trusts, be finished ere long. While waiting for this, which is to give the correction to every coefficient of the ordinary lunar theory, he is employing himself partly in re-arranging the whole work for publication, and in putting calculations in order for that correction of coefficients; and partly on three ramifications or supplements of the theory, relating to—the effect of the earth's oblateness; the effect of change of position of the ecliptic plane; and the effect of change of excentricity of the earth's orbit, and Lunar Acceleration. The last of these he has completed to his satisfaction, requiring only an examination of the external factor; the two others are progressing. The Admiralty have assisted him, on Estimates, with a moderate grant (of amount named by himself), but much of the expense has been private.

In conclusion, he remarks that all three departments of the Observatory, viz. the Astronomical, the Magnetical and Meteorological, and the Photographic and Spectroscopic, appear at present to be working efficiently and well. But he can easily imagine circumstances which would interfere materially with the successful continuation, in one place, of this triplicate series of observations. Though he thinks this possibility of partial failure worthy the contemplation of the Board of Visitors, yet he does not see any necessity for action of any kind at the present time.

OBSERVATIONS OF THE SATELLITES OF MARS*.— Prof. Pickering publishes a very fine series of measures of position-angles and distances of Deimos and Phobos, made with a filar micrometer at the Harvard College Observatory. The observations of Deimos extend from 1879 Oct. 20 to 1880 Jan. 10, and those of Phobos from 1879 Oct. 26 to Dec. 7. Of the former 207 sets of measures were obtained, and of the latter 74, each set consisting of 5 separate readings. Deimos was observed on 29 nights, and Phobos on 13, and four observers took part in the work by turns, so that systematic errors might be eliminated as far as possible.

THE TRANSIT OF MERCURY 1878, MAY 6 †.—Mr. D. P. Todd observed the four contacts at Washington with a 5-inch Clark refractor, and during the transit made a systematic search for a possible satellite, but without success. He also made a series of measures of the diameter of Mercury at different position-angles with an Airy double-image micrometer. The mean diameter thus found is 11″·84, giving for the value of the diameter at distance unity 6″·604 ± 0″·004. The value of a revolution of the micrometer-screw was determined by the measurement of a white disk about two feet in diameter on a background of jet blackness, placed at a distance of about 3080 feet. Assuming a solar parallax of 8″·81, the resulting diameter of Mercury would be 4781 kilometres = 2971 miles. The following are some of the best previous determinations of the diameter (at distance unity):—Bessel (Heliometer) 6″·68; Main (Double-Image Micr.) 6″·89; Bruhns (Wire Micr.) 6″·52; Kaiser (Double-Image Micr.) 6″·606; Wolff (Heliometer) 6″·23. Mr. Todd's measures do not show any sensible ellipticity.

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* Astronomische Nachrichten, No. 2312.

† Proc. Amer. Assoc. for Advancement of Science 1879, August.

Astronomical Memoranda, 1880, *July.*

Sun. July 1, sets $8^h\ 18^m$, rises $15^h\ 49^m$; July 15, sets $8^h\ 9^m$, rises $16^h\ 4^m$; July 31, sets $7^h\ 48^m$, rises $16^h\ 26^m$.

Equation of time:—Sun *after* Clock, July 1, $3^m\ 38^s$; July 11, $5^m\ 17^s$; July 21, $6^m\ 10^s$; July 31, $6^m\ 6^s$.

Sidereal Time at Mean Noon:—July 1, $6^h\ 39^m{\cdot}7$; July 11, $7^h\ 19^m{\cdot}1$; July 21, $7^h\ 58^m{\cdot}5$; July 31, $8^h\ 37^m{\cdot}9$.

An Annular Eclipse of the Sun, invisible at Greenwich, occurs on July 7. It is visible at the Cape of Good Hope as a Partial Eclipse.

Moon.	*rises.*			*sets.*			*rises.*	
	h	m		h	m		h	m
July 1..	12	2	July 11..	9	31	July 21..		
2..	12	31	12..	9	48	22..	8	12
3..	13	7	13..	10	4	23..	8	33
4..	13	49	14..	10	23	24..	8	51
5..	14	43	15..	10	45	25..	9	8
6..	15	43	16..	11	16	26..	9	27
7..			17..	11	56	27..	9	45
8..	*sets.*		18..	12	51	28..	10	6
9..	8	56	19..	14	2	29..	10	33
10..	9	14	20..	15	22	30..	11	6
						31..	11	47

New Moon, July 7, $1^h\ 21^m$; First Quarter, July 14, $18^h\ 16^m$; Full, July 21, $9^h\ 2^m$; Last Quarter, July 28, $11^h\ 41^m$.

Mercury is an evening star, at greatest elongation (26° 21′ E.) on July 8. On July 1 he sets $1^h\ 25^m$ after the Sun.

Venus is in superior conjunction on July 13.

Mars is an evening star in Leo, setting shortly after the Sun. He is near Regulus on the evening of July 17, being about 40′ N. of that star.

Jupiter is nearly stationary in Pisces; in quadrature on July 9. Diameter:—July 1, 37″·2; July 31, 41″·0.

	R.A.	Dec.	tr.	rises
July 1,	$1^h\ 5^m{\cdot}7$,	5° 35′ N.,	$18^h\ 23^m$,	$11^h\ 51^m$
31,	1 14 ·0	6 17 N.,	16 33	9 58

Daily rate of rotation 870°·60 = 2^r 150°·60; Hourly rate 36°·275.

Saturn is also nearly stationary, about one third of the way from *a* Piscium to *β* Arietis.

	R.A.	Dec.	tr.	rises
July 1,	$1^h\ 46^m{\cdot}1$,	8° 22′ N.,	$19^h\ 4^m$,	$12^h\ 17^m$
31,	1 51 ·0	8 42 N.,	17 10	10 22

	Outer Ring.		Inner Ring.		Ball.
	Maj. Axis.	Min. Axis.	Maj. Axis.	Min. Axis.	Diam.
July 13 ..	39″·86	10″·66	26″·50	7″·09	15″·6
Aug. 2 ..	41 ·30	11 ·15	27 ·47	7 ·41	16 ·6

The south side of the rings is visible, the elevation of the Earth above their plane being 15° 31′ S. on July 13, and 15° 40′ S. on Aug. 2; and of the Sun 13° 6′ S. and 13° 23′ S.

Phenomena.

	G. M. T.		
	h	m	
July 1	12	42	J. i Ec. D.
	12	48	[782] Oc. R. 274°.
2	13	38	J. i Tr. E.
4	13	15	J. iii Sh. I.
7			Ecl. of Sun, invisible at Greenwich.
	22		*Mercury* Elong. 26° 21′ E.
8	14	36	J. i Ec. D.
9	13	18	J. i Tr. I.
	14		*Jupiter* in quadr.
10	12	40	J. i Oc. R.
12	14	28	J. ii Ec. D.
13	12		*Venus* sup. conjunct.
14	11	26	J. ii Tr. I.
	14	6	J. ii Tr. E.
15	12	54	J. iii Oc. D.
	15	18	J. iii Oc. R.
17	8	1	[5253] Oc. D. 73°.
	10	39	[5286] Oc. D. 34°.
July 17	14	34	J. i Oc. R.
18	11	54	J. i Tr. E.
20	13		*Saturn* in quadr.
	13	39	[6607] Oc. near approach 20°.
21	13	59	J. ii Tr. I.
22	11	9	J. iii Ec. D.
	13	47	J. iii Ec. R.
23	11	45	J. ii Oc. R.
24	12	52	J. i Ec. D.
25	11	33	J. i Tr. I.
	13	46	J. i Tr. E.
26	10	54	J. i Oc. R.
27	16	24	101 Pisc. Oc. R. 292°.
29	14	53	ζ Ariet. Oc. D. 30°.
	15	4	ζ Ariet. Oc. R. 11°.
	15	10	J. iii Ec. D.
30	14	14	J. ii Oc. R.
31	14	46	J. i Ec. D.
	15	25	99 Tauri Oc. R. 299°.

The angles at Oc., D., and R. are reckoned from the *apparent* N. point towards the right of the Moon's inverted image.

Publications received:—Prof. Simon Newcomb, *Transformation of Hansen's Lunar Theory compared with the Theory of Delaunay* (Washington, 1880)—Prof. S. P. Langley, *Address to Section A of American Association for Advancement of Science* (Salem, 1879)—C. E. Burton, *Physical Observations of Mars*, 1879–80 (Trans. Roy. Dublin Soc., New Series, Vol. i. Part 12)—Stonyhurst College Observatory, *Meteorological and Magnetical Observations*, 1879—H. C. Russell, *Rain and River Observations in New South Wales*, 1879 (Sydney, 1880)—Balfour Stewart, *On the Long Period Inequality in Rainfall* (Proc. Lit. & Phil. Soc. Manchester, Vol. xix. No. 10)—Howard Grubb, *On the Equatoreal Telescope and on the new Observatory of Queen's College, Cork* (Proc. Roy. Dublin Soc.).

EDITOR.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

No. 40. AUGUST 2. 1880.

Practical Spectroscopy in 1880.—The Language.

WHATEVER the former scientific ages of the world may have been famous for or distinguished by, the present is surely the age of spectroscopy—an age, at least, when near a thousand observers are suddenly found at work on that subject, where only three, possibly four, were known some thirty years ago, and they generally using an opening between window-shutters for the modern most refined "slit" arrangement, distance for a collimator, long eyesight in place of an examining telescope, and one big lump of a simple prism in place of a train of prisms of very compound construction.

Nor can it be wondered at that such myriad popularity has now at last befallen the subject, at least ever since the opening up by Bunsen and Kirchhoff, Angström and Thalén, Plücker and Hittorf; for, one after another, every new spectroscopic explorer during the last twenty years, who has followed those mighty leaders into the wonders of the spectral light-land, brings back some still more precious discovery than before, whether in Astronomy or Physics, Pharmacy or Chemistry, or any other of the practical sciences. Wonderful, too, as these discoveries generally are, there is somehow usually a peculiar presentiment of certainty about them, which can hardly claim existence or parallel in any other equally refined, discriminating, and transcendental branch, whether of optics or any other applied science.

Hence it is that the crowds of observers with the slit and the prism, the collimator and the telescope, redouble themselves over the land, convinced beforehand that all the time they can spend therein is well spent, and their pains certain to be rewarded. So then, in truth, how voluminous their documents do become; for what data can be multiplied so easily as merely numerical statements of any thing measurable? and as every modern spectroscope is turned out by its maker with some kind of divided scale, micrometer, graduated arc, and what not, away goes each enthusiastic observer reading off the numbers instantly before his eye, entering them in countless series into his books; and only when the ecstatic pursuit of bright lines and dark bands and haze is brought up all standing by every page being filled, only then is there leisure to think of a few consequences—or for one observer to compare his numbers with those collected by another; and then, behold, they each talk an unknown language! There is no one who can interpret their words into the universal recognized speech of theoretical science in nature; and the laborious measures of each man, standing apart and uncomparable with those of his fellows, lose in scientific value, and may be lost or perish from want of human sympathy.

Here, then, is evidently an evil under the sun, claiming, so far as it still prevails, instant correction; for it is becoming worse in the daily accumulation, the mountainous piling up of documents year after year, and will be even more serious in the multiplying spectroscopy of the future. Then let every new hand in the subject be fully assured that, while accurate numerical spectrum-place is undoubtedly the one ruling cynosure to be aimed at through all spectroscopic observation, it is not to be obtained by reading off, ever so minutely, any kind of scale whatever that either is or can be attached to any prism; and this is because, besides other reasons, every bit of glass that is successively made into a prism has its own prismatic individualities or idiosyncrasies—pulling out one part of the spectrum of light more or less than another; and for no more assignable reason than that one piece of glass is not another piece of glass, though they may look ever so alike to ordinary external examination, and have been ground and polished ever so nicely to the same refracting angle by the working optician.

But though we must be clear, even to severity, on this point, there is no occasion for any hard words; so that if indeed some, happily by no means all, of the best of modern observers have published occasionally spectrum-places of lines or bands of light seen by them, in terms merely of the scales engraved on the brass rims of their own prismatic spectroscopes, and without any other scientific importance than that (and by so doing have wrought some remarkable scientific confusions in their time, such as the disputed resemblances or identifications a few years ago between the spectral phenomena of Solar Corona, Zodiacal Light, Aurora, and a certain iron line, which was "green"), these gentlemen have been neverthelesss influenced by a fine enthusiasm to distinguish themselves worthily by contributing to the newest scientific information of their day, but have been merely a little hasty in one part of their proceedings. In which case, however, they will be none the worse for reflecting maturely upon it now; especially as we shall soon find that some most appropriate principles to guide a nascent science of mensurational spectroscopy were really worked out during time long past; so that actually thirty or forty years ago, or a lifetime almost, before the world was stirred by the dawn of the general spectroscopic day, and when, as I have said before, there were hardly more than three or four minds publicly known to be engaged upon it, yet those were master-minds, and did even research into some things then, almost as if in very preparation for the needs of the present day.

Thus, if we take a flight back to the 'Philosophical Transactions' for 1835, we shall find there (duly enshrined among the now classic memoirs of other great departed, such as Faraday, J. W. Lubbock, Whewell, Rowan Hamilton, and Pond) a short, but terse and powerful paper by the Rev. Baden Powell, containing, though perhaps somewhat unconsciously, the germs of the only orthodox scale for spectroscopy which the science need ever hope for; and whose simultaneous importance and value for the theory of light was stated in that (in spectroscopy) distant day to have been likewise clearly perceived and earnestly insisted on by "Professor Airy * and Sir John Herschel."

This method depended on the wave-lengths of light, and had its peculiar strength therein; because, while every thing else varied both considerably and irregularly in any experiments that could be made or computed, the one only thing which kept up exactly with the refractive index was, as described in the gradually developing theory of light in that day, "the *length of the interval* for each ray, the Newtonian fit, or the undulatory wave, which (by whatever name it be called) has demonstrably a real existence in the nature of light, and the value of which, for each of the definite rays, has also been determined by Fraunhofer, with his usual accuracy, from phenomena totally independent of refraction, viz., his very remarkable experiments, in which a spectrum, absolutely pure and perfect, is obtained without the intervention of any prism, from the *interferences* produced by a fine grating of parallel wires covering an object-glass. The positions assumed by the several rays here depend on nothing but the lengths of their periods or waves simply as such; and the intervals between them are precisely proportional to the differences of those lengths. These lengths decrease from the red to the blue end of the spectrum."

Seeking, then, not only an empirical law, which should connect the wave-length with its place in the spectrum, but rather the exact formula given by the theory of light for the relation between the length of a wave and the velocity of its propagation, and the agreement or disagreement of that with Fraunhofer's measures of spectra formed by several different media, Prof. Baden Powell comes to the conclusion "that the theory of undulations, as mathematically set forth by M. Cauchy, supplies at once both the law and the explanation of the phenomena of dispersion;" or, as we may roughly describe the result for spectrum-mapping purposes, that to each place in the spectrum there belongs a certain wave-length, and any thing that could alter the wave-length of any ray of light would alter its place in the spectrum simultaneously.

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* In special testimony of this older than Nestor's early and intimate acquaintance with, and varied experimentations in, the true *arcana* of modern spectroscopic observation, I may mention that in 1856 he kindly lent me, for use in the Teneriffe astronomical experiment, several of his old slit-arrangements for solar spectroscopy. Very curious old things in tarnished and blackened brass they were too, but effective as small slits worked by little screws of very fine thread.

Then what are these all-important lengths at any definite places in the spectrum?

At the places of the principal black, or "Fraunhofer," lines in the solar spectrum, from B to H, they are as follows (in the first column), according to that paper of 1835:—

At	Paris inch.	British inch.
B....	0·0000 2541	0·0000 27082
C....	0·0000 2422	0·0000 25814
D....	0·0000 2175	0·0000 23182
E....	0·0000 1945	0·0000 20730
F....	0·0000 1794	0·0000 19121
G....	0·0000 1587	0·0000 16915
H....	0·0000 1464	0·0000 15603

But the unit of measure here being that latterly disgraced one, the old Paris inch, as once adopted for certain purposes in Germany, and bearing a relation to the British inch of 1·0658 to 1·0000, let us reduce the numbers to British inches, and they become those given in the second column—something evidently so very small that the common sense of our philosophers, when writing for the mass of the people at that time, instead of plaguing them with so many vacant decimal places in a long fraction, bent the bow in the other way, and told them how many of such very little undulations would be comprised in an inch (British of course). The waves of light at each standard (Fraunhofer) line were therefore then declared to be:—

At B, 36 925 to the inch.
C, 38 739
D, 43 137
E, 48 239
F, 52 299
G, 59 119
H, 64 090 *

And there can be no doubt that the community did form a clearer idea of the size of waves of, say red light, by being told that they were 38,000 to the inch, than that they measured 0·000025814 of an inch. But there were to be experienced more troubles, more examples of how to make a matter which was abstruse enough

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* The conversion of one of these series of numbers into the other is performed most rapidly by the use of a table of Reciprocals—whereof an example from 1 to 10,000 was published by the Society for the Diffusion of Useful Knowledge in 1840; and another, from 1 to 100,000, by Colonel Oakes, in 1865: Hirst Smyth and Son (Taylor and Walton), 137 Gower Street, W.C.; and C. and E. Langton, 150 Fleet Street, London.

already, still more difficult for practical apprehension, before that spectrum-place for spectroscopic observers should be reduced to its simplest form and most foundational numbers.

Strange to say, too, the greatest obstructions to any such desirable eventuality were found in Germany, the land of Fraunhofer, but where Fraunhofer had been forgotten, precisely by those who should most have remembered him, and at the instant when his wave-lengths of light should have met their world-wide reward, in universal application and every-day employment. For when the year 1860 published to mankind the startling intelligence from that father-land of the discovery of "spectrum analysis," the venerable Professor Bunsen (excellent chiefly in chemistry) produced his spectrum results in mere arbitrary numbers, they being in fact, merely the accidental scale attached to his prismatic spectroscope. Again, and almost simultaneously therewith, the learned Professors Plücker and Hittorf were splendid in their physical researches, by means of the spectroscope, into the behaviour of the various chemicals they operated on, but expressed their results also, on their fine quarto map-plates of spectra, in no other more significant and explanatory manner than a nicely engraved scale of equal parts on the paper, to set forth the unequal deviations of dispersions in their prisms. And, lastly, Professor Kirchhoff, who almost attained immortality by the magnificent stride in advance which he made in the length and minuteness of his Solar-spectrum map (vastly beyond any thing ever seen before in the world), lost all his chances again of that best of immortality, the being useful for ever, because not only was his map unfurnished with any thing better than an accidental scale, on paper also, but his prisms contracted some and expanded other parts of the base-line of light so variously, as to become quite a broken reed for any one to lean on for exact spectrum position; and certainly proved nothing in that department for the pure region of theory, a something of, and for, the mind of man far above any forms of mere instrumentation.

But happily what Germany had forgotten or neglected, Scandinavia had picked up and cherished. For, in the far north of Sweden, at the ancient University of Upsala, Professors Angström and Thalén were at that very time labouring to perfect Fraunhofer's grating method of diffraction spectra—Angström chiefly applying it to the Sun, and Thalén to chemicals rendered incandescent by the induction-spark; but both of them expressing their work finally in only wave-lengths of light. They continued, too, for a while, as Fraunhofer had long previously begun, with the use of that most convenient unit of measure so universally known to all Saxon, Scandinavian, and Teutonic peoples, the inch—that happy medium between the infinitely great and the infinitely small, between the size of man's planetary home the earth and the size of a molecule of gaseous matter; and it seemed as if the Northmen were once more, though now in the service of peace and for the promotion of science, just about to conquer all Europe, when, at the last instant before coming out in print, straining too hard to secure something of an unfortunate adventitious glitter of the modern day, Angström, in a fatal moment, failed in loyalty to the hereditary metrological traditions of his heroic country, and actually published the chief labour of his Northman's life, his great Solar-spectrum map, which he so fondly desired to become the Normal Solar spectrum of all the civilized human race; yes, published it, but how? In terms of wave-lengths of light of course, just as Fraunhofer himself would have desired; but expressed, as Fraunhofer never did, in terms of the latest invented, most inconvenient standard of measure ever proposed to any nation or people, viz. the French metre—that metre, actually supposed by some amongst the public to be in a manner necessarily scientific, because it is new, because it is, like Greek, unsuitable to all the ordinary purposes of life, because of its bad, crooked, roundabout principle of earth-commensurability, viz. a straight line formed upon a curved surface, and that badly carried out; and because it was invented by a few o'er-vaulting Doctrinaires, who at that time cared neither for God nor the people, neither for religion nor for history. Such, then, is the French metre, blindly taken up by a few even of our scientific men, when it is really so very much more the symbol of Communism and Social Democracy, of the destruction of libraries and the drowning of La Place's MSS.: the curse of the future, born as it was amidst the reign of terror 90 years ago, resuscitated when Paris was burning in all her chief palaces 9 years ago by the hands of her own children; and used in secret as the emblem of RED antipathy to all governments both human and divine—a scriptural revolutionary mark of the beast, which several other countries besides France, quaking to their foundations, are now beginning to fear they will have to suffer from before the present generation has entirely passed away.

But the lamentable metrological step for spectroscopy was taken in Upsala, past all ordinary recall. No other large solar spectrum than Angström's had been observed throughout in wave-lengths, and he had given them in terms of the French metre. Voluntarily, therefore, or involuntarily, these numbers are now being used by the rising spectroscopists of the day. An immense number of new spectroscopic observations are being taken in those terms, and many men who ought to have denied both themselves and their incomes for the sake of principle, are beginning, not only to tolerate the method, but to try to sweeten it for this country's fatal swallowing. Do, however, what they may, there will come up, almost as if to check their wreckless worldliness, two very serious difficulties wherever the new French metre is introduced in place of the ancient inch; for, *first*, if a wave-length of light at the spectrum-place, say of the solar line D, measures only 0·0000 23182 of a British inch, and if that has been voted already an inconvenient fraction to have to employ, what sort of fraction will the same natural and real length of an undulation of light be, in terms of a metre, which is = 39·37079 of the same inches? The sensible working answer to that question is sure to come up in time, though perhaps only when English Franco-metric men shall have tried and exhausted all their futile inventions to hide the real difficulty and glorify the badge of the Commune, which they, we hope, really hate, and do not in secret fraternize with.

Meanwhile the wave-length of light at D, just quoted, when reduced to parts of a metre, is evidently

$$\frac{0{\cdot}0000\ 23182}{39{\cdot}37079} = 0{\cdot}000\ 000\ 5888, \text{ nearly.}$$

This is surely a large number of vacant fractional places for a man to have to carry about the world with him; wherefore it has been proposed, by those who palter with the real evil, to call the last figure given above a "tenth metre," or to imagine a unit so small as to be only the billionth of a metre; and then to state, that 5888 of these totally invisible units of measure span the length of that particular undulation of light. But the matter cannot rest even there, for as spectrum-place is measurable now with the best instruments to five places of figures, it becomes necessary to subdivide these invisible units, and to say, in an inapprehensible sort of manner, that 5888·1 of those unknown things are equal to a length which any one can, after all, form a very good practical idea of, when simply told, that there are 43,137 of them in an inch British—a unit of measure which he sees, uses and handles every day; and of which there are 25 in the sacred cubit of the Bible, the geometrical cubit of the whole Earth, because it is the ten-millionth part of the said Earth's semi-axis of rotation.

And this leads on to the *second* objection against the French metric arrangement; for a scale for spectrum-place, made up of equal parts, bearing numbers according to wave-lengths directly, produces a most dissimilar representation to any thing ever seen in a prismatic spectrum. The prism does indeed, we all know, rather crowd up the red end of the spectrum and stretch out the violet; but the wave-length method of representation expands the red so excessively as, in the solar spectrum, to leave the lines there few and far apart, but to crowd up the violet to such a degree that the lines observed there in Nature have not standing room in the map. Now this is what Angström's Normal Solar spectrum in its French dress does—the very short visible distance between solar A and B being measured by 737, and the very long distance between G and H being measured by only 339 of the same units.

That particular practical defect would no doubt be found if simple wave-lengths were expressed in fractions of an inch directly; while such a scale would also have the further defect of Angström's French metric numbers, of *decreasing* from red to violet of the spectrum, or exactly opposite to the increase of the index of refraction, which is itself the very agent in all prisms for producing any spectrum at all. But the inch does not leave us in this difficulty. With the kindly feelings and the practical suitableness of old hereditary customs of the human race for what is most important to the poor and needy, the uneducated and the most numerous in the mass of the people, the inch and its customs have, as we have already hinted, opened a way of escape for the scientific and the few out of the double absurdity they have been landed in by the French metre; and it is a way as efficacious as simple.

Let the wave-lengths only be expressed, as any clever and handy mechanic would naturally express the size of any thing so very small, viz. by the number of them in an inch, and then the shorter lengths of violet over red waves of light, when used as divisors of the inch, produce precisely the greater numbers which we require at that violet or *more* refrangible end of the spectrum. In fact, a spectrum drawn out on that principle of scale (viz. of wave-lengths expressed by the number of waves in a larger unit of measure) comes most charmingly between the appearance of the spectrum as seen in a prism on one side and in a diffraction-grating on the other; and which of these two methods is going to be the favourite with spectroscopists in the future? "That I know not, no not I;" so admirable have been the machines lately invented by L. M. Rutherfurd, Esq., of New York, for the construction and multiplication of the gratings; so that, though at one time Fraunhofer was the only man who possessed even a moderately close grating, all sorts of observers may now revel in possession of superb ones, having 18,000 lines and more to the inch *.

In every way, then, we obtain a more natural scale and representation of spectra by the use of numbers of waves to the inch, than by the length of waves in terms of a metre, or the billionth of a metre either. And though the British Association for the Advancement of Science has endeavoured to give that French metrical method a new life, by converting the fractions of a metre into the number contained in a metre (or by referring again to the reciprocals of the numbers †), yet the figures are not so handy as those of the inch content, the metre having to use generally 7 places of numbers to express what the inch can do as well with 5 places.

Besides which circumstance, it is necessary to take into account that the French-following peoples have not adopted or looked with favour on wave-lengths expressed by wave-numbers in a metre; so that in practical reality the only two existing and conflicting systems for spectrum representation are, at present, either wave-lengths in French metrical fractions, or wave-lengths by their numbers in the inch British, the old French inch having been

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* We are told that Mr. Rutherfurd's machine is driven entirely by water-power, and after it has once begun a grating is never interfered with until it has finished it—a period generally of ten days and ten nights, in a room of most equal temperature, the water pressure being also specially regulated.

† They call them also "oscillation frequencies," but which seems to needlessly complicate the question by bringing in the idea of time and its arbitrary units of measure, when reference to linear measure is all that is really required to fix and describe the phenomenon under observation.

WAVE-LENGTHS OF LIGHT AT DIFFERENT PARTS OF THE SPECTRUM.

Part of the Spectrum approximately.	In terms of the French metre.	In terms of the British inch.	The number in a British inch.
Ultra Red	0·000 000 8758	0·0000 3448	29 000
	0·000 000 8466	0·0000 3333	30 000
	8193	3226	31 000
Crimson Red ..	7937	3125	32 000
	7697	3030	33 000
	7470	2941	34 000
Red	0·000 000 7257	0·0000 2857	35 000
	7055	2778	36 000
	6865	2703	37 000
Scarlet Red ..	6684	2632	38 000
	6513	2564	39 000
Light Red	0·000 000 6350	0·0000 2500	40 000
	6195	2439	41 000
Orange	6049	2381	42 000
	5907	2326	43 000
Yellow	5773	2273	44 000
	0·000 000 5644	0·0000 2222	45 000
Citron	5522	2174	46 000
	5404	2128	47 000
	5292	2083	48 000
	5184	2041	49 000
Green	0·000 000 5080	0·0000 2000	50 000
	4980	1961	51 000
	4885	1923	52 000
	4792	1887	53 000
Glaucous	4704	1852	54 000
	0·000 000 4618	0·0000 1818	55 000
Blue	4536	1786	56 000
	4456	1754	57 000
Indigo	4378	1724	58 000
	4305	1695	59 000
Violet........	0·000 000 4233	0·0000 1667	60 000
	4164	1639	61 000
	4097	1613	62 000
Lavender	4032	1587	63 000
	3969	1563	64 000

annihilated by the French people themselves, and all accidental and arbitrary prism-scales being allowed now by every one to have been a mistake which must never be repeated.

But the two "wave" methods existing as above, will not only oppose each other by complete dissimilarity through all the numbers employed, and by one of them beginning with its smallest numbers at the red end, and the other at the violet end of the spectrum, but their inherent spirits of opposition will manifest themselves in the orientation of their spectra also; so that while the French-metre party are fond of beginning their spectra with the red end to their right, the British-inch party begin with theirs to the left; and who shall decide between them?

Nature and all civilized mankind, of course; and they settle it thus promptly.

Every written and printed page in every European country begins at the top and on the left, and continues thence to the right; the left side of a sheet of paper, and the upper corner, is therefore the universally allowed place for the beginning of whatever is to be depicted or written upon it; we are bound, therefore, to place on that side and in that corner whatever Nature shall call the beginning of her spectrum. And which part does she so call? Here is the test. Whenever the temperature of bodies is raised out of and above that grand standard which is the condition of human existence, up to that intensity where they begin to give out light, and thereby furnish the first food for the spectroscope, red is the first spectral colour which appears, and violet the last, by the admission of all parties; so that our argument is now ended.

Whatever conclusions pure literary or theoretical men may arrive at on the whole course of this inquiry, I may perhaps be allowed, as a practical man, to testify further that I had no full satisfaction, and my observations had not their completest accuracy, until I had amended, for myself in private, the language in which their spectroscopic results were to be expressed, both numerically and graphically; and until, also, I had mounted every spectrum map in my possession with the red end towards the left, and given it a scale of wave-numbers per British inch*, increasing, of course, from left to right, and when in columns from top to bottom; while in order to have the means of immediately interpreting into numbers understandable to me any chance observations on my subjects that I might meet with in books by French metrical men, I prepared a simple conversion-table, of which the few numbers on p. 499 will give a practical idea.

Will the reader consent to hear any thing more about spectroscopy in the language of the last of those columns alone?

PIAZZI SMYTH.

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* Had British law allowed, I should, of course, have preferred to use geometrical, *i. e.* evenly Earth commensurable inches, such as the inches of the inspired design of the ancient Great Pyramid and the Bible, whereof 500 000 000, and not the 500 500 000 very nearly, of the British inches, measure the length of the Earth's axis of rotation.

Solar Physics *.—Part I.

Leaving to those of wider knowledge the survey of the whole field of scientific labour, it has seemed to me that I could best present to you some account of that branch of it with which I am most familiar, which is that of "Solar Physics."

This study is essentially a modern one. Astronomy, which in the earliest times could only mark the annual path of the sun, or count the stars, with the invention of the telescope still concerned itself more with the motions of the heavenly bodies than with their physical nature. It sought out new methods of precision to fix the places of these stars, and to mark out for the navigator the path of the Moon on the celestial dial; it united itself ultimately with the sister science of mathematics in predicting the places of the heavenly bodies from the law of gravitation; but it was still as a surveyor and marker of boundaries in the field of space that the observer chiefly laboured, and we associate the most striking triumphs of the classic astronomy with this work of precision. It is this aspect that appeals even to the imagination, and which is seized as distinctive by the poet of Urania:—

"That little Vernier on whose slender lines
The midnight taper trembles as it shines,
A silent index, tracks the planets' march
In all their wanderings through the ethereal arch,
Tells through the mist where dazzled Mercury burns,
And marks the spot where Uranus returns."

These are noble aims and noble results; but it is curious to see how observers of the last century, who had learned this excellent lesson of precision, had learned no other, and remained indifferent to a great question to which the old methods did not apply. We are called into existence by a great central fire, the Sun, by which we continue to exist from one hour to another. What is it? what is this heat which it pours into space, and with whose cessation we shall cease? How long will it continue to feed our lives? A few years ago, with almost the sole exceptions of the Herschels and Pouillet, no one even asked these questions, much less intelligently sought their solution.

It is hard to say to whom the awakening of attention is due; yet if any one were to be named, it should perhaps be the Italian physicist Melloni, "the Newton of heat."

His book, 'La Thermochrose,' has to me an attraction of its own; for the author, with the ingenuous confidence of his nation, begins, not by describing his thermopile or galvanometer, but by taking the reader into his personal experience, and telling him how even as a child he felt an invincible curiosity about what we have just seen hardly any one else then cared for, and how, rising long before dawn, he loved to seek some quiet spot, to wait there in the silence of the sleeping world the first beams of the sun, and as he felt their warmth and heard the stir of life they awakened round him, how he too was stirred with wonder and interest as to the nature of that mysterious thing, radiant heat, and resolved to give his future to its study. If, to distinguish a cause for wonder and inquiry in what to the common mind has called for neither, be a characteristic of genius, then Melloni must be allowed its possession, and in his but too short years he showed the world how much interest and importance lay in this then neglected study, which so many with clearer knowledge and better methods follow to-day.

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* Address of Prof. S. P. Langley to Section A of the American Association for the Advancement of Science, August 1879. Communnicated by the author.

Fraunhofer's previous work had prepared the way for the spectroscope, and with the now awakened interest in these questions, its employment by Kirchhoff in 1860 may be said to inaugurate the present study of solar physics, as distinguished from the classic astronomy, which concerned itself with number and measure first, and in a wholly secondary degree with the physical characteristics of the heavenly bodies. This study occupies itself with the former, indeed, but chiefly in aid of other investigations; and by the study of solar physics, then, we mean much more than a telescopic examination of the Sun—we mean besides this the analysis of its radiations by the spectroscope, their summation by the photometer and thermopile, the determinations of its heat and the possible effects of changes in it on terrestrial meteorology, and generally the pursuit of all those problems which unite the methods of physics and astronomy.

In 1860 we already knew that the Sun was surrounded by an envelope then visible only during total eclipses, and which was surmised to be gaseous; and of the Sun itself we knew very little more than that it was a hot globe with spots upon it; for, though Schwabe had observed the periodicity of the spots, and Carrington was already at work, their results were not wholly public, and the facts of the variable velocity of the Sun's rotation were rather the surmises of a few than part of the body of acquired knowledge. Since then this branch of astronomy has grown almost to the position of an independent science, and, though it has not yet been distinctly divided into specialities in its turn like its elder sisters, yet we already see a tendency to their formations. Thus, with the study of the motions of the solar surface we associate with the names just mentioned those of Spörer, De la Rue, and Wolf; with eye-studies of the photosphere or solar meteorology, those of Dawes, Secchi, and others; with the telescopic use of the spectroscope those of Huggins, Janssen, Lockyer, Secchi, Young, and Tacchini. The work of mapping the spectrum, begun by Kirchhoff, has been continued by Angström, Mascart, and Cornu, while photography, in the hands of Rutherfurd, Janssen, and Draper, has largely superseded telescopic studies of the photosphere; and the list might be enlarged indefinitely. Let us glance at part of the work done by these during the past twenty years, for their labours make the history of our study.

The work of Carrington, completed in 1861, taught us what had before been suspected—both the periodicity of the spots and that this great globe, so far as we can see it, has different periods of rotation, its equatorial zones completing a revolution in less time than its polar ones. We know very little more on this point now, the cause of both phenomena remaining wholly mysterious to-day.

In the next year (1862) an impulse was given to the study of the solar surface by the announcement of a supposed discovery of gigantic individual bodies in it, of from 500 to 1000 miles in length, distinct from each other, and existing in countless numbers. This extraordinary statement was not easily disproved, as it is with great difficulty that the real structure is discernible by the best telescopes. Forms, we can scarcely call them "bodies," are undoubtedly there, of a size and in numbers which could only exist on so vast a surface, and which are no doubt the chief immediate cause of the sun's light and heat—but what are their causes in turn, and what is their real nature? The suggestion was made at the time by the then, perhaps, most eminent living astronomer, that they might be, in a sort, living things—*beings*, in fact, whose vital force gave us the solar heat; a suggestion which we may smile at now, but which was received at the time with a kind of awe, as adumbrating some possible truth. Of its author I would speak with all possible respect in citing it, which I do here, as nothing can better indicate the obscurity of our knowledge, even at so recent a period. We may look back on such a possible suggestion and its connection with that "vital force," now itself banished by physiology, as a kind of landmark on the road we have travelled. Our science, young as it is, is old enough to have had its age of fable.

Since that time, in France, in Italy, in England, and here, thousands of telescopic studies have been made with the purpose of defining these forms, and of learning more about the growth of those mysterious objects with which they are associated—the Sun's spots, which drew the attention of Fabricius and Galileo, and which still attract our own more than ever to-day, with problems which seem nearly insoluble. Every thing we see convinces us that the solar surface in which they are formed is neither a solid nor a liquid, but composed of volumes of whirling vapours; yet through this vapour, which seems to offer no resistance, come eruptions of explosive violence such as one would suppose must arise from the sudden bursting of some rigid shell. The turmoil within the areas of disturbance is so great, the area itself so vast and enclosing such diversities of action, that we are still doubtful how far this action is downward, how far upward.

Under the circumstances, we can hardly say that twenty years of observation in this department have brought us results commensurate with the labour expended, nor have we derived great aid from photography until some recent advances of which I have presently to speak.

A review of our past studies of the corona is a review of the solar eclipses during the past twenty years; for it is a fact, unparalleled in the sciences of observation, that the opportunities for this knowledge last only minutes, and are separated by intervals of years. Till 1860 it was uncertain whether the protuberances belonged to the Sun or Moon; but in that year the then newly applied photographic method made it nearly certain that they were parts of the former, and previous surmises that they were extensions of an envelope everywhere surrounding the Sun were confirmed. In 1868 some traces of the corona were first photographed. The spectroscope was used upon the prominences, their gaseous nature was proved, and nine of the chromospheric lines were determined; and nearly together Messrs. Janssen and Lockyer made the discovery that these lines could be seen without an eclipse: 1869 brought that eclipse which traversed our own territory, and in this the distinctive coronal line was first observed by Young and by Harkness; while in this, and yet more in the eclipse of 1870 and 1871, we obtained better photographs of the corona, and greatly increased our knowledge of its apparent structure.

It is hardly possible to present even in the briefest way any review here of the separate history of spectroscopic research since 1860, during which time it has been connected with most of the important steps in every field of our study. It has, in the hands of Messrs. Huggins, Zöllner, and Young, made visible to us the forms of the chromosphere, and enabled us to measure the velocity of motions upon the Sun otherwise beyond estimate, while at the same time it has given us independent data for the absolute velocity of other suns in space, and for that of the rotation of our own solar photosphere. It, has, in the hands of Secchi and others, connected our knowledge of our Sun's physical constitution, and perhaps of its past history, with that of other suns, and even assumed to give us information whence we might infer something as to their mass, as well as physical constitution, while it has immensely increased the number of lines mapped twenty years since in the spectrum, and modified the ideas we then entertained as to the interpretation of these lines themselves.

The important question of the amount of heat received from the Sun has been the subject of almost uninterrupted experiment and study during the period under review, but without essentially altering the data of Herschel and Pouillet which we already possessed. In this field the French physicists and our countryman, Mr. Ericsson, have been prominent workers, and we have attained results possessing all desirable certitude relatively to our knowledge in other branches.

Investigations on the solar temperature have been carried on by many observers, but with results which are thus far less satisfactory.

I am painfully sensible of the inadequacy of this review of the history of solar physics, but the brief time before me warns me to come from its past to its present. Within the last two years the difficulties I have alluded to, as so great in eye-studies of the solar surface, have been singularly modified by the remarkable advance of solar photography at the hands of M. Janssen. When I recently visited his observatory at Meudon, I found him producing original negatives on a scale of nearly thirty English inches to the solar diameter, and which bear enlargement to nearly ten feet with remarkable precision; and one of these negatives, which presents over a million discrete cloud-forms, can be taken in $\frac{1}{3000}$ of a second. In another branch of photography, that of the reproduction of spectral lines, for which so much is due to Rutherfurd and Draper, I know nothing more surprising than the recent success of Captain Abney (of the Royal Engineers) at South Kensington, who has photographed the red end of the spectrum, and far beyond the red end, to a wave-length of about 12,000 tenth-metres. As this statement may of itself convey no clear idea to some of my audience, let me explain in less technical language that it means we can now photograph objects in absolute darkness—objects which are not luminous—simply by the heat they give out. This is a discovery which obviously lends itself to important practical applications, while it is of further interest as bringing another proof of that identity of heat and light, with radiations differing only in wave-length, long since surmised by physicists, and asserted prominently by Dr. John W. Draper, whose photographs are also the earliest in the path which Captain Abney has carried on by independent methods. Theoretically, there would seem to be no limit to this power of photography so long as objects radiate any heat whatever.

Of recent coronal studies, I have only to speak of the opportunity afforded by the eclipse of last year in our own Western territory. Observed as it was in the pure air of the Rocky Mountains, we found an immense and hitherto almost unsuspected extension of the corona in the direction of the solar equator, such as to make it increasingly probable that the outer corona and the zodiacal light are different appearances with a common origin. The physical constitution of the inner corona seemed to be modified by the weakness or absence of a former constituent; and perhaps we may say that some additional knowledge was gained as to its telescopic structure and its absolute light, while the polariscopic evidence was contradictory.

In the light of our latest knowledge, what, then, is the corona? We do not know. We have literally had but about twenty minutes in the last twenty years to look at it, and from that brief study it remains every way problematical. The extent of this vast solar appendage is unknown, its constitution is unknown, its function is unknown, and it is still uncertain whether we can devise any means for its study which will free us from this dependence upon mometary glimpses. Our only hope, since the most powerful telescope is useless in our lower atmosphere, seems to be to transport our observatory to some mountain-height, like that of Etna or the elevated tablelands of Colorado. There, even, we cannot be sure of seeing it without an eclipse; but there, if anywhere, ingenuity will be hopefully employed in an endeavour to remove the difficulties which bar the way. After spending some weeks this year myself upon Mount Etna, on which the new solar observatory is to be built, I can testify to the excellence of such a station; and yet, when we have sites equally good, I cannot but regret that it should be left to others to first enter such a promising field. S. P. LANGLEY.

[To be continued.]

The Lunar Crater "Peirce A."

PEIRCE A is, it need barely be stated, the most northern and smallest of three craters situate in the western part of the Mare Crisium. During the half lunation that it is visible it undergoes some singular changes, which are recorded below. The following history is gathered from a series of 84 observations by various observers, either forwarded to the writer or published in the pages of the 'Astronomical Register.' Here is the analysis of the observations by the different observers:—Birt, 7 (1873 to 1877); Dennett, 60 (1877 to 1880); Gray, 6 (1879); Harding, 8 (1879–80); Richards, 2 (1879); and Sadler, 1 (1877). Besides this, one photograph by Rutherfurd has been examined. It is purposed first to give a brief monthly history of the crater and then say a few words respecting discordant observations.

As the Sun rises on Peirce A it is evidently a crater with walls of no very considerable external elevation, and of a somewhat dark character. As the Sun rises over Cassini, faint traces of a bright ring about the size of Picard are to be seen round about Peirce A, the southern wall of which touches the southern border of this ring. The northern wall is near the middle of the ring. North, a little east of Peirce A, near the edge of the ring there is a brightish spot visible. The interior of this ring becomes darker than the surrounding *mare*, but whether the darkening takes place before or after the appearance of the ring none of the observations made show. The shadow does not entirely leave the interior of Peirce A until the terminator has passed over Timocharis, by which time the ring has fully formed round the crater, whose western and southern walls have totally disappeared. The northern wall remains visible as a tiny bright spot just E. of the centre of the ring, and the eastern wall may be traced as a faint luminous ridge, looking more like a continuation of the Proclus light-streak VIII. (which extends from a spot close S.W. of Proclus to the S.E. wall of Peirce A) than any thing else. From this time for some 18 or 20 hours the part of the crater wall visible is very pale, and, with small instruments or an unsteady atmosphere, is very apt to be overlooked.

It is at this time very similar to the bright spot N. of it, but quickly brightens above it. Sometimes the darkest phase does not occur until the terminator has arrived nearly at Heraclides. But occasionally the crater pales again when the terminator passes Galileo, and remains dull until it reaches Riccioli. After this it is usually brighter until the terminator reaches Messala, when it becomes visible as a crater. As the Sun begins to set on Hahn's western wall the bright ring gets very pale, and entirely disappears as the terminator reaches the western part of the floor of the Mare Crisium. Peirce A remains a distinct shadow-throwing crater until it is lost in the darkness which gradually covers the *Mare*. When the dark border disappears none of the observations determine, but it is still visible as the terminator reaches Hansen's W. wall. But to render the history as comprehensive and lucid as possible the following table of phenomena is reduced to the mean age at mean libration:—

Moon's Age. d.	h.	m.	Phenomena.
3	1	49	Peirce A on morning terminator.
6	3	20	Bright ring appears round it.
7	16	30	Shadow finally leaves Peirce A, the S. and W. walls disappear, and the E. wall fades.
8	12	40	The N. wall usually brightens, though it sometimes remains dull until 9^d 7^h.
11	11	15	The N. wall frequently becomes dull again.
13	11	5	Again brightens.
16	2	25	The bright ring begins to fade.
16	11	40	The bright ring disappears.
17	3	35	Peirce A assumes the character of a crater*.
17	20	11	Passes over the evening terminator.

Some observers have occasionally missed the crater altogether, but from a careful consideration of the observations it seems probable that the apparent disappearance was entirely due to small apertures or unsteady atmosphere. Once Harding wrote that he could not see it, but he unfortunately omits the *time* of his observation. On consulting Dennett's observation book, from 9^h 30^m to 9^h 45^m on the same evening, 1879, Oct. 25^d, we find "Peirce A very small, but somewhat lucid. Two similar, but perhaps less lucid, spots in a line N. of the crater." Mention is made that the definition was rather fluttery. Harding employed a 3-in. Wray, Dennett a 5¼-in. Calver telescope, both of excellent quality. Again, 1879, Oct. 26^d 9^h, Harding writes that he can make it out "only by careful looking to-night..... After another careful look I am by no means sure that I can see Peirce A." On the same evening, 10^h 18^m to 11^h, Dennett writes, "Peirce A is brighter to-night, and, though small, is very distinct. It is decidedly brighter now than the objects N. of it." But there are a few other observations not so easily disposed of. 1879, May 29^d 10^h 30^m (Moon's age 8^d 16^h 40^m), Gray found Peirce A obscured by one of the Proclus light-streaks, and the dark aureole absent. Harding and Dennett (at 8^h 23^m and 9^h 25^m respectively) both observed the streaks on this evening, but neither mention Peirce A, though the latter would have mentioned it had he missed it; on the other hand he records "VIII. as usual," *i. e.* running up to Peirce A. 1879, June 28^d 10^h (Moon's age 9^d 1^h 50^m), Mr. Gray again saw the crater obscured by a curve in a light-streak. An hour previously Dennett observed the neighbourhood, but made no note respecting Peirce A, and says the light-streak was as usual. Again, 1879, March 8^d 9^h 40^m (Moon's age 15^d 17^h 37^m), Rev. W. J. B. Richards says "Peirce A is quite gone. A ray from Proclus passes between it and Peirce. The ray seems brighter and denser on the S. side, and Peirce A is just caught and hidden by the lighter northern side of it." 50 minutes later Dennett observed the district, but makes no mention of missing the crater. These obscurations seem very singular, and it is necessary to have, if possible, confirmatory observations. Until these are made some doubt must hang over the observations as to whether the N. wall of Peirce A was not mistaken for the bright spot lying N. of it. But the ground within the luminous ring appears to be not always especially dark. Gray missed it 1879, May 29^d 10^h 30^m. Dennett found it not darker than the *Mare*, 1880, Jan. 21^d 10^h 25^m (Moon's age 9^d 23^h 45^m). The ring round the crater is likewise subject to change, all sides not being always equally visible, according to the observations of Gray and Dennett. What is this ring? It does not appear to be a bank, we think.

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* This phase is apparently given too late by about 1^d, according to a later observation by Dennett.

F. C. DENNETT.

Double Stars for August.

THE most important pairs have already been given, see 'Observatory,' No. 28, Vol. iii. p. 114.

The places for 1880 of the stars of which some account is given below are as follows:—

	R. A.		Dec.	
	h.	m.	°	′
ϵ^1 Lyræ......	18	40·4	+39	33
α^2 Capricorni..	20	11·4	−12	54·7

The group of ϵ^1 Lyræ.

As this interesting group of stars is now well placed for observation, and as it has long been a favourite test-group for the small telescopes of amateur astronomers, the following historical summary will probably be read with interest by many. This system (?)

may be described as consisting of the two binaries ϵ^1, ϵ^2 (the two constituting the ϵ Lyræ of Bayer), the famous *debilissima* pair, at least two stars following, and at least two pairs preceding that delicate object.

As is well known, ϵ^1 and ϵ^2, as double stars, were discovered by by H_1. He thus described the pairs in 1779:—" 5 and 6, ϵ Lyra, Fl. 4 and 5. A very curious double star. At first sight it appears double at some considerable distance; and by attending a little, we see that each of the stars is a very delicate double star." Since H_1's observations, ϵ^1 has decreased about 16° in angle, and ϵ^2 about 30°.

The 9½-magnitude star following, and the delicate pair (called the *debilissima* by H_2) between ϵ^1 and ϵ^2, were also discovered by H_1. In his MSS.[*] Sir William has the following note, accompanied by a diagram showing the above-named "double-double," the 9½-magnitude star, and the *debilissima* pair:—" Nov. 29, 1782. Inter ϵ and ϵ Lyræ (Mayer's e) the 20 ft. [reflector, aperture about 18 inches] shows me two small stars I have never before taken notice of with the 7 ft. [reflector, aperture 6 inches]. That which is below [the 9th mag. star following] I have often noticed." H_1 appears not to have seen any of the other minute stars in this group preceding the faint pair.

In 1823 H_2 and So examined this region, apparently without being aware that H_1 had seen three stars between ϵ^1 and ϵ^2. They used the 20 ft., 10 ft., 7 ft. reflectors (apertures 18 in., 9 in., 6 in. respectively), and the 5 in. and 3·8 in. refractors. All the smaller instruments failed to show the faint pair; but the 20 ft. enabled the observers to see " a double star, whose distance is one fourth that of ϵ from 5 (*i. e.* 53″), in the middle between them." All the instruments showed " a small star of about the 10th magnitude preceding[†] them both, and making an isosceles triangle of about 100° at the vertex with ϵ and 5" (Phil. Trans. 1824, pt. 2, p. 313). This pair of small stars H_2 calls "*Debilissima* inter 4 (ϵ) et 5 Lyræ."

Dawes observed these stars in 1829, 1830, 1833, and later. His diagram in the 'Astronomical Register' for 1864 shows ϵ^1, ϵ^2, the *debilissima* pair, a star following, and two pairs preceding.

Lamont[‡], in 1837, made measures of the position-angles of the stars he saw in this group; his diagram shows ϵ^1, ϵ^2, the *debilissima* pair, the pair of very faint stars just preceding it, another faint star still more west, and the conspicuous 9½ magnitude to the east of the line joining ϵ^1 and ϵ^2.

More recently Webb, Cooper, Key, Grover, Prince, Holden, Godfrey, Squire Sadler, and others have seen all the above objects, and probably some other very faint stars to the west of the line through ϵ^1 and ϵ^2 (see 'Astronomical Register' for 1864, 1871, 1872, &c.).

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[*] Mr. Sadler pointed out this note to me.

[†] This should be *following*, not *preceding*.

[‡] 'Observationes astronomicæ in speculâ Regiâ Monachiensi,' &c., 1843.

Lassell in 1856 detected a very small star about 25″ north of G. Dawes saw one preceding this last object, and Ward and Sadler in 1876 detected a faint star following F. The order of brightness is probably as follows:—E; F; G; Dawes's star; Lamont's most western star; Lamont's two other stars; Ward and Sadler's star; Lassell's star.

Magnitudes.—Representing ϵ^1 by the letters A B, ϵ^2 by C D, the *debilissima* by F G (F being the preceding star), the 9½ magnitude star to the east by E, and Burnham's by H, the magnitudes run as follows:—A 4·6, B 6·3, C 4·9, D 5·2, E 9·5, F* 10·5, G* 10·5.

Variability.—Struve suspected C D (ϵ^2) of variability ('Mens. Micrometricæ,' p. 52). Several amateur observers have thought they had traced decided change of relative brightness in F and G; neither Mr. Sadler's observations nor those made here, however, confirm this opinion.

The system (?) of α^1, α^2 Capricorni.

Although it cannot yet perhaps be said that these two stars are animated by a common proper motion, the facts that each has minute companions, that some of these faint objects form very fine tests for telescopes of large aperture, and that α^1, α^2 constitute a double star to the naked eye, combine to make them a very interesting object. Flammarion has examined several eastern and other ancient catalogues, but without finding any record of this object as a double star. It appears as such for the first time in the Catalogue of Hevelius.

α^1.

If we call the minute stars a^1 and b^1, we have

α^1 a^1	225°	30″	1827±	H_2.
α^1 b^1	200	30±	1878	Bu.

b^1 was detected by Mr. Burnham.

For the distance and angle of the principal stars we have the following:—

α^1 α^2	290°·6	376″·3	1876	Schur.

Magnitudes: α^1 4, a^1 9·5. Colours: α^1 yellow, a^1 lilac tinge (Smyth).

α^2.

Calling the small stars a^2, b^2, c, we have

α^2 a^2	150°·2	7″·41	1878	Bu.

Other measures are:—

150°±	8″	1826±	H_2.
134 ·8	6	1829±	,,
141 ·2	6	1831±	,,

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* Flammarion, 12, 12; H_2 and So, 13, 13.

145°·0	5″	1838	Sm.
144 ·1	6 ·36	1846	Mitchel.
146 ·2	..	1874	Holden.

In 1862 the small companion a^2 was found to be a double star by Mr. A. G. Clark with the 18½-in. refractor now at the Dearborn Observatory. Prof. Young in Colorado, at an elevation of 8000 feet, could see it double with a 9·4-in. refractor. Mr. Burnham gives the following measures:—

$a^2\,b^2$	57°·6	1″·72	1874·6	Holden.
	58 ·6	1 ·24	4·6	Newcomb.
	65 ·2	1 ·14	5·7	Hall.
	63 ·2	..	6·7	”
	61 ·2	1 ·06	8·5	Bu.

Smyth records a distant small star, c:—

$a^2\,c$	155°·7	198″·0	1838·7	Sm.

Magnitudes.—For a^1 and a^1 Smyth gives 4, 9. Mr. Burnham's b^1 is no doubt a very faint object. For a^2, a^2 Smyth has 3, 16. Sir John Herschel, who discovered a^2, has these remarks on it:—"The small star is brighter than the 18th mag.;" "a very beautiful and delicate object;" "the latter [a^2, a^2] is one of the most beautiful and delicate objects in the heavens." And Mr. Burnham says, "Under favourable conditions a 6-inch refractor will show it fairly."

JOSEPH GLEDHILL.

Mr. Edward Crossley's Observatory,
Bermerside, Halifax.

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*Meteor Notes for August**.

WITH favourable weather the ensuing return of the Perseids may be well observed, for the Moon, being only a few days past conjunction, will be invisible throughout the night. It is essential to record the horary number of meteors on the 10th, so that the visible intensity of the shower may be compared with its former apparitions. We have already accumulated a large number of determinations of the radiant-point; but they are not all satisfactory, so that its exact position cannot be said to have been definitely ascertained within very small limits of error. Some observers ascribe a diffuse radiant region, extending from χ Persei to the stars B–C Camelopardi [Bode], while others have centred the shower further south near γ and α Persei; and it is difficult to explain the differences unless we assume that the shower consists of a group of concentric streams intersecting the earth at nearly the same points. In 1879 the radiants, as independently fixed by several observers, agreed with unusual precision *, and would appear to show that, when resting upon a number of foreshortened paths, appearing within a few degrees of the centre, it is capable of being found with considerable accuracy.

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* See 'Observatory,' Vol. i. p. 125, Vol. ii. pp. 131, 132, and Vol. iii. pp. 116, 117, for further notes on August showers.

Meteors from Perseus are also numerous on the nights of Aug. 1–3. They belong to a well-defined shower, slightly below χ Persei (at 32° +53°). During the first half of the month, radiants should be looked for at 31° +18°, 44° +25°, 70° +65°, and on the 21st–25th a shower of Draconids (298° +60°) was well observed in 1879. But it is impossible to enumerate more than a small minority of the numerous showers of the August period proceeding from the constellations of Pegasus, Lacerta, Cassiopeia, Andromeda, Perseus, Auriga, &c. &c.

Large meteors should be expected on the 3rd, 5th, 7th–13th, 15th, and 19th–22nd.

Recent Observations.—The Rev. S. J. Johnson, of Mitcheldean, Gloucestershire, writes that a meteor of great brilliancy was observed there at $11^h\ 51\frac{3}{4}^m$ on July 1. "It was of a red colour, surpassing any planet at maximum. Course from a point one third of the distance from λ Boötis to Cor Caroli; it shot towards the latter star. Another meteor was seen on June 12 at $11^h\ 43^m$ = ♃, travelling in a straight line between γ Draconis and δ Cygni. Began 6° from the former star, ending 3° from the latter."

A large fireball, emitting a light equal to that of the Moon when nearly full, was seen by Prof. E. W. Claypole, of Antioch College, Ohio, U.S., on June 10, $9^h\ 10^m$. "It followed a path almost directly from η Ursæ Majoris (Benetnasch) between γ and χ, about midway, and disappeared behind a cloud on its way to the W.N.W. point of the horizon. It was visible about two seconds, and left no sparks behind." The radiant-point cannot be definitely fixed from this isolated description, though it evidently lies between Hercules and Sagittarius. This fine meteor may have belonged to a shower near η Serpentis, which the writer deduced from the Italian observations on June 10–13, 1872, at 273° −3°. It presents a close agreement with Comet III. 1618, having a radiant at 273°·5 +0°·5, June 10, with which Schmidt's showers at 282° −3° and 266° −12° for June may also be associated.

A fireball, estimated = half moon, was seen in daylight on July 9 at $8^h\ 20^m \pm$ P.M., by the Rev. Lloyd Jones, 1 mile E. of Greenwich Observatory. Colour very bright light blue; it had a long streaming tail or train of ruddy sparks. Motion slow and stately; duration quite 5 seconds, but may have been 10 seconds. Path from left to right inclined 10° to horizon. The point of disappearance, referred

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* The observations here referred to were as follows:—

Tupman	45°	+56°	Corder	45°	+57°
Sawyer	44½	+57	Cornish	47¼	+57½
Denning	46°	+58°,			

giving a mean position at 45°·5 +57°·1 for the shower of 1879.

to trees some 200 yards distant, was afterwards found to be due east at altitude $9° \pm 1°$. The same evening, at about $9^h\ 15^m$, a "most remarkable meteor" was observed by Mr. B. Dowson, of Upper Broughton, Notts. Its track was first noticed 25° above the horizon, and as nearly as possible N.E., or from that to E.N.E. The course was southerly, and tended slowly downwards, the meteor finally disappearing at a point due east, and 10° above horizon. The stars were scarcely visible to the naked eye at this time; but the spot where the track was first observed was noted to be nearly on a line drawn from α Lyræ to α Cygni. The peculiar characteristics of the display were the slowness of motion and the length of time the meteor remained visible. "As it passed through the air it underwent several changes, and twice seemed to hesitate, and on each occasion something like an explosion or discharge of fiery matter appeared to take place. I estimate that it remained visible for at least 15 seconds." These descriptions must refer to separate objects, as there is nearly an hour's difference of time; but, curiously enough, the points of disappearance are absolutely identical, and the direction and duration of flight agree very closely.

Later the same night, at $9^h\ 46^m$ G.M.T., a bright meteor, = Arcturus, was noted by Mr. Christie at Greenwich, with a path from $195° + 3°$ to $208° - 7°$ (from near δ Virginis nearly to κ Virginis). Slow; duration 2 or 3 seconds. This meteor was observed by Mr. C. A. Bowdler, Lambeth Road, London, and described by him in the 'English Mechanic' (July 16, p. 447). "On July 9, at a quarter to 10, I observed a very beautiful meteor pass slowly from west to east in a horizontal direction for about 12° or 15°, crossing the equator at 200° R.A. near the star δ Virginis. It had a bright nucleus, and long streaky tail, tapering to the end, and lasted four or five seconds." The meteor was also seen at Great Yarmouth and March (Cambs.).

W. F. DENNING.

CORRESPONDENCE.

To the Editor of 'The Observatory.'

On a Phenomenon shown in a Photograph of the Spectrum of Jupiter.

SIR,—

At a late Meeting of the R.A.S. I made some remarks regarding Professor Draper's photograph of the spectrum of Jupiter which were not quite explicit enough, it seems to me, in reading them over. I should like to point out how the phenomenon (viz., intensity of image in the middle of the spectrum about G, and a bifurcation in the violet and ultra-violet) can be readily accounted for.

If an image of the Sun be thrown on the top part of the slit, so that the limb is a little below the centre, and a photograph be taken, it will be found that instead of having a sharp edge corresponding to the limb, the spectrum will gradually narrow towards the ultra-violet, the top of the slit, however, giving a defined boundary. If the image of the Sun during exposure be caused to move downwards, so that the opposite limb is a little above the centre, the photograph will show precisely the phenomenon that Professor Draper's Jupiter-spectrum shows. The reason of this thinning off in the solar-spectrum photograph I will not explain, as it is evidently due to the difference in absorption of the limb and the centre; and the same, we may reasonably believe, holds good when the image of Jupiter takes the place of that of the Sun.

I am Sir, yours faithfully,
W. DE W. ABNEY.

Southern Double Stars.

SIR,—

Among the double stars observed by Sir J. Herschel at the Cape of Good Hope, there are some on which he makes special remarks at the end of his Catalogue. Of four of these particular stars I have recently made the following measures of position and distance by means of an excellent position-micrometer on my 4½-inch equatoreal. In each case the brighter star was employed as the reference star; and the angle of position is to be understood as reckoned from north in the order *north, following, south, preceding.* In the case of *p* Eridani it was difficult to decide which was the brighter star; but I have given the eastern or *following* one the preference.

Star.	Mags.	Epoch.	Pos.-Angle.	Dist.	No. of Measures.
			°	″	
λ Tucanæ	7, 8	1880·205	79·1	20·50	4
p Eridani	6, 6	1880·186	231·9	6·51	10
		1880·189	233·3	6·23	7
υ Argûs	3, 8	1880·293	125·4	4·94	5
α Centauri............	1, 3	1880·186	186·7	4·97	8
		1880·351	186·4	5·31	10

It appears that my measures of λ Tucanæ and *υ* Argûs agree pretty well with those of Sir J. Herschel; both the angle of position and the distance of *p* Eridani have, however, greatly changed since his measures were taken. 'Nature,' of December 28th, 1876, draws attention to this star, and recommends its observation by some southern astronomer. If no other recent observation has been recorded, the result above given will be interesting. The star was employed by me in comparisons of the great southern comet I. 1865, on March 16, 17, and 18 of that year. In conclusion, it will be seen that the companion of *a* Centauri has now passed the meridian of the principal star, in accordance with the prediction of Dr. Doberck in No. 2245 of the 'Astronomische Nachrichten.'

I am, Sir, yours faithfully,
JOHN TEBBUTT.

Observatory,
Windsor, N. S. Wales,
1880, May 11.

NOTES.

ANNALS OF HARVARD COLLEGE OBSERVATORY.—In No. 36, p. 387, we noticed the Photometric Observations contained in Vol. xi. part 1. These consisted for the most part of measures of the light of stars; in Part ii., to which we now turn, the determinations of the brightness of the various satellites are given. In these researches Prof. Pickering had great difficulties to overcome on account of the extreme faintness of some of the objects whose light was to be measured; the results, however, are of special interest, as affording some idea of the real size of these minute members of the solar system. Prof. Pickering's attention was first directed to the newly discovered satellites of Mars, and he divised various new forms of photometer for the examination of these faint objects. These photometers are all described in Part i., and the first two chapters of Part ii. (Chap. v. and vi.) are devoted to determinations of their constants, determinations made with great care and by various methods. The results obtained for the satellites of Mars in 1877 and in 1879 are given in Chap. vii. and in an appendix respectively. The problem was one of great difficulty, involving the measurement of a photometric interval as great as that between the Sun and the Moon, the planet Mars being several hundred thousand times as bright as his satellites. As a direct comparison was impossible, the light of the satellite was compared with that received from a small portion of Mars through a small hole 1-100th of a millimetre in diameter, *i. e.* 1-100th of the diameter of Mars, or about 40 miles, the diameter of Mars in the telescope being one millimetre. Much difficulty was experienced in making such a hole, and it was only after various attempts, described in Appendix B, that Prof. Pickering succeeded in boring suitable holes in a piece of platinum foil gummed on a glass plate.

The light of Mars seen through the smallest hole that could be made was, after all, much greater than that of the satellite, which must therefore, if its albedo or reflecting power is about the same as that of Mars, have a diameter much less than forty miles.

The following are the results of comparisons with Mars, as well as with Saturn and certain faint stars near which the satellites passed:—

Mars = 667,000 × Phobos in 1877; Equiv. diam. = 5·57 miles.
Mars = 879,000 × Deimos in 1877; „ „ = 4·9 ± 0·1 miles.
Mars = 550,000 × Deimos in 1879; „ „ = 6·1 ± 0·1 miles.
Mars = 461,000 × Deimos foll. Mars; Equiv. diam. = 6·7 miles.
Mars = 673,000 × Deimos prec. Mars; Equiv. diam. = 5·5 miles.

The equivalent diameter of the satellite is calculated on the assumption that its albedo is the same as that of Mars.

It appears, then, from Prof. Pickering's results that Deimos was apparently much brighter in 1879 then in 1877, and that it was brighter by nearly half a magnitude on the following side than on the preceding side of its orbit. In support of this, Prof. Pickering points out that there is a large preponderance of micrometer observations of Deimos on the following side, showing that the satellite was more easily seen in that part of its orbit. The micrometer measures of Phobos were nearly equally distributed *p* and *f*. No photometric measures were made of this satellite in 1879. The general result of all the observations is that Deimos is about 14·5 magnitudes (on the photometric scale) fainter than Mars, and about 0·3 magnitude fainter than Phobos, the equivalent diameters of the two satellites being about 7 and 6 miles respectively, corresponding to 0″·010 and 0″·009 at mean distance. That is, the satellites are fainter than an area of the surface of Mars having a diameter of 0″·01 at distance unity, or 1-1000th part of the diameter of Mars. Notwithstanding the minuteness of the points of light compared, a difference in colour was detected between the outer satellite and Mars, Deimos appearing to be blue, whilst the artificial star formed by Mars was noted as red.

It is worthy of remark that Prof. Pickering found a certain amount of light in the field to be not prejudicial to the visibility of the satellites; and he remarks that it is possible that as a certain amount of light is required to affect the retina, the light from the field added to that of the satellite may prove sufficient to render the latter visible. The effect would be analogous to the increased sensitiveness of a photographic plate after exposure to a feeble source of light.

The observations of the satellites of Jupiter in Chapter viii. are of special interest, from the comparison of equivalent diameters (photometrically inferred) and the diameters as actually measured. The following are the results:—

Satellite	I.	II.	III.	IV.
Light ratio..........	1786	2032	1271	3597
Eqivalent diameter ..	0″·873	0″·818	1″·035	0″·615
Measured diameter ..	1″·081	0″·910	1″·537	1″·282
Relative albedo	0·652	0·809	0·453	0·230

The first line gives the ratio of the light of Jupiter to that of each of the satellites, the last the albedo relative to Jupiter. To obtain the absolute albedo for each satellite, the relative albedo must be multiplied by the albedo of Jupiter (0·610). The view that the light of the satellites is variable is not confirmed by Prof. Pickering's observations.

A large number of photometric measures of the eight satellites of Saturn were made in 1877–8, special attention being devoted to the variations of light of Iapetus, which were found to extend from 40 to 140 (taking the mean brightness as 100). The following formula was found to represent the variations of light:—

$$L = 100 - 50 \sin v + 10 \cos 2v,$$

where L is the light of the satellite at any longitude v.

On this formula Prof. Pickering remarks, "The absence of the term $c \cos v$ shows that if the variation in light is due to unequal brightness of the two hemispheres of Iapetus, one half of each will always be turned towards Saturn. In other words, it would present to an observer at the north pole of Saturn the appearance of a half moon, the semicircle to the right being about four times as bright as that to the left.

"Some doubt is thrown on the term $e \cos 2v$, since the comparisons with Saturn do not confirm its presence. Since the value of e is positive, it cannot be supposed to indicate that Iapetus is elongated in the direction of Saturn, although a slight elongation of this kind is probably implied in the assumption that the satellite turns once on its axis during each of its revolutions. An elongation sufficient to produce this effect might be caused by the attraction of Saturn, but it would be far too small to be perceptible photometrically. To make $e = +10$, the equatoreal diameter of the satellite turned towards Saturn should be exceeded by that at right angles to it in the ratio of 9 to 11; so great a difference does not seem probable. The term $e \cos 2v$ could also be accounted for by two bright or dark spots on the satellite. A dark spot on one side covering less than a hemisphere would also give a variation in light closely resembling that given by the formula. The most natural explanation, however, is that the dark and light portions are irregularly distributed on Iapetus, like the land and water on our Earth, and that one hemisphere is, on the whole, much darker than the other. The smaller variations may be assumed to be such that the formula given above represents them closely."

The following are the equivalent diameters of the satellites in miles found by Prof. Pickering:—Mimas 292 ± 9; Enceladus 370 ± 10; Tethys 570 ± 18; Dione 542 ± 17; Rhea 745 ± 27; Titan 1406 ± 52; Hyperion 193 ± 5; Iapetus, mean 486 ± 4, max. 574, min. 307.

Since the equivalent diameters are measures of light and not of actual size, two additional numbers have been given corresponding to the maximum and minimum brightness of Iapetus.

The satellites of Uranus and Neptune have the following equivalent diameters (in miles):—Titania 586 ± 16; Oberon 544 ± 15; Satellite of Neptune 2260 ± 60. The large equivalent diameter of the satellite of Neptune is noteworthy, and Prof. Pickering remarks that the principal causes of its apparent faintness are presumably its comparative proximity to its primary and its great distance from the Sun. No observations were obtained of the two inner satellites of Uranus, owing to their extreme faintness.

Determinations are also given of the equivalent diameters of some of the minor planets, as follows:—Pallas 167 miles; Juno 94 ±4; Vesta 319±10; Antiope 51±3; Brunhild 20±3; Eva 14 ±1; Menippe 12±1.

Though these equivalent diameters must not be accepted as actual diameters, they doubtless give a pretty close approximation to the size of the smaller members of the solar system, even allowing for a large uncertainty in the albedo. It would seem from the Harvard College determinations that some of the smaller asteroids are at any rate comparable in size with the satellites of Mars, and the step from these to the larger meteorites is not a very large one.

The volume also contains photometric measures of 103 unequal double stars and some miscellaneous results.

Bearing in mind the great care that has been taken to avoid systematic errors by varying the methods of observation, the importance of this contribution to Celestial Photometry can hardly be overestimated, and we heartily congratulate Prof. Pickering on the outcome of his energetic direction of the Harvard College Observatory.

VARIABILITY OF A MINOR PLANET*.—The minor planet Frigga, No. 77, was discovered by Prof. C. H. F. Peters in 1862, and after being observed at the next opposition in 1864, was lost till Prof. Tietjen picked it up again in 1868. Since that time it was sought in vain at each succeeding opposition, till that of last year, when it was found in 1879, July 16, not very far from its calculated place. The circumstance that for so many years a careful search was made for the planet without its being detected, leads Prof. Peters to suspect it of variability; and this suspicion is confirmed by his estimates of its magnitude last year throughout the period of observation (July 17 to Oct. 4). After applying corrections to the brightness for reduction to the distance at mean opposition and for atmospheric absorption as depending on the zenith-distance, it appears that the planet was a whole magnitude brighter before opposition than after, having diminished continuously from July to October.

From last year's observations the mean opposition magnitude would be 10·89; and as its theoretical brightness would have been 10·1 and 10·6 in the oppositions of 1871 and 1875, it could hardly have escaped Prof. Peters's careful search with the help of a chart showing all stars down to the 13th magnitude, unless the planet had greatly diminished in brightness. It is too early to speculate as to the causes of such a change of brightness, for the fact has first to be established beyond doubt; but it is certain, Prof. Peters

* Astronomische Nachrichten, No. 2314.

observes, that the length of the period of the fluctuation does not allow of our connecting it with rotation of the planet.

RELATION BETWEEN COLOURS AND MAGNITUDES OF BINARY STARS [*].—Prof. Holden has tabulated the magnitudes and colours of 162 stars which are certainly binary according to a list furnished by Mr. Burnham, and finds that of these the components of 122 binary stars of the same colour differ in magnitude on the average only $0^{m}\cdot5$, whilst those of 40 of different colours differ $2^{m}\cdot4$. The colours and magnitudes are for the most part taken from M. Struve's estimations. Prof. Holden quotes a conclusion by Dr. Huggins and Prof. Miller that the characteristic colours of stars are due to the absorptive action of their atmospheres, and compares it with the two following facts:—(1) The colour of a solid body cooling would as it cooled pass through the shades *white*, *yellow*, *orange*, *red*, but not through *green*, *blue*, or *purple*. (2) We do not find *isolated* stars of decided green, blue, or purple colours. A few such have been recorded, but in most cases erroneously[†]. In general such stars are small, and, as far as I know, invariably associated with larger stars. That is, the isolated stars appear always of the colours which would arise in the cooling of solid or liquid masses, and never as if necessarily surrounded with absorptive atmospheres—*i. e.* never decided violet or purple.

It is to be noted, however, that we require more certain data as to the colours of stars before Prof. Holden's facts can be accepted as indisputable. Eye estimations are notoriously untrustworthy.

PROGRESS OF ASTRONOMY IN 1879[‡].—Mr. J. L. E. Dreyer has compiled a valuable record of the Progress of Astronomy in 1879, in which he sketches the principal astronomical events of the year, giving short accounts of the more important or interesting investigations which have been published during that period. This record is in continuation of that which was written for the year 1878 by Prof. E. S. Holden, for the 'Annual Record of Science and Industry,' a publication which has been discontinued. In future Nos. we propose to give some extracts from Mr. Dreyer's useful publication.

THE COMPANION OF SIRIUS [§].—Prof. Asaph Hall calls attention to the investigations of the variable proper motion of Sirius by Bessel, Peters, Safford, and Auwers, in connection with the companion discovered by Alvan Clark, and points out that though the result investigated by four able astronomers is entitled to great weight, there are some things which throw a slight degree of doubt on the identity of the Clark companion with Bessel's disturbing body. First, the relatively great mass of the companion, which, according to Auwers, is one half the mass of Sirius or seven times the mass of the Sun. Unless, therefore, we assume a special constitution for this companion, it ought to shine with the brilliancy of a star of the first magnitude, while in fact it is only of about the ninth magnitude. Again, the micrometer observations, which extend ever 18 years, or more than one third of the period of the disturbing body, indicate that the periodic time will prove to be decidedly greater than what the disturbed motion of Sirius calls for; and Prof. Hall urges astronomers to make careful observations of this interesting object for a few more years, so as to decide the question of identity.

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[*] Amer. Journ. 1880, June.

[†] For example Admiral Smyth calls *a* Lyræ a "green" star.—E. S. H.

[‡] Proc. Roy. Dublin Soc. 1880.

[§] Amer. Journ. 1880, June.

Red Spot on Jupiter.

Dr. Lohse* has lately observed three transits of the middle of the Red Spot across the meridian, bisecting the illuminated disk of Jupiter as follows:—1880, June 20, $15^h\ 26^m{\cdot}1$; July 2, $15^h\ 16^m{\cdot}9$; July 9, $16^h\ 3^m{\cdot}4$, Berlin M.T. (Berlin is $53^m\ 34^s{\cdot}9$ E. of Greenwich). The Red Spot appears to have scarcely changed since last opposition; and the following ephemeris, based on Dr. Lohse's observations, and on Mr. H. Pratt's period, $9^h\ 55^m\ 33^s{\cdot}9 = 9^h{\cdot}9261$ (daily rotation $870^\circ{\cdot}43$), will indicate the transits which can be conveniently observed in Europe:—

G.M.T. of Transits of Red Spot across Central Meridian.

	h		h		h
Aug. 2....	14·9	Aug. 10....	11·5	Aug. 21....	15·4
3....	10·7	12....	13·1	22....	11·3
4....	16·5	14....	14·7	24....	12·9
5....	12·4	15....	10·6	26....	14·5
7....	14·0	17....	12·2	27....	10·4
8....	9·8	19....	13·8	28....	16·2
9....	15·6	20....	9·7	29....	12·0
				31....	13·6

It may be convenient to note that transits on any given day take place $4^h{\cdot}15$ earlier and $5^h{\cdot}78$ later than a transit on the preceding day. Hence a transit convenient for observation in the United States will occur on the day following any one of those given in the table for Greenwich time, and $0^h{\cdot}64$ earlier by Washington time. Thus there will be a transit on Aug. 3 at $14^h{\cdot}3$ Washington M.T., on Aug. 4 at $10^h{\cdot}1$, on Aug. 5 at $15^h{\cdot}9$, &c. It may also be noted that transits recur at intervals of 5 and 7 days at nearly the same hours ($0^h{\cdot}90$ earlier and $0^h{\cdot}73$ later respectively).

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* Astron. Nachr. No. 2325.

Astronomical Memoranda, 1880, *August.*

Sun. August 1, sets $7^h\,46^m$, rises $16^h\,27^m$; August 16, sets $7^h\,19^m$, rises $16^h\,51^m$; August 31, sets $6^h\,47^m$, rises $17^h\,15^m$.

Equation of time:—Sun *after* Clock, August 1, $6^m\,3^s$; August 11, $4^m\,55^s$; August 21, $2^m\,50^s$; August 31, $0^m\,1^s$.

Sidereal Time at Mean Noon:—August 1, $8^h\,41^m\cdot9$; August 11, $9^h\,21^m\cdot3$; August 21, $10^h\,0^m\cdot7$; August 31, $10^h\,40^m\cdot2$.

Moon.	*rises.*			*sets.*			*rises.*	
	h	m		h	m		h	m
Aug. 1..	12	35	Aug. 11..	8	51	Aug. 21..	7	13
2..	13	33	12..	9	19	22..	7	31
3..	14	38	13..	9	54	23..	7	50
4..	15	46	14..	10	41	24..	8	10
5..			15..	11	43	25..	8	35
6..			16..	12	57	26..	9	5
7..	*sets.*		17..	14	22	27..	9	43
8..	7	56	18..	15	47	28..	10	29
9..	8	12	19..	*rises.*		29..	11	24
10..	8	32	20..	6	55	30..	12	25
						31..	13	33

New Moon, August 5, $15^h\,48^m$; First Quarter, August 13, $0^h\,42^m$; Full, August 19, $17^h\,18^m$; Last Quarter, August 27, $4^h\,15^m$.

Mercury is in inferior conjunction on August 5, after which he is a morning star, at greatest elongation (18° 21′ W.) on August 23, when he rises in the E.N.E. $1^h\,45^m$ before the Sun.

Venus and *Mars* are both evening stars, setting soon after the Sun.

Jupiter is stationary in Pisces. Diameter:—August 1, 41″·1; August 31, 44″·7.

	R.A.	Dec.	tr.	rises
Aug. 1,	$1^h\,14^m\cdot1$	6° 17′ N.,	$16^h\,30^m$	$9^h\,54^m$
16,	1 14 ·1	6 13 N.,	15 31	8 56
31,	1 11 ·4	5 53 N.,	14 29	7 55

Daily rate of rotation 870°·70 = 2^r 150°·70; Hourly rate 36°·28; Period $9^h\cdot923$. (Derived from Mr. Marth's Ephemeris for Physical Observations of Jupiter, 'Monthly Notices,' May 1880.)

Saturn is also stationary, about one third of the way from α Piscium to β Arietis.

	R.A.	Dec.	tr.	rises
Aug. 1,	$1^h\,51^m\cdot1$	8° 42′ N.,	$17^h\,6^m$	$10^h\,18^m$
16,	1 51 ·3	8 39 N.,	16 8	9 20
31,	1 50 ·1	8 28 N.,	15 7	8 21

	Outer Ring.		Inner Ring.		Ball.
	Maj. Axis.	Min. Axis.	Maj. Axis.	Min. Axis.	Diam.
Aug. 2 ..	41″·30	11″·15	27″·47	7″·41	16″·6
22 ..	42 ·75	11 ·46	28 ·43	7 ·62	17 ·2

The south side of the rings is visible, the elevation of the Earth above their plane being 15° 40′ S. on August 2, and 15° 33′ S. on August 22, and of the Sun 13° 23′ S. and 13° 39′ S.

Phenomena.

	G. M. T.		
	h	m	
Aug. 1	13	24	J. i Tr. I.
2	10	49	J. iii Tr. I.
	12	45	J. i Oc. R.
	13	6	J. iii Tr. E.
6	11	33	J. ii Ec. D.
8	10	52	J. ii Tr. E.
	15	13	J. i Tr. I.
9	11	9	J. i Ec. D.
	12	7	J. iii Sh. E.
	14	34	J. iii Tr. I.
	14	34	J. i Oc. R.
10	11	54	J. i Tr. E.
13	14	8	J. ii Ec. D.
15	10	40	J. ii Tr. I.
	13	17	J. ii Tr. E.
16	6	22	26 Sagitt. Oc. D. 103°.
	13	3	J. i Ec. D.
	13	21	J. iii Sh. I.
17	11	29	J. i Tr. I.
	13	42	J. i Tr. E.
18	10	50	J. i Oc. R.
20	9	58	J. iii Oc. R.
21	7	43	[8152] Oc. R. 318°.
	9	22	κ Pisc. Oc.* 208°.
Aug. 21	9	50	9 Pisc. Oc. R. 241°.
	15	30	16 Pisc. Oc. R. 339°.
22	13	4	J. ii Tr. I.
	15	24	45 Pisc. Oc. D. 303°.
	15	41	J. ii Tr. E.
	18		*Mercury* Elong. 18° 21′ W.
23	14	58	J. i Ec. D.
24	10	39	J. ii Oc. R.
	13	17	J. i Tr. I.
	15	29	J. i Tr. E.
25	9	26	J. i Ec. D.
	12	37	J. i Oc. R.
	15	53	47 Ariet. Oc.* 201°.
26	9	55	J. i Tr. E.
27	9	47	J. iii Ec. R.
	11	20	J. iii Oc. D.
	13	29	J. iii Oc. R.
28	16	38	121 Tauri Oc. R. 244°.
29	13	46	μ Gem. Oc.* 0°.
	15	26	J. ii. Tr. I.
31	12	58	J. ii. Oc. R.
	15	3	J. i Tr. I.

* A near approach.

The angles at Oc., D., and R. are reckoned from the *apparent* N. point towards the right of the Moon's inverted image.

Publications received:—Prof. Holden, *A Subject Index to the Publications of the U.S. Naval Observatory*, 1845–1875—Prof. Holden, *Catalogue of Library of U.S. Naval Observatory, Washington*, Part i. *Astronomical Bibliography—Greenwich Spectroscopic and Photographic Results*, 1878 *and* 1879—J. L. E. Dreyer, *Record of the Progress of Astronomy during the Year* 1879 (Proc. Roy. Dublin Soc. 1880)—Prof. Asaph Hall, *On the Observations of Double Stars* (Proc. Amer. Assoc. 1879); *Note on the Companion of Sirius* (Amer. Journ. 1880, June)—Dr. B. A. Gould, *Sobre el Cometa de Febrero de* 1880 (Buenos Aires, 1880)—Prof. E. Loomis, *Contributions to Meteorology* (Amer. Journ. 1880, July)—*Annual Report of the Board of Directors of Chicago Astronomical Society, and Report of Director of Dearborn Observatory*, 1880—Prof. C. A. Young, *Method of Investigating Errors in form of Pivots of Meridian Instrument* (Proc. Amer. Assoc. 1879); *The Colour Correction of Certain Achromatic Object-glasses* (Amer. Journ. 1880, June).

EDITOR.

THE OBSERVATORY,

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Practical Spectroscopy in 1880.—Part II.

STEPPING-STONES THEREIN.

THE idea developed in our last communication,—as to the propriety of all Anglo-Saxon spectroscopic observations being expressed in one numerical language alone, and that the most suitable for the masses of the people, whenever observations are either published, or even when only communicated in MSS. from one private friend to another,—need not throw out of use a single good prismatic instrument which is presently at work anywhere.

The measuring scales attached to them at present may, indeed, give forth any thing but the desired ultimate wave-numbers, and some of them may read by the agency of optical images in the field of view, others by verniers or even microscope-micrometers on a divided circle rim, or by endless screw on a toothed arc, or by a long tangent screw varying its inclinations, and by no means realizing the same value to a revolution in the middle and at either end of its length, yet one and all may easily be brought to give nearly equally correct results at last, if a right principle of reduction be followed. And one and all must submit in any case to some principle of reduction if they are to speak a common language, right or wrong; so that there is no needless labour in what is now to be advocated; and which is mainly this, viz. that the observation must be considered a differential one, both only and exceedingly; for it should be so arranged with respect to its absolute references as to be simultaneous nearly in time; and for space, to trust as little as possible to any appreciable length of the instrument's own scale.

Part of this principle has long since been carried out by various good observers; in so far that they collect a set of standard spectrum-places of certain very visible lines in wave-numbers, observe the readings of those lines in Nature on the scales of their own instruments, whatever they may be; and then, by the well-known method of projecting observations and tabular places on squared paper, with the instrument's scale along one side, and the wave-numbers on the adjoining one at right angles to the first, they deduce a set of numbers, translating the readings of their special scales at any point into the desiderated wave-expressions. The earliest example that I am acquainted with in spectroscopy was that of Dr. Vogel, who, in a paper on the spectrum of the Aurora, gave (from a curve previously formed) the readings of each revolution of his micrometer-screw in wave-lengths; and a complete exhibition of the whole process is contained in M. Lecoq de Boisbaudran's admirable work 'Spectres Lumineux,' pp. 21–24 and 195–200, vol. i., and plate xxix. vol. ii.

For approximate purposes and with small dispersions this method may be quite enough, and is certainly very convenient; but when ultra accuracy comes to be required, the momentary variations of temperature, in altering the dispersions of any prisms, as well as the local changes or varying errors in the scales themselves, must be met; wherefore then a small supplementary curve, for every hour's observation during a long evening, will be found a necessity, and may be easily arranged thus:—

Know what you are going to do, *i. e.* in what part of the spectrum you are going to observe. Select from your list of standard places of spectral lines, by great authorities for wave-length determinations, at least three fiducial reference-lines (of a kind which you have the mechanical, chemical, and other means for causing to appear), one at the beginning, another in the middle, and another at the end of the spectral arc you are about to measure; and be careful to observe these standard lines both before you begin, and after you have finished, observing your unknown phenomena between. Reduce all the observations, without exception, to wave-numbers by your general table for that purpose already formed, and you will find that your standard lines will not come out according to their exact tabular values in any instance; neither will the observations of them, at the end of the hour, give precisely the same results as those at its commencement; and this by larger quantities than can be attributed to mere errors of bisection in the field of view. But there is usually perceptible enough a method and a system in their variations, which when projected, even very roughly, *currente calamo*, on squared paper will give a most instructive and suggestive curve—enabling a series of supercorrections for each observation to be drawn out, dependent both on the time at which it was taken and the part of the spectral arc in which it was situated.

In fact there is hardly any source of error which cannot be picked up, and allowed for, by this principle of the curves at the beginning and end of each set of observations; seeing that those who desire the utmost perfection can make their spectral arcs within three reference points continually smaller, and may return on their standard lines at much shorter intervals than an hour, scrupulously avoiding also to alter either the focusing or the slit-breadth meanwhile. But, generally, one supplementary curve one width of slit, and one constant focus is enough for any fifth portion of the spectrum, when using those more moderate dispersions under which both chemical, stellar, and, in fact, almost all observers, except the great solar authorities probing into infinity, perform their most useful work; and it is within that dispersion limit we are now to confine our attention.

The chief necessity then for every working spectroscopic observer, who would both do good work and be understood by his countrymen therein, is, *first*, to be in possession of a list of well-determined places of certain spectral lines; and, *second*, to have the means of calling up these living lines of light into fact before him, to the end that he may observe them there and then with all distinctness, certainty, and facility too if possible.

As to the *first* half of this requirement, I have been engaged for several years in selecting and comparing the places of standard lines by different "grating" observers, much as the framers of catalogues of stars deal with their various authorities. But though I am daily using them, and am recommending them both for publication and general use as being, I believe, just about as good as they possibly can be at this time, yet I am aware they are not perfect, and that discrepancies will be found in the fifth, sometimes even in the fourth place of the numbers employed. But we need not wait for these ultra-improvements of difficult cases; for ages may pass away before the desired end is obtained, so very slow and long-linked are the steps of approximation by which the last touches of improvement are conveyed to the numerical measures of any branch of exact science: witness the many years during which, in time past, and even now, still varying results keep pouring forth from the astronomical press for that long since fairly approached quantity, the mean distance of the Earth from the Sun; or see new refraction tables on refraction tables still being observed for, as though the action of the atmosphere in altering the angular heights of the stars had never been considered before, and remarkably good work, too, accomplished on the basis of the older tables of fifty and more years ago.

But as to the *second* half of the above requirement, we are landed there at once among chemical and physical operations, some of which are very old and some exceedingly new; while day by day such altered explanations of both the old and the new are being given by mathematical physics, that some men stand almost aghast, wondering what it is all to end in, or what we are to understand by what we see even in the simplest experiments at the very beginning of the subject. This, however, is exactly the state of things into which it is my duty to try to throw in some little practically guiding light if it be possible; and to show, amid the conflict of theories, what may be most trusted, as well as the easiest means of obtaining it.

PRACTICAL SPECTRA AT FOUR STAGES OF TEMPERATURE.

How well all the world has been made acquainted with that little group of pictured spectra of the principal metalloids, when their salts are burned in lamp-flame, published by Professors Bunsen and Kirchhoff, and believed to be as simple as certain! But now M. Thalén denounces it as most vicious *, inexact, uncertain, and most complicated, introducing, by the act of combustion in open air, many things not well understood even yet; and altogether needlessly introduced, says he, because his friend M. Angström and himself had, previously to M. Bunsen's publication, shown that all the required incandescent vapourizings for spectroscopy could be produced by the electric spark under more simple and safe conditions. But even when using that very electric spark, Messrs. Plücker and Hittorf came to totally opposite conclusions, on the very foundations of all chemical physics, to Messrs. Angström and Thalén—the former demonstrating, as they believed, that every chemical element has totally different spectra at certain very different degrees of temperature; while the latter were equally strong that no real elementary matter could have possibly any but one single spectrum through all the ranges of temperature. They did not deny, by any means, that the look of many spectra is altered by change of temperature; as when, by increase thereof, what was in lithium's spectrum its α and brightest line in the red, sinks down to a third rank; its β line, in the orange, brightens up so as to become α; and a line not dreamt of before, in the blue, comes out as β. But these are mere intensity changes of one and the same spectrum, and the places of lines when visible at all are not altered by it in the least; while the effects, on the other hand, observed and made most visible by Plücker, and confessed to be true as facts by Thalén, showed an utter and total change of spectrum when passing from one stage of temperature to another: as when a spectrum of many hazy bands passes at a bound into a spectrum of a very few bright lines only; and this sort of change the Upsala physicists maintained could only have been brought about by the element under discussion having, at that moment, either combined, or left its combination, with some other element.

Fortunately, however, for our present purpose of procuring trustworthy stepping-stones for those who desire practically to spectroscope the unknown, we need not bind ourselves to any theoretical explanation of how or why the changes are produced, but only seek to know their appearances when made; and then we not only find a certain class of spectral phenomena always coming forth, and to be rigidly depended on (whenever they do come), at one given temperature over a considerable range of the same, and another class at another and unmistakably different temperature, but

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* "Recherches sur les Spectres des Métalloïdes" par A. J. Angström et T. R. Thalén, extrait des Nova Acta Reg. Soc. Sc. Upsal, sér. 3, vol. ix. pp. 3 & 4.

we soon arrive involuntarily at this piece of general physics, viz. that there are certain natural spectral appearances endued with a superior vitality as it were to others, so as to be capable of living through many successive stages of temperature, like some created animal beings through several geological ages. Wherefore who would not, if he could, use such lines as these, in preference to others, for his spectral references, when gauging the depths of the physical universe?

Now in spite of the diatribes of some excellent savants against lamp-flame for rendering incandescent certain chemical salts for the spectroscope, it is still so very generally used in the world, and found to be so convenient, economical, and typical of the lowest stage of temperature for producing effective luminosity, that we shall adopt it here as our first temperature stage and stand-point; excepting always the cold condition for absorption phenomena.

The next higher practical stage of temperature to lamp-flame is that of the electric spark, viz. the simple, uncondensed spark, as it is called, which is given off by the now universally known and employed Ruhmkorff's induction-coil; but which spark is nevertheless, from recent and still continuing experiments by Prof. Alexander Herschel, far more intense than the positively simple and absolutely uncondensed spark of the same length coming off from a frictional-electricity machine. The induction-coil spark is in fact exposed to a primary condensation from the condenser included in its own base; but though somewhat strengthened thereby, it cannot approach the intensity of those sparks which are submitted to a second and more ostensible condensation by Leyden jar, after the spark has been given off by the coil. The simple coil-spark forms therefore a temperature field of its own, producing a very similar set of effects, varying only in brightness, whether the sparks are 0·5, or 1, or several inches long; and these effects, while notably transcending lamp-flame in dealing with solutions of metallic salts, are yet not hot enough to bring out the lines of many of the pure metals themselves. With simple sparks therefore of 2 inches long, M. Lecoq de Boisbaudran, with admirable judgment confining himself thereto, carried out all his researches on metallic compounds; and it was enough, for he was thereby finally rewarded with his discovery of that new metal, which he presented patriotically to his county as "gallium."

But when that simple spark is exchanged for the spark subsequently condensed by jar, such an intense increase of heat is then produced, that whether they be large or small sparks the spectra of all the metallic salts are changed almost beyond recognition; supplied with a greater number of lines, and these cleaner, sharper, freer from haze-bands than before; and now, too, even the most refractory metals may be dealt with in their dried and hammered state. The condensed spark therefore, while it forms a distinctly third stage of temperature, never to be mixed up with the second, is the most important of all for astronomical spectroscopy; for it is then only that the more numerous and striking coincidences of chemical bright lines and dark Fraunhofer solar lines begin to come out; while the Sun itself forms a fourth, and apparently still more intense stage of heat, with further resulting modifications of spectral phenomena (see 'Transactions of Royal Society of Edinburgh,' vol. xxix.; a paper entitled "the Solar Spectrum in 1877 and 1878").

Beginning now practically with No. 1, or lamp-flame effects, and taking our stand therein, and searching among its lines for any which live through into the higher temperatures above indicated, the following 21 instances may be collected (confirmed, too, by the records of more than one observer), wherein the whole 21 reproduce themselves on the second stage of temperature, viz. that of the simple electric spark, say 2 inches long; 18 reproduce themselves in 6-inch electric sparks, powerfully jar-condensed; but 3 only are found in the supernally heated fires of the Sun; or thus:—

Chemicals, the chief spectral lines in	Wave-number spectrum place observed in			
	Lamp-flame.	Two-inch simple induction-sparks.	Six-inch Induction-sparks, jar-condensed.	The Sun.
Barium, α	45 884	45 889	45 896	
Calcium, e	39 082	39 082	39 085	39 093
„ α^1	40 957	40 958	40 953	?
„ α^2	41 095	41 095	41 100	?
Calcium, anonym.	60 099	60 099	60 098	60 100
Indium, α	56 310	56 310	56 319	
„ β	61 937	61 937	61 935	
Lithium, α	37 878	37 878	37 873	
„ β	41 625	41 626	41 629	
Manganese, α	45 471	45 461		
Potassium, α^1	32 990	32 990	32 986	
„ α^2	33 090	33 158	33 116	
„ β^1	43 559	43 558	43 567	
„ β^2	43 770	43 786	43 785	
„ β^3	43 919	43 926	43 929	
„ γ	62 798	62 807	62 810	
Sodium, α	43 108	43 108	{ 43 086 { 43 131	43 086 43 130
Strontium, γ	37 652	37 643		
„ δ	39 082	39 096		
„ β	55 134	55 131	{ 55 121 { 55 132	
Thallium, α	47 487	47 487	47 490	

The above instances being all of notably conspicuous lines, such as there are very few of in any spectrum, there can be no doubt, from the closeness of the observed places by different observers under very different circumstances, that the same feature of the same chemical has been caught at each temperature stage wherever it is visible at all; and of the other, and far more numerous, fainter lamp-flame lines which blaze out there, but are never met with again in higher spheres of heat, we need say no more on our present inquiry. Nor, again, need we enlarge on the many lines in simple electric spark spectra, totally unknown to lamp-flame; and the still more numerous lines in condensed electric sparks, still wider from lamp-flame, but having so many analogues in the Sun. Hundreds of these latter coincidences were discovered by Angström and Thalén; and hundreds more, as of the legions of titanium, have been added by Lockyer, Meldola, and others; but these later additions have been generally of such almost infinitely thin weak lines, that their whole number hardly makes up, in practical solar physics, for one of the really great lines of the earlier day; *i. e.* one of those lines denoted by Fraunhofer with the capitals of the alphabet from A to H inclusive; while to make the new swarms of excessively faint lines show at all requires a combined force of galvanic battery, size of coil, surface of condenser, and dispersion of prisms such as few private persons are possessed of. PIAZZI SMYTH.

[To be continued.]

Solar Physics.—Part II.

OF recent spectro-photographic observation, I may mention the valuable work of M. Cornu, who, working at the other extremity of the spectrum from Captain Abney, has extended it beyond the violet to a wave-length of 2900, far beyond which the solar spectrum undoubtedly exists, but where M. Cornu finds our own atmosphere to interpose an almost impassable barrier. The solar spectrum, therefore, is now known by photography through three times the extent of the visible portion, and this great gain on our former knowledge may be said to have been completed for us in the past year.

In last November and subsequently, Mr. Lockyer has made the extremely important announcement that, reasoning from analogies furnished by known compoumds, he has been able to show that many elements are really compound bodies, which, incompletely dissociated at the highest temperature we can command, furnish under the form of feeble lines the spectra of their components.

I do not enter here into discussion of points still in debate; but that which has arisen round this and the recent communications of Dr. Henry Draper, at any rate, elicits the evidence of the immense labour now requisite in establishing new facts in our science; and the refinement of some of the adverse explanations suggested in controversy shows us to how limited a company of specialists we must look as judges in matters so important.

The instrumental aids of our study have grown in the period under review with the demand for greater accuracy, until the detached prisms of Kirchhoff's apparatus are replaced by trains of automatically adjustable mechanism, giving us in Thollon's recent instrument the equivalent dispersion of thirty prisms of flint, or what has replaced the "gitter" of Fraunhofer, that wonderful product of skill, the Rutherfurd grating, which for a large variety of uses has already supplanted the prism. Observatories especially devoted to solar physics are being established by European governments, as at Potsdam by Prussia, and at Meudon by France. I have already alluded to that on Etna, and I hope it will not be long before we have a distinctly physical observatory within our own territory. There is no step in our power to take which promises so much for immediate advance as the installation of one in a suitably elevated station, for certain investigations can be made *only* under this condition, and no amount of instrumental appliance, patience, or skill, at a lower altitude, supplies their place.

In now reviewing the acquisitions which this twenty years' labour has brought us, we cannot but agree that we have achieved a great deal, and yet must admit, with wonder at the field still before us, how little is our progress in comparison with what remains unknown.

We have found out how to detect daily the outbursts from the Sun which were before invisible; but we watch these outpourings of enormous forms without yet knowing what drives them forth, without being sure how far our very view is not in part illusion.

We have learned how to study and fix many of the wonderful details of spot-actions without knowing what spots are. We see them presenting themselves in increasing importance through a term of years, and then diminishing, and we attempt to assign a period to these cycles of growth and decay. This period is often fixed at about eleven years, with a perhaps unjustifiable confidence, for we cannot be said to know whether what we have seen in so brief a time is constant or variable, nor whether it be not the mere incident of some greater cycle, whose course began before man was here to see it, and whose term may not be complete till he has gone.

We are possibly now led to ask what our science has taught us on the connection of these remote changes with questions which affect our daily lives, and perhaps to put the utilitarian question, "What is all this worth?"

We find at the present time our study growing into a closer union, not merely with stellar astronomy on the one hand and terrestrial meteorology on the other, but with all the physical sciences, than would once have been supposed possible. Thus, to give a single instance, whatever be the result of the discussions aroused by Mr. Lockyer's statements, it seems likely that we are to look to the analysis of the solar radiations for the most favourable evidence of that resolvability of our so-called elements to simpler forms, which our chemists are now very generally ready to admit as possible.

It is in the solar spectrum that we are now searching for the laws of the molecular groupings which affect the ultimate constitution of matter; and in recent questions as to the real nature of certain terrestrial elements, which our laboratories cannot yet deal with, the Mount Sherman observations of Professor Young on the appearance of their analogues in the Sun have been accepted by both parties in debates before the Royal Society as pertinent evidence, the only doubt as to which lies in its interpretation.

Of problems "practical" in the sense that their utility is apparent alike to the learned and the unlearned, there are two at least of the highest importance which now occupy us.

The solar heat, which grows for us the food by which we live, is no doubt in one sense the final cause of every meteorological change, bringing those years of want and years of plenty which are due to local variations of climate, that depend, through a chain of causes very remote and obscure, no doubt, yet finally, upon the Sun. We have seen the magnetic needles vibrating all over the globe together at the time of a sudden commotion upon the solar surface; we watch the increase and decrease of auroras, and find we can almost predict their frequency, so apparently united are they by some mysterious bond with the changes of solar spots; and we look with natural hope for other signs of union which may enable us to anticipate more important effects on our meteorology. Extreme pains have been devoted—in some cases misdevoted—to researches aiming to establish such a connexion, by collecting data as to the changes in rainfall, the movements of storms, the prices of grain, and of almost every feature of terrestrial meteorology, in order to see whether these run through periods coincident with those of known changes on the solar surface. It will be admitted by the most utilitarian that the end aimed at is a worthy one, for the practical result of success, such as some believe possible, would be to enable its attainer to predict the price of breadstuffs years in advance, to control the markets of the world, to bestow, if unselfish, an almost priceless knowledge to man, or, if self-seeking, to acquire wealth beyond wish.

I need hardly say that the attempt has thus far been unsuccessful. There is hardly any topic on which there is more popular interest, hardly any on which there is more popular error, than this of the supposed influence of the Sun on the weather. By means of the study of what Professor Smyth terms the "rainband" in the spectrum, we appear to have lately gained increased facility in predicting local weather-changes; but, excepting this comparatively unimportant contribution, studies connected with the Sun have as yet done very little for us here, and it seems necessary to say that, as far as prophecy is concerned, none of us are yet prophets, or more able to tell from our knowledge of the Sun what the weather will be next week than what the harvest will be next year.

There is another utilitarian aspect of our study about which there is less public interest, but more real promise: I mean that which concerns the direct application of solar heat to arts and manufactures. These are now all using it indirectly—by the water, for instance, which it lifts into the clouds to turn the mills of Lowell or Lawrence as it flows back to the sea, or by the coal which it stored in former ages to drive our engines to-day. These indirect means use but the feeblest portion of the solar heat, which is in theory capable of furnishing nearly one horse-power for each square yard of the earth's surface under full sunshine.

What we have actually realized in experiments is still considerable.

The visitor to the last Paris Exposition may have seen upon its grounds a machine of strange appearance, in the open air, pointing sunward the axis of an immense reflector, shaped like a truncated cone, which gathered the rays to a linear focus upon the boiler of a working steam-engine, which it drove thus by direct solar heat. Many not dissimilar solar engines have been built in this country and in India, the particular one of which I speak, due to M. Mouchot, having actually realized about one horse-power to ten feet square of surface.

We are startled, when we make the computation, to find the immensity of the force thus placed at our disposal, or to see what the utilization of the waste places of the Earth would bring us. Upon the limited area of the Adirondack wilderness to the north of us, for instance, the daily wasted sun-power actually realizable, and after every allowance for loss, is many times that of all the estimated steam-power at present in use in the whole world. I am not myself so far utilitarian as to wish to see this use made of our pleasant summer haunts; but there are regions of the Earth at present as entirely worthless as that great African desert which it is now proposed to partly reconvert to an inland sea, a sun-burned area now apparently hopelessly useless to man, and yet on which an amount of power is every year poured in utter waste which could not be made good by the consumption of all the coal known to underlie the soil of Great Britain.

Such machines as those of M. Mouchot, owing to the expense of construction and attendance, cost more than an engine driven by coal, though the Sun supplies its power gratis; but it is simply, it seems to me, a question of time when, with another form which I believe our researches already indicate, such engines may become an economical as well as a mechanical success; and in a larger sense it is still only a question of time when the rapidly consuming coal-beds of Great Britain yield their last, and her manufacturing empire is transferred to countries which have not exhausted their supply. But these will exhaust their own in turn; the stock, though great, is finite and not renewable; and we must look, for the only power we know which can replace coal, to those regions of the Earth now desolated by solar heat, and to which future empire may probably tend.

We have considered the past and the present of our study; for its future there remains the solution of all the great problems I have already alluded to; but these questions are so interlocked that the complete answer to one will probably not be given till we are nearly ready to answer all.

I have spoken of the fallacy of the popular impression of the result of our study as enabling us to predict the weather, or to anticipate the character of coming harvests. Repeating my belief that we as yet know nothing here, or next to nothing, I yet do not mean to disparage the object of such researches, nor even to deny the possibility of their ultimate success. We can look forward, among other fair dreams for our science's future, to a time when it will enable us to predict the years of plenty or play the part of a beneficent Providence, by warning in season against those of famine, which have cost in our time so many million lives in China and in India. These are, I repeat, still dreams only; but we may call them hopes if we will—hopes of which increased knowledge may deprive us, but of which we cannot say it may not bring fruition.

There remains among the greatest problems of the future of our science the all-important one to the whole human race of the future constancy of the Sun's heat, of which we have, it seems to me, no assurance of the present rate of supply. We have, it is true, every assurance that in the contraction of the solar mass and in the supply of meteoric matter, we have heat to warm the human race for periods almost beyond limit; but we learn also that this heat is tempered to us by a solar envelope, which seems to be, as far as we know, in conditions which do not favour stability. It is constantly being added to by eruptions from within the Sun, caused by we know not what, and constantly diminished by some counter-process which we understand as little. When we consider that the thickening of this solar atmosphere would bring back the age of ice, or its thinning carry our polar regions to tropical temperature, and when we remember that rhythmical action, not uniformity, seems to be the law of nature here, we can feel no certainty of the future constancy of the solar heat, nor of our protection against such changes as seem to have befallen other suns in space, and against which we are powerless to guard.

But such considerations of our ignorance and helplessness, while they may prevent us from any undue pride in what our science has already attained, may teach us renewed confidence from the very brevity of our life. These green fields around us were once covered with glacial ice, and the change has been absolute from that condition to the one of to-day. Yet in the lifetime of any one of the thousands of insect generations which have succeeded each other in these fields, there must have seemed no alteration; and, remembering what instants our own lives are, in a like comparison with the uncounted ages of the Sun's history, we may well reckon that our generation shall see no change.

In the little span which is allowed us, however, we will try to learn something more of that source of light, life, and power of which we are materially the creatures; and, if we can leave a knowledge which will not die with ourselves, feel that we have left also the record of a something in us "which owes no homage to the Sun." S. P. LANGLEY.

Total Solar Eclipses.

No further particulars of the total eclipse of January 11, in the present year, seem to have come to hand, other than what were contained in the 'Observatory' for March and April, nos. 35 and 36.

The expression "clear white light" applied to the corona (see 'Observatory,' March, no. 35, p. 360), reminds one of the "delicate band" (*ταινία λεπτὴ*) spoken of by "Leon the deacon" in his account of the totality of the eclipse of 968, December 22 (see 'Astronomische Nachrichten,' no. 1832). [The latter phenomenon was total, over a tract of country in the south-west of our own land, very soon after sunrise.] Undoubtedly this eclipse of 968 has an interest of its own for us, in being the first in which we have a distinct mention of the corona. Is there any further distinct reference to it till Dr. Wyberd's observation in 1652?

As to the Californian eclipse of last January, it is not mentioned by Mr. R. T. Paine in his account of the American total solar eclipses of the present century, given in the Monthly Notices R. A. S. 1869, because he refers to those total in the United States. It appears the shadow must have crossed the States five times in the course of the present century (if we include such western parts as Texas), viz. in 1806, 1834, 1860, 1869, 1878, and it will do so again in 1900. As to the passages of the shadow across the American continent after that date, I have seen no account; but on roughly examining the eclipses in the first half of the next century, it appears the conditions would be fulfilled on the following dates:—

1918, June 8; 1925, January 24; 1932, August 31.

The first mentioned seems total in the extreme Southern States, the last two in the extreme Northern ones.

Abbenhall Rectory, Gloucester, S. J. JOHNSON.
1880, August 2.

Double Stars for September.

For the most interesting and important pairs see 'The Observatory,' Vol. ii. p. 148 and Vol. iii. p. 143.

α Aquilæ. R.A. 19^{h} 44^{m}·9, Dec. +8° 33′. Mag. A 1½, B 10.

1821	326°·2	153″·5	Σ.
1877	311 ·9	156 ·1	Flammarion.

Flammarion says that Alvan Clark has seen eight small stars nearer the principal star than the old companion. The proper motion of α is considerable, +0″·54 in R.A. and −0″·38 in N.P.D.

β Aquilæ. R.A. 19^{h} 49^{m}·4, Dec. +6° 6′. Mag. 3·4, 11·3.

First recorded double at Munich in 1838. It is OΣ 532.

1838·8	23°·8	11″·81	Munich.
1878·6	16 ·5	12 ·00	β.

The two stars have a common proper motion.

β Delphini. R.A. 20^{h} 31^{m}·9, Dec. +14° 11′. Mag. A 4, B 6, C 13, D 10½.

AB	355°±	0″·7±	1873·6	β.
	53 ·7	0 ·24	1878·6	„
AC	115 ·7	27 ·37	·6	„
AD	335 ·6	35 ·06	1877·7	„

This is β 151. C was measured by H_2 and Sm. AB form a binary in rapid motion. AD was observed by H_1 in 1781, 348°, 25″·9.

κ Delphini. R.A. 20^{h} 33^{m}·3, Dec. +9° 40′. Mag. 4·8, 11·4.

This is OΣ 533.

1851·6	12°·4	10″·23	OΣ.
1878·2	329 ·2	10 ·59	β.

α Delphini. R.A. 20^{h} 34^{m}, Dec. +15° 29′. Mag. A 5·6, B ?, C 13, D 13, E?

B is an excessively faint star detected by β with the 26-inch.

AC	279°·7	42″·02	1877·8	β.
	·9	·47	1878·7	„
AD	330 ·2	47 ·96	7·8	„
AE	114 ·1	....	7·8	„

C is the nearest star seen by H_1.

λ Cygni. R.A. 20^{h} 42^{m}·7, Dec. +36° 3′. Mag. 5·5, 7·6, 10.

This is OΣ 413.

AB	1842·6	122°·3	0″·65	OΣ.
	1878·5	85 ·3	0 ·74	β.

A rapid binary.

AC probably unchanged, distance 85″±.

OΣ 424. R.A. $20^h\ 53^m{\cdot}6$, Dec. +15° 6′. Mag. 7–8, 8·9.

AB	1845·7	332°·0	0″·46	OΣ.
	1878·6	327 ·7	0 ·45	β.

β saw a faint star on an angle of 306°·3 in 1878.

JOSEPH GLEDHILL.

Mr. Edward Crossley's Observatory,
Bermerside, Halifax.

Meteor Notes for September *.

OBSERVATIONS may be made, in the absence of moonlight, during the first 12 nights and in the evenings after the 26th. A shower of large meteors from a point in 306° +54° N. of α Cygni should be expected on the 1st–3rd; and on the 6th–7th, 11th–13th, and 25th fireballs are especially numerous. The chief radiants determined by Heis during the first half of the month were at 39° +63°, 45° +40°, 320° +63°, 342° +9°, and 10° +39°. He also gives a shower at 55° +37° (near ε Persei) which is probably the same as one observed by Tupman in 1871, Sept. 7–15, at 66° +40°, by Zezioli in 1869 Sept. 6, 60° +32°, and by the writer in 1877 Sept. 4–16, 61° +36°. The central position of the radiant derived from a mean of these observations is at 60°·5 +36°·2. There are several showers in Auriga during this and the ensuing months of October and November. Mr. Greg gives the average position as at 84° +48° from a number of different observations. In 1879, between the 14th–25th, five showers of Aurigids were observed more or less distinctly from the following points:—76° +32°, 87° +43°, 76° +44°, 90° +60°, and 76° +56°.

Recent Observations.—The large meteor seen by Mr. Dunkin on April 7, and described in the 'Observatory' for May, p. 414, was also observed by Mr. J. M. Coates at Moulton, Spalding (lat. 52° 48′ N., long. 0° 3′ W.), and he gives the path as from R.A. $6^h\ 45^m$, Dec. 17° S., to R.A. $8^h\ 0^m$, Dec. 25° S. There was much haze; only the larger stars were visible. The train of the meteor was very long. The "wavy course" described by several observers was not noticed. Duration of the meteor 6 or 7 secs., the motion being apparently very slow. The radiant-point derived from the combined observations is at 42° +18° in the eastern extremity of Aries.

The bright meteor of July 9, $9^h\ 46^m$ ('Observatory,' August, p. 513), was seen by Mr. F. C. Penrose at Wimbledon, at $9^h\ 45^m$, and estimated = ♀. It passed about 4° above Spica and disappeared soon afterwards, as nearly as could be noted, in alt. 16°, azimuth 50° west of south, travelling at the rate of 8° per second. The apparent course was only slightly inclined to the horizon, approaching it at an angle of about 1 in 10. Colour light yellowish till the moment of extinction, when it became blue and fainter, finally disappearing without any sign of explosion. Its course was somewhat wavy, and the tail it left behind very evanescent *. The meteor was also observed from Clapham Common, at 9^h 45^m, in the S.W., travelling through Virgo about 10° or so above Spica. It was brighter than Venus and moved slowly.

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* See 'Observatory,' Vol. i. p. 157, Vol. ii. p. 164, and Vol. iii. p. 145, for further notes on September showers. See also Vol. iii. p. 203 for a list of the major radiant-points observed by the writer in September 1879.

The writer at Bristol noted meteors as unusually numerous on July 29, between 10^h and 12^h; 15 were observed in an occasional look out during that interval, but the Moon was up after 10^h 45^m. Some fine meteors were seen from a radiant at 257° +64° near ζ Draconis. On Aug. 2, 11^h to 12^h, 10 meteors were seen, of which 4 belonged to the Perseids II., but the sky was partly overcast. Mr. Corder at Writtle saw more than 60 meteors, in a watch exceeding 4 hours, on July 29–30; and 20 of these were Perseids, radiating, as nearly as possible, from 29° +56° (χ Persei). A few Aquariads were seen from a centre at 331° −10°.

The Perseids were observed with marked success this year, the Moon being absent and the weather eminently favourable for such work. At Bristol the writer made observations as follows:—

Date. 1880.	Watch. h	Time. h	h	Meteors seen.	Perseids.	Horary nos., 1 observer. All meteors.	Perseids.	Chief radiant. ° °
Aug. 6	1½	10 to	11½	19	5	13	3	38 +56
7, 8 ...	4½	11	14	112	54	25	12	41 +55
9	2½	11	13½	111	71	44	28	44 +55
10	1½	10	11½	50	41	34	28	45 +57
11	2½	10½	13	64	43	26	17	48 +57
12	2½	10½	13	45	19	18	8	
13	1½	10½	12	18	7	12	5	49½+57½
Aug. 6–13...	16½	10 to	14	419	240	24·6	14·4	44 +56

The maximum apparently occurred on the night of the 9th; but on the 10th a fog partly obscured the stars and had increased so much at 11^h 30^m that further observations were impossible. Moreover, the watch was began at an earlier period than usual, when the radiant was low, and the fact must be remembered that this year being leap year the meteors might be expected to return earlier; thus the real intensity of the display may have taken place before the evening of the 10th. The radiant-point was determined each night from short meteors close to it, and it apparently advanced from 38° +56° to 49½° +57½° during the eight consecutive nights of observation. The streaks which the meteors almost invariably

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* 'Nature,' July 22, 1880, p. 267.

left, marking the directions of their flights, allowed the centre of divergence to be found with great accuracy; and though a few Perseids were seen not exactly conformable to one and the same point of departure, the radiant was sharply defined on each occasion. The shifting of the radiant was noticed by the writer in August 1877, and referred to in the 'Observatory,' Vol. i. p. 166; and it seems probable that the shower of 20 meteors seen by Mr. Corder on July 29–30 this year, from a point at 29° + 56° (and described above), together with the notable display of streak-leaving meteors from below χ Persei seen by the writer in 1878 on July 30–Aug. 1, and called Perseids II., may after all be the avant-couriers of the true Perseids and belong, in fact, to the same identical stream.

The largest meteors recently seen at Bristol were as follows:—

Date.	Time.	Mag.	Path from α	Path from δ	Path to α	Path to δ
1880.	h m		°	°	°	°
Aug. 7 ……	12 25	< ♀	55	+53	64	+50
„ 8 ……	13 37	=¼ ☽	266½	+ 7	264½	+ 2
„ 9 ……	12 23	> ♀	332	+ 7	320	−10

These meteors left bright streaks and belonged to the Perseids.

Many others were seen equal to Jupiter or Mars. The fireball of Aug. 8, 13^h 37^m, was remarkably brilliant; at the time of its appearance the sky had become overcast except at a low altitude in the western sky, where a few stars could still be observed; a vivid flash came from that direction, and a bright streak was immediately noticed over the star γ Ophiuchi.

Mr. H. Corder, at Writtle, Essex, saw 395 meteors between Aug. 1 and 11, of which 234 were conformable to Perseus. The night of the 10th was very hazy and generally overcast, so that between 9^h and 12^h only 39 were recorded; but it was estimated that with a clear sky the horary number would have been about 40. The chief radiant of the true Perseids was determined at 45° + 58°.

Major Tupman observing at Cookham, near Maidenhead, saw 13 meteors (11 Perseids) on Aug. 8, 10^h 34^m to 11^h 34^m. On Aug. 9 the sky was very clear, and a watch sustained from 9^h 56^m to 12^h 5^m revealed 31 meteors, of which 28 were Perseids and 3 Cygnids. On Aug. 10, 9^h 5^m to 10^h 43^m, there were 15 or 16 Perseids and a few other small meteors. Sky clouded over at 10^h 43^m. On Aug. 11 the sky was generally overcast.

The Rev. G. T. Ryves reports from Stoke-on-Trent as follows:— Aug. 8, 10^h to 11^h, 19 meteors, including 10 or 11 Perseids. On Aug. 9, between 9^h 45^m and 12^h 45^m, 95 meteors were seen, including 80 or 85 Perseids. On Aug. 10, 9^h 30^m to 11^h 30^m, 126 meteors, nearly all of which were Perseids. Mr. Ryves was assisted by two friends on the 9th before 11^h, and during the whole period of observation on the 10th. He gives diagrams of some of the largest meteors seen, and notes that at 12^h 25^m, Aug. 9, a very bright meteor was imperfectly seen in the Milky Way near horizon in S.W., moving about S.W. This is evidently the same meteor as one seen at Bristol at $12^h\ 23^m$ that night. Mr. Ryves, in summing up his observations, remarks that "in this year's display there has been a great falling off in the average brightness of the meteors as compared with former years. Of the 240 meteors seen here, not more than a dozen were such as would have attracted the attention of any but trained observers, the great majority requiring rather exceptionally keen eyes to detect them at all."

The Rev. S. J. Johnson, of Mitcheldean, Glos., writes that on Aug. 9, $10^h\ 57^m$ to $11^h\ 57^m$, 20 meteors were seen there. On Aug. 10 there was much cloud at times, especially in the eastern sky. 16 meteors were noted between $10^h\ 3^m$ and $11^h\ 3^m$. On Aug. 11, 12 meteors were seen in a clear sky between 10^h and 11^h. Several of the meteors observed on these nights were =♃, and a large proportion as bright as 1st-mag. stars. At Bristol the brilliancy of the visible meteors was only recorded in special instances, but the display was thought to compare favourably in this respect with its former apparitions.

A bright aurora was seen by several observers on the nights of August 11 and 12.

W. F. Denning.

CORRESPONDENCE.

To the Editor of 'The Observatory.'

δ Cygni.

Sir,—

Great interest attaches to this star on account of the difficulty observers have had, at various periods, in seeing the small companion. This can scarcely be due to variation in the state of the air, as experienced observers could tell when this should or should not prevent their seeing such an object. But more important still is the strange variety in the estimates of B's colour. Could some of your correspondents, who have access to such records, collect the estimates of its colour made by different observers, and by the same observer at various times, an advance might be made towards an explanation of these anomalies. A variation in colour is not less probable than one in magnitude; and if proved to exist in this case, will throw light on the condition of some stellar envelopes.

To observe the spectrum of both for variations is also important.

The companion seems to me now to be a muddy blue, mag. 7.

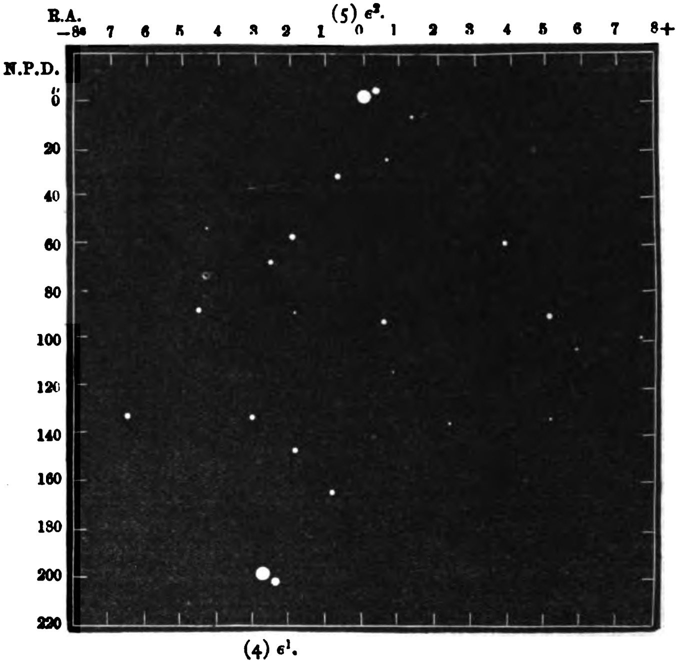
I am Sir, yours faithfully,
Edward B. Kirk.

Saltcoats, Ayrshire, N.B.,
1880, July 24.

The Group of ε Lyræ.

Sir,—

If the enclosed map of the small stars visible within a circle which just includes the components of ε Lyræ and a small star



8 seconds in R.A. on their east is of any interest in connection with Mr. Gledhill's interesting paper in the last Number, it is at the service of the readers of the 'Observatory.' It was constructed with some care, the places of the chief stars being measured by the micrometer, and the fainter ones being placed by alignment. The stars it contains were seen in May 1876 with my With speculum of 8·15-in. aperture, power 220. Other stars are visible near; but my attention was limited to the circle described above.

I am, Sir, yours truly,

Brighton, 1880, Aug. 5. Henry Pratt.

Elements of Comet I. 1880.

Sir,—

I regret to say that, notwithstanding a sharp look-out, the persistently cloudy state of the horizon prevented me from getting one observation even of the great southern comet of February last. Although the tail was visible on several evenings, the head was seen for a few seconds only on one occasion. Mr. Ellery has kindly supplied me with observations made at the Melbourne Observatory on February 9, 10, 14, 15, 16, and 17. I have deduced the following parabolic orbit from the observations of February 9, 14, and 17 corrected for aberration and parallax:—

Per. Pass. 1880, Jan. 27^{d}·56330, G. M. T.
Long. Per. 227° 22′ 53″·4. Inclination 36° 41′ 41″·9.
Long. Node 358 22 48 ·6. Per. Dist. 0·0067243.
Mean Equinox 1880·0. Motion Retrograde.

These elements differ in some respects from Mr. Hind's approximate orbit in the 'Observatory' for April last. Unfortunately the position for Feb. 14 rests on one comparison only with σ Sculptoris. Three other comparisons were obtained on the same evening with an anonymous star, whose accurate place is yet to be determined. I find it impossible, on the hypothesis of parabolic motion, to satisfy the middle place both in longitude and latitude. When the longitude is satisfied, the residual in latitude becomes magnified far beyond the limits of probable error of observation; the orbit is therefore probably elliptic. My elements closely resemble the third orbit computed by Laugier and Mauvais for the great southern comet of 1843; and it is a remarkable fact that these computers assign a period of thirty-five years to their ellipse. (See Encke's edition of Olbers' 'Abhandlung,' 1847, pp. 198 and 230.) I trust when Dr. Gould's early observations come to hand to be able to deduce a more accurate orbit.

I am, Sir, yours faithfully,
JOHN TEBBUTT.

Windsor, N. S. Wales,
1880, June 21.

A Fine Prominence.

SIR,—

On the 31st of July last there was a splendid display on the limb of the Sun. If you have heard nothing of it from any other quarter, an account from an amateur may seem to you likely to interest some of your readers.

The instrument used was a telescope of 4-in. aperture, the spectroscope carrying three compound prisms (each composed of 5 simple prisms), two being fixed in a frame and screwed in as an eyepiece, the third moving in arc, with a collimator and telescope of six inches: the weight counterpoised from a wooden arm above.

On the following limb, a little north, appeared a sheaf of prominences, spear-headed, not so high as the vaporous masses usually seen, but intensely bright, giving the idea of molten metal. Forms changing, but not rapidly. I immediately saw also another image, fainter but quite distinct, nearly halfway between C and B. Having no cross lines, I could only estimate the position, which seemed to be at $\frac{4}{9}$ of the distance towards B. I see, in Schellen's 'Spectrum Analysis,' mention made of a line in this part of the spectrum as seen occasionally.

I then turned to the line D 3. The image here was magnificent, in bright pale straw-colour. But, besides, four or five images of the prominences appeared as yellow spots, threaded on the black D 1 and D 2 lines. A very beautiful sight.

The same appearance, less bright, was seen on the lines *b*, and also on the fourth magnesium line lower down in the spectrum.

At F the image was exceedingly bright. Beyond this the hydrogen lines were not seen, the correction of the prisms not being favourable.

The day following a large spot appeared, which must have been on the limb at the place and time of this display. But to-day the spot must be on the opposite limb, and there are no signs of any such activity.

I sometimes wonder that these solar changes are so little noticed, and imagine how magnificent they would be if the details were brought out by powerful instruments.

A telescope mounted in the usual way is not well suited for this purpose. The pillar is not high enough, the mounting is not, usually, made to carry the weight, and when a train of prisms is used the image is dwarfed by the small collimator.

I would suggest an arrangement like the following:—a rough tube, two feet longer than focal length, carrying inside two compound prisms with 10-inch collimator, the third compound prism moving in arc; this tube mounted, when required, on pillar 6 feet high, on plain polar axis, as in the so-called *zodiacal* arrangement figured in Simms's little book, simple hand movement by Hook's joint and tangent screw, and no graduation. This arrangement I have for several years used, and have found it inexpensive and thoroughly efficient. The tube is taken out from shelter, object-glass put in, and the whole set up in five minutes, and the prominences are seen and followed with perfect ease.

I am, very faithfully yours,
J. G. Hickley.

Walton Rectory,
Bridgwater, Aug. 13, 1880.

Spectrum of Aurora.

Sir,—

Between 10^{h} 30^{m} and midnight on August 12 there was a very fine display of Aurora here. Long pale silvery streamers rose from the northern horizon at all points of an arc of at least 150°, the centre of which was considerably west of the true north. These converged towards a point 15° or 20° south of the zenith, but did not quite meet, breaking off before the point of convergence was reached.

The spectrum consisted of:—

1. A bright band in the yellow more refrangible than D.
2. A fainter and broader band about *b*, brighter and better defined on the least refrangible side.

3. A pretty distinct band near F.
4. A very faint line near G.
5. Between D and F a faint light, broken near F by a broad dark band.

This spectrum was the same in all parts, allowing for varying brightness.

On the Sun that day were 5 or 6 large spots recently formed.

I am, Sir, yours faithfully,
EDWARD B. KIRK.

Saltcoats, N.B.,
1880, August 13.

[Mr. Hickley, writing from Bridgwater, mentions a beautiful aurora both on Aug. 11 and 12, beginning about 8^h 30^m and continuing till midnight. It was brightest on Aug. 12, and the spectrum showed three bright bands and a hazy band besides. Simultaneously with this display, disturbances of the magnets such as have not been observed for some years past were recorded at Greenwich, where magnetic storms accompanied by strong earth-currents were noted from Aug. 10 22^h to Aug. 11 12^h, from Aug. 12 0^h to 18^h, and again in the evening of Aug. 13. No aurora was seen at Greenwich, the sky being cloudy after 10^h on Aug. 12 and 13 and cloudy throughout on Aug. 11.

At the time of these disturbances, there were five large spots on the Sun's disk, the greatest display of solar activity that we have witnessed for several years. The largest of these spots appeared at the east limb about July 31 or Aug. 1, and on Aug. 9 (after a period of cloudy weather) four other groups were noted near the last limb.—ED.]

NOTES.

THE AUGUST PERSEIDS.—At the Royal Observatory, Greenwich, the sky was generally unfavourable on August 7, 8, 12, and 13; but on August 9, 10, and 11 it was sufficiently fine for observation, although not perfectly good on either of the two last-mentioned nights. The paths of 106 meteors were recorded on August 9, 10, and 11, with the following result:—

Day.	Total number noted.	Number from Perseus radiant.	Percentage from Perseus radiant.
Aug. 9	51	39	76
10	36	27	75
11	19	11	58

The proportion of Perseus meteors noted is thus considerably greater this year than in the year 1879.

On August 10 the aggregate number of meteors appearing in each quarter-hour, from 9^h to 14^h, was counted (without noting the paths) by two observers, one looking northward, and one southward. In all 129 were seen, distributed as follows:—

Period.	9^h to 10^h.	10^h to 11^h.	11^h to 12^h.	12^h to 13^h.	13^h to 14^h.
First quarter ..	6	2	8	3	8
Second quarter..	7	3	14	4	4
Third quarter ..	2	8	4	10	10
Fourth quarter..	3	7	5	13	8
Hourly totals..	18	20	31	30	30

The number of meteors whose paths were observed on August 10 giving, as above, 75 per cent. as the proportion of meteors from the Perseus radiant, this proportion will probably apply also to the preceding hourly numbers, from which we may conclude that the greatest hourly number of Perseus meteors on Aug. 10 was under 25.

The observations were made by the observers of the Magnetical and Meteorological Department.

THE RESISTING MEDIUM AND THE COMETS OF 1843 AND 1880[*].—Prof. Oppolzer has published two important papers on this subject. In the first he discusses the effect of a resisting medium on Winnecke's comet of $5\frac{1}{2}$ years' period, which has a perihelion distance of about 0·77, and compares the effects on Encke's, Winnecke's, and Faye's comets respectively. Winnecke's comet has been observed in 1819, 1858, 1869, and 1875, its identity with the comet 1819 iii. of Pons having been pointed out by Winnecke in 1858; and it will come to perihelion again at the beginning of next December, but under unfavourable conditions for observation. To satisfy the observations in 1858, 1869, and 1875, Prof. Oppolzer finds that it is necessary either to diminish the mass of Jupiter by 1-1051th part, or to suppose an acceleration in the daily mean motion of 0″·01436 in a revolution, a result which agrees closely with that found previously from the observations of 1819. Modern determinations of Jupiter's mass confirm so exactly the adopted value found by Airy and Bessel, that the first hypothesis is in itself improbable, and further the observations are not well represented by it. Calling U the resistance of the medium (in terms of the unit defined by Encke, 'Astr. Nachr.' Vol. ix. No. 210, p. 332), Prof. Oppolzer finds, from the acceleration $\Delta\mu = 0''{\cdot}01436$, that $U = \frac{1}{930}$ in the case of Winnecke's comet, whilst Asten had found for Encke's comet $\Delta\mu = 0''{\cdot}1044$, giving $U = \frac{1}{862}$ ('Observatory,' Vol. i. p. 22), a remarkably close agreement. As regards Faye's comet, with a value $\frac{1}{900}$ for U, the acceleration would be only $\Delta\mu = 0''{\cdot}0026$, a very small quantity. The absence of any sensible accelerating effect in the case of Faye's comet therefore affords no argument against Encke's hypothesis of a resisting medium. With regard to the secular variation of the eccentricity, Prof. Oppolzer finds for Encke's comet $\Delta\phi = -3''{\cdot}68$ in one revolution (agreeing exactly with Asten's result from observation); for Winnecke's comet $\Delta\phi = -1''{\cdot}08$; for Faye's comet $\Delta\phi = -0''{\cdot}32$, where ϕ is the usual angle of eccentricity ($\sin\phi = e$).

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[*] Astronomische Nachrichten, Nos. 2314, 2319.

Prof. Oppolzer's second paper deals with the effect of a resisting medium on the comets of 1843 and 1880. He remarks that though the identity of the comets of 1843 and 1880 has been commonly asserted on the strength of the resemblance between their elements, yet there is this serious difficulty, that, according to Hubbard's calculations, it is impossible to reconcile the observations in 1843 with a period of 37 years on the theory of gravitation alone. Seeing that his investigations on the orbit of Winnecke's comet support Encke's hypothesis of a resisting medium (his latest calculations giving for $\Delta\mu$ 0''·01498, and for U $\frac{1}{891}$), he has tried whether this hypothesis would also remove the difficulties in the case of the comet of 1843, but has only obtained negative results. From formulæ in which the product of the resisting force by the small quantity $1-e$ is neglected, it appears that with the value $U=\frac{1}{880}$, the influence of the resisting medium during the period of observation in 1843 is almost insensible, and consequently that Encke's hypothesis will give no help towards the identification of the comets of 1843 and 1880, since Hubbard's investigation, which shows intolerable discordances with a period of 37 years, holds equally well whether the effect of a resisting medium be taken into account or not.

It might be urged against this conclusion that possibly a substantial increase of the constant of resistance U might produce accordance; but it may be easily shown that for this comet a substantial diminution of U must be accepted on other grounds. For on calculating the effect of a resisting medium with $U=\frac{1}{880}$, it appears that if the comet of 1843 had been moving in a parabola before its approach to the Sun in 1843, it would have had after its perihelion passage a period of about 24 years; and though this result is not irreconcilable with a period of 37 years, yet in the future there would be a rapid decrease, so that the next period would be only 10 years, and so on—results which are not supported by observations, and are incompatible with the conclusions founded on Hubbard's calculations. Satisfactory results are obtained by a diminution of U to the tenth part of the adopted value; and such an assumption is not to be considered inadmissible, since we may provisionally assume that the resistance is proportional directly to the cross section of the moving body and inversely to its mass. Thus, taking the densities of the different heavenly bodies to be the same (which, however, is certainly not the case), the influence of a resisting medium would be inversely proportional to the radius of the ball, and it would suffice to assume that the nucleus of the comet of 1843 had ten times the radius, or, with the same dimensions, ten times the density of Encke's and Winnecke's comets. This assumption of the relation between the value of U and the density and dimensions of the body is in accordance with the fact that so far no appreciable effect of the resisting medium on the planets has been observed, and on the other hand leads to the conclusion that cosmic matter in a state of very fine division will fall slowly towards the Sun's surface, and will reach it with a nearly constant and definite velocity, whatever be the velocity with which it came within the sphere of the Sun's attraction. But these last conclusions only hold in their extreme consequences if the resisting medium be considered to be continuous, and not if it be imagined to be very finely divided cosmical matter, the luminiferous ether being at the same time supposed to offer no resistance. We are almost driven to the hypothesis of discontinuity when we consider that, according to Schiaparelli's investigations, the meteor-swarms, which would probably have a very small radius in proportion to the nuclei of comets, move with nearly parabolic velocities. Certainly the resisting medium must be very rarefied in the regions in which the Earth moves. The numbers deduced by Schiaparelli from Schmidt's observations for the hourly frequency of meteor-showers would point to a smaller velocity; and it even seems to follow from them that in the region of the Earth the meteor-swarms rushing towards the Sun are more numerous than those moving away, which is what Encke's hypothesis would appear to require.

Prof. Oppolzer next proceeds to consider various forms of disturbing force which might explain the anomaly in the motion of Encke's comet, viz.:—(1) a force along the radius vector, whether connected with electricity, with heat, or with any thing else; (2) a force perpendicular to the radius vector; (3) a tangential force due to the resistance of a medium. The only assumption with regard to the form of the function is that the disturbing force must decidedly increase with approach to the Sun.

Taking the hypothesis of a radial disturbing force, a relation is found between $\Delta\phi$ and $\Delta\mu$, from which with Asten's value of $\Delta\mu$ for Encke's comet, $\Delta\phi = -4''{\cdot}21$, whilst Asten's value from observations is $\Delta\phi = -3''{\cdot}68 \pm 0''{\cdot}15$, showing a decided discordance between theory and observation. Further, to explain the acceleration, we must assume that the radial disturbing force is, at least partly, a function of the odd powers of the differential coefficients of the radius vector with respect to the time, an assumption which would lead to a finite velocity of propagation of the forces. This is an hypothesis which could hardly be accepted. Taking the modified law of attraction, in accordance with this hypothesis, to be $\frac{k^2}{r^2}(1 - \frac{dr}{dt} W)$, where W is a constant to be determined by observation of Encke's comet, and making the arbitrary assumption that W is nearly constant for all comets, the acceleration $\Delta\mu$ for Winnecke's comet would be $+0''{\cdot}0209$, with which the observed value $+0''{\cdot}0150$ might be reconciled by altering the assumed value of Jupiter's mass $(\frac{1}{1049})$ in the direction of Bessel's value. For Faye's comet $\Delta\mu$ would $= +0''{\cdot}0036$, a small quantity, but hardly so small as to escape observation. As regards the comet of 1843, if it had a parabolic orbit before its approach to the Sun, it would have had a period of 500 or 600 years a few days after perihelion passage. Though this result agrees with Hubbard's most probable ellipse, it does not help in identifying the comets of 1843 and 1880. On the whole it must be admitted that radial disturbing forces cannot be accepted with any probability as explaining the anomalies.

Since we have no instance of a displacement of the plane of the orbit of any comet, a normal force must be supposed to act in that plane; and Prof. Oppolzer shows that the same relation $\Delta\mu = -\frac{3k}{a^{\frac{1}{2}} \cos\phi} . \Delta\phi$ between $\Delta\mu$ and $\Delta\phi$ holds equally for a normal force in the orbit-plane and for a tangential force, this relation being quite independent of the form of the function expressing this force. He concludes that the empirical values of $\Delta\mu$ and $\Delta\phi$ accord completely with the hypothesis of a resisting medium, but that the form of the disturbing force is perfectly arbitrary. Amongst the various forms which may be suggested, that in which the density follows an exponential law, the resistance being proportional to the density and the square of the velocity, is specially considered; and from the accelerations for Encke's and Winnecke's comets the formula Tangential force $= -c^2 \times 1''\cdot555\ e^{-3\cdot1r}$ is deduced (e being the base of the Napierian logarithms). With this, the following values of the accelerations are obtained:—

Comet	Encke.	Winnecke.	Faye.
$\Delta\mu$............	$+0''\cdot1044$	$+0''\cdot0149$	$+0''\cdot0005$
$\Delta\phi$............	$-3''\cdot71$	$-1''\cdot19$	$-0''\cdot07$

This form for the resisting force has therefore the great advantage of making the effect on Faye's comet almost insensible. For the comet of 1843 it would reduce its orbit from a parabola to an ellipse with a period of 200,000 years. Prof. Oppolzer remarks, however, that, in view of the possibility that the constant $1''\cdot555$ in the above formula may have different values for different comets, the numerical results are somewhat illusory.

Prof. Oppolzer reserves for a future investigation the inquiry whether the enormous development of the tails of the comets of 1811, 1843, and 1880 might not afford an explanation of the anomalous motion of the first and of the identity of the two latter, remarking here, however, that the supposition of a difference between the centre of gravity of the comet and the observed nucleus is inadmissible on the generally accepted theory of the constitution of comets. Meanwhile he would recommend caution in accepting the identity of the comets of 1843 and 1880, as proved by the resemblance of their five elements, considering that in not a few instances comets known to be different describe very similar paths, coincidences for which plausible explanations may be easily found.

ARMAGH CATALOGUE OF STARS.—Dr. Robinson has published 'Places of 10,000 Stars, observed at the Armagh Observatory.' The stars are those of the fainter ones in the 'Histoire Céleste' (nearly all between the 6th and 7·5 mag.) which have not been recently observed at other observatories. The instruments at Armagh not being optically powerful enough for this work, the mural circle was furnished with a new telescope having an object-glass of seven inches aperture. The observations were all made by the Rev. Ch. Faris, in the years 1868–76, each star being observed four or five times. Only the mean results are given. (Trans. Roy. Dubl. Soc., new ser., Vol. 1. Pt. 9.)—*J. L. E. Dreyer.*

URANOMÉTRIE GÉNÉRALE.—The first volume of the second series of the 'Annales de l'Observatoire de Bruxelles' contains a 'Uranométrie Générale,' by M. Houzeau. During a stay in Jamaica the author resolved to form a new Uranometry, which should possess the advantage above all others hitherto published, that it was to be the work of a single individual, observing all the stars in both hemispheres visible to the naked eye within a short space of time. He accordingly commenced working on January 28, 1875, and the work was finished on February 28, 1876. He had first prepared maps, on which all the stars were plotted down without regard to magnitude; then every region was gone over two or three times, and the magnitudes carefully estimated, the six usual classes being used, but each class only divided into two halves. The atlas thus formed consists of five plates, on which the stars, their letters, the names and limits of the constellations (but not their figures), and the Milky Way are depicted. The latter has been attended to with great care, the deeper or paler hue of the light-green colour representing greater or lesser brightness. The equatoreal zone between ±45° is drawn on a cylindrical projection, the circles of R.A. and Decl. being equidistant parallel lines, and the two polar calots on a polar projection, the parallels being equidistant circles. The catalogue of stars for 1880 is divided into four sections, separated by the equator and the parallels of 45°. It contains 5719 stars. M. Houzeau has examined the distribution of the stars in four ways:—1, with respect to the solar equator; 2, with respect to the direction of the Sun's proper motion; 3, perpendicular to this direction; 4, with respect to the Milky Way. No law whatever was found in the first three ways; while the fourth mode of proceeding confirmed W. Struve's conclusion that the density of stellar layers parallel to the plane of the Milky Way decreases very regularly and gradually towards the poles of the latter.—*J. L. E. Dreyer.*

THE RECURRENCE OF SOLAR ECLIPSES*.—This paper by Prof.

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* Astronomical Papers for the use of the American Nautical Almanac, Vol. i. pt. i. Tables of Eclipses.

Newcomb presents what the author supposes to be "a new theory of the recurrence of solar eclipses founded on some hitherto unnoticed properties of the 18-year eclipse cycle." As a matter of fact, these properties have been known to astronomers since the times of the Chaldæans, though of late years they have been overlooked in the ordinary text-books, and it has been left for Prof. Newcomb to rediscover them. In the *Saros* or 18-year cycle, the Sun and Moon return very nearly together to the Moon's node, and they also return very nearly to the same anomaly. Thus 223 lunations correspond to 242·01 returns of the Moon to her node, to 238·99 returns to her apogee or perigee, and to 18 years and 10⅓ days, or very nearly 18 returns of the Sun to apogee or perigee. By virtue of the first relation, if the Sun and Moon were at the Node together, so that an eclipse took place, they would be together again very nearly at the Node at the end of 18 years 10⅓ days, and another eclipse would occur under similar circumstances. But the irregular motions of the Moon and Sun would disturb the recurrence of eclipses if it were not for the last two relations, by virtue of which the Moon and Sun return very nearly to the same anomaly. It is to these "two remarkable chance relations" that Prof. Newcomb calls attention. It would seem, however, at least doubtful whether the return to the Node at the end of 223 lunations does not rather deserve the title of a remarkable chance relation. For Ptolemy (Almagest, Book iv. Chap. ii.) refers to the *Saros* as a means of finding the length of the mean lunation, a matter of the greatest practical importance to the Greeks and other nations who reckoned by lunar months. He states that the ancient mathematicians "sought, in comparing observations of lunar eclipses, for the period which would contain always a constant number of lunations and a constant amount of movement in longitude. The more ancient estimated this time at 6585⅓ days, in which period they found that there were nearly 223 lunations completed, 239 returns of anomaly, 242 returns to the same latitude, 241 revolutions in longitude, and 10⅔ days beyond the 18 revolutions of the Sun, considering their restitution with reference to the fixed stars. They called this time periodic, as first restoring these different movements to their original state." Afterwards Hipparchus deduced a more exact period of 126,007 days and 1 hour, at the end of which the time of eclipses returns after a constant number of lunations and a constant movement of longitude, there being in this period 4267 lunations and 4573 returns to the same anomaly, but not a return to the same latitude. Hipparchus also deduced a period of 5458 lunations, equivalent to 5923 revolutions, with respect to the node, by comparing eclipses of the Moon exactly similar in size and duration.

These extracts suffice to show that the Greeks were fully alive to the importance of eliminating the effect of the irregular motions of the Moon and Sun, and that they sought carefully for periods which would satisfy this condition, and bring about an exact recurrence of eclipses.

In more modern times Halley called attention to the property of the *Saros*, which brings the Moon back again to the same anomaly, and pointed out that the error of the tables at any given date would recur at the end of 18 years and 10 days "under the like circumstances of the same distance of the Moon from the Sun and *Apogæon*;" so that the true place of the Moon could thus be predicted with greater accuracy ('Phil. Trans.' 1731, p. 187). The most recent account that we have met with of the various periods contained in the *Saros* is the very clear statement given by Delambre ('Astronomie,' Vol. ii. p. 320). It is curious that the important properties of the *Saros* should have been passed over unnoticed by modern writers, and they would seem to have been generally forgotten; for in two recent notices of Prof. Newcomb's 'Tables of Eclipses' which we have read, his theory of the recurrence of eclipses is spoken of as a new method.

There is another point of importance in Prof. Newcomb's paper, viz., that "owing to the mean retrocession of 28′ from the node in each cycle, the corresponding eclipses in successive cycles are subject to a progressive change." Thus all eclipses may be divided into sets, the successive eclipses of any one set (separated by intervals of 18 years) coming in at one pole of the Earth, and working off at the other pole after sixty or seventy cycles. Prof. Newcomb has utilized this circumstance by the formation of tables for the calculation of the approximate elements of any Solar Eclipse between B.C. 700 and A.D. 2300. The division of eclipses into sets and the progressive change in the successive eclipses have long been known; and the facts are very clearly stated at some length by Ferguson in his 'Astronomy.' He speaks of an eclipse as returning every 18 years for 70 or more periods, and gives tables showing the effect of the difference between the mean and true places of the Moon and Sun on the successive returns of the eclipse which first came on the Earth in A.D. 1295, and which will finally leave the Earth in A.D. 2611.

GREENWICH SPECTROSCOPIC AND PHOTOGRAPHIC RESULTS, 1878 AND 1879.—These have been published in advance of the respective annual volumes of Greenwich Observations, of which they are to form part. The Spectroscopic Results contain observations of Solar prominences and determinations of motions of stars in the line of sight (32 stars in 1878, and 17 in 1879) made with the "Half-prism" Spectroscope, and miscellaneous observations of the spectrum of the eclipsed Moon and of the spectra of comets. In the Photographic Results the areas, distances from Sun's centre, and position-angles are given for Spots and Faculæ, and the Heliographic longitudes and latitudes for Spots, all for the years 1878 and 1879. Tables are also given exhibiting the means of daily areas of umbræ, whole spots, and faculæ for each rotation of the Sun and for each year, and the mean heliographic latitude and mean distance from the equator of the spotted area for the whole range of years 1874 to 1879. The following are the results from the seven years' photographs, the means of daily areas for the years 1873 to 1877 being taken from the Astronomical Results, 1877:—

	Mean of Daily Areas.			Mean	Mean
	Umbræ.	Whole Spots.	Faculæ.	Hel. Lat.	Dist. from Equator.
1873 ..	116	678	2882	° ′	° ′
1874 ..	83	583	1095*	−3 2	10 53
1875 ..	45	255	475	+1 40	10 30
1876 ..	26	132	226	−4 14	11 19
1877 ..	20	92	170	−4 17	9 32
1878 ..	5	22	87	+7 0	7 13
1879 ..	10	45	163	−9 11	23 1
1880 ..	45	208	635	+6 41	20 23

The means for 1873 refer to the last six months, and for 1880 to the first five rotations (not yet published).

The photographs indicate that the minimum both for spots and faculæ occurred about 1879 January, there having been a complete absence of spots from 1878 Nov. 11 to 1879 April 8, and of faculæ from 1878 Dec. 13 to 1879 Feb. 8, and again from Feb. 19 to April 4. The rapid increase in the last quarter of 1879 is very marked and is accompanied by an increase in the mean distance from the Sun's equator, corresponding with the law that the spots are, on the whole, further from the equator at maximum than at minimum. The individual results show the increase in an equally striking manner. Thus the latitudes of spots range from 0° to 15° in 1878, and from 12° to 35° in the last four months of 1879 and in the first four of 1880.

WITH reference to Prof. Langley's remark ('Observatory,' No. 40, p. 504) that "till 1860 it was uncertain whether the protuberances belonged to the Sun or Moon," Sir G. B. Airy calls attention to his observations of the eclipse of 1851 (Mem. R. A. S. vol. xxi. p. 1, &c.), which showed that the Moon's disk in its passage over the Sun covered up prominences on one side and uncovered others on the other side, indicating that the prominences belonged to the Sun and not to the Moon. Drawings and diagrams are given which establish this fact in the clearest manner.

WE learn from 'Nature' that Faye's comet was picked up on Aug. 2 by Mr. Common with his large reflector, in the position given by Axel Möller's ephemeris. It is extremely faint.

EDITOR.

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* This result is too small, as many of the photographs taken during the early part of 1874 do not show the faculæ with sufficient distinctness to allow of their measurement. The mean daily area for the half-year beginning 1874, July 2, is 1257.

Elements and Ephemeris of Comet b 1880 (*Schäberle's*)*.

By M. Bigourdan.

The following elements have been deduced from three normal places formed from observations (1) on April 8, 10, 11, 13; (2) on April 26, 28, 29, 30; (3) on May 14, 16, 17, 18; and they satisfactorily represent all the observations from April 6 to May 18:—

Per. Pass. 1880, July 1^{d}·83846 Paris M.T.
Long. Per. 42° 30′ 56″·1 Inclination 123° 3′ 36″·1
Long. Node 257 15 13 ·3 Log. Per. Dist. 0·258474
Mean Equinox 1880·0.

Ephemeris for 12^{h} *Paris M.T.*

1880.	R.A.	Dec.	Log. Δ.	Brightness.
Sept. 2 ..	6^{h} 58^{m} 3^{s}·9	23° 31′ 35 N.	0·36709	0·83
4 ..	6 57 24 ·9	22 51 36	0·36183	0·84
6 ..	6 56 39 ·5	22 10 44	0·35641	0·85
8 ..	6 55 47 ·5	21 28 57	0·35083	0·86
10 ..	6 54 48 ·6	20 46 9	0·34509	0·88
12 ..	6 53 42 ·3	20 2 16	0·33921	0·89
14 ..	6 52 28 ·4	19 17 16	0·33319	0·90
16 ..	6 51 6 ·5	18 31 4	0·32705	0·92
Oct. 2 ..	6 34 35 ·0	11 29 39	0·27505	1·06
4 ..	6 31 42 ·4	10 29 35	0·26849	1·08
6 ..	6 28 37 ·6	9 27 43	0·26201	1·10
8 ..	6 25 20 ·1	8 24 3	0·25564	1·12
10 ..	6 21 49 ·6	7 18 36	0·24941	1·14
12 ..	6 18 5 ·9	6 11 21	0·24339	1·16
14 ..	6 14 8 ·8	5 2 24 N.	0·23759	1·17

The brightness at the date of discovery (April 6) is taken as unity. The comet is approaching the Earth, and receding slowly from the Sun, throughout September and October.

G.M.T. of Transits of Red Spot across Central Meridian of Jupiter.

	h		h		h
Sept. 1..	9·5	Sept. 11..	7·7	Sept. 21..	5·9
2..	15·3	12..	13·5	22..	11·7
3..	11·1	13..	9·3	23..	7·5
4..	7·0	14..	15·1	24..	13·3
5..	12·7	15..	10·9	25..	9·1
6..	8·6	16..	6·8	26..	14·9
7..	14·4	17..	12·6	27..	10·8
8..	10·2	18..	8·4	28..	6·6
9..	16·0	19..	14·2	29..	12·4
10..	11·8	20..	10·0	30..	8·2

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* Comptes Rendus Vol. xci. Nos. 2 and 3.

Astronomical Memoranda, 1880, *September*.

Sun. Sept. 1, sets 6^h 44^m, rises 17^h 16^m; Sept. 11, sets 6^h 22^m, rises 17^h 32^m; Sept. 21, sets 5^h 59^m, rises 17^h 48^m; Oct. 1, sets 5^h 36^m, rises 18^h 4^m.

Equation of Time:—Sun *before* Clock, Sept. 1, 0^m 17^s; Sept. 11, 3^m 38^s; Sept. 21, 7^m 10^s; Oct. 1, 10^m 31^s.

Sidereal Time at Mean Noon:—Sept. 1, 10^h $44^m{\cdot}1$; Sept. 11, 11^h $23^m{\cdot}5$; Sept. 21, 12^h $3^m{\cdot}0$; Oct. 1, 12^h $42^m{\cdot}4$.

Moon.	*rises.*			*sets.*			*rises.*	
	h	m		h	m		h	m
Sept. 1..	14	43	Sept. 11..	9	35	Sept. 21..	6	37
2..	15	54	12..	10	44	22..	7	6
3..	17	7	13..	12	3	23..	7	40
4..			14..	13	25	24..	8	24
5..			15..	14	48	25..	9	14
6..	*sets.*		16..	16	8	26..	10	14
7..	6	59	17..	*rises.*		27..	11	17
8..	7	23	18..	5	34	28..	12	26
9..	7	56	19..	5	54	29..	13	36
10..	8	39	20..	6	14	30..	14	48

New Moon, Sept, 4, 4^h 52^m; First Quarter, Sept. 11, 6^h 25^m; Full Moon, Sept. 18, 3^h 29^m; Last Quarter, Sept. 25, 23^h 9^m.

Mercury is a morning star in the E.N.E. till Sept. 16, when he is in superior conjunction.

Venus and *Mars* are both evening stars, setting soon after the Sun. They are in conjunction with each other on Sept. 7.

Jupiter is in Pisces, rising soon after sunset. Diameter:—Sept. 1, 44″·8; Oct. 1, 46″·8.

Sept. 1, R.A. 1^h $11^m{\cdot}1$, Dec. 5° 51′ N., tr. 14^h 25^m, rises 7^h 51^m
Oct. 1, 0 58 ·9 4 31 N., 12 14 5 48

Daily rate of rotation 870°·74 = 2^r 150°·74; Hourly rate 36°·28; Period $9^h{\cdot}922$. (Derived from Mr. Marth's Ephemeris for Physical Observations of Jupiter, 'Monthly Notices,' May 1880.)

Saturn is situated at about one third of the distance from α Piscium to β Arietis.

Sept. 1, R.A. 1^h $49^m{\cdot}9$, Dec. 8° 27′ N., tr. 15^h 3^m, rises 8^h 17^m
Oct. 1, 1 43 ·6 7 45 N., 12 59 6 16

	Outer Ring.		Inner Ring.		Ball.
	Maj. Axis.	Min. Axis.	Maj Axis.	Min. Axis.	Diam.
Sept. 11 ..	44″·00	11″·54	29″·26	7″·67	17″·6
Oct. 1 ..	44 ·83	11 ·36	29 ·81	7 ·55	18 ·0

The south side of the rings is visible, the elevation of the Earth above their plane being 15° 12′ S. on Sept. 11, and 14° 41′ S. on Oct. 1, and of the Sun 13° 56′ S. and 14° 12′ S.

Phenomena.

	G. M. T.		
	h	m	
Sept. 1	11	21	J. i Ec. D.
	14	24	J. i Oc. R.
2	9	29	J. i Tr. I.
	11	42	J. i Tr. E.
3	8	50	J. i Oc. R.
	11	17	J. iii Ec. D.
	13	48	J. iii Ec. R.
	14	48	J. iii Oc. D.
	16	56	J. iii Oc. R.
7	6		♀ ☌ ♂, ♀ 31′ N.
	11	11	J. ii Ec. D.
	15	15	J. ii Oc. R.
8	13	15	J. i Ec. D.
	16	10	J. i Oc. R.
9	9	31	J. ii Tr. E.
	11	14	J. i Tr. I.
	13	27	J. i Tr. E.
10	10	36	J. i Oc. R.
	15	19	J. iii Ec. D.
11	7	26	θ Ophiuchi Oc.* 184°.
13	7	14	50 Sagitt. Oc. D. 61°.
	9	42	[6671] Oc. D. 84°.
14	8	3	J. iii Tr. I.
	8	7	J. iii Sh. E.
	9	39	[7043] Oc. D. 73°.
	10	11	J. iii Tr. E.
	13	12	[7097] Oc. D. 102°.
	13	46	J. ii Ec. D.
15	15	10	J. i Ec. D.
16	9	12	J. ii Tr. I.
	11	49	J. ii Tr. E.
	12	58	J. i Tr. I.
	15	11	J. i Tr. E.
17	9	39	J. i Ec. D.
	12	20	J. i Oc. R.
Sept. 18	9	36	J. i Tr. E.
20	10	43	104 Piscium Oc. R. 330°.
21	9	28	J. iii Sh. I.
	11	21	J. iii Tr. I.
	12	7	J. iii Sh. E.
	12	51	[782] Oc. R. 224°.
	13	30	J. iii Tr. E.
	16	21	J. ii Ec. D.
22	10	33	τ′ Arietis Oc. R. 273°.
23	11	29	J. ii Tr. I.
	14	5	J. ii Tr. E.
	14	42	J. i Tr. I.
	16	54	J. i Tr. E.
24	11	34	J. i Ec. D.
	14	5	J. i Oc. R.
25	8	52	J. ii Oc. R.
	9	8	J. i Tr. I.
	11	20	J. i Tr. E.
	13	34	1 Gemin. Oc. R. 243°.
	17	3	3 Gemin. Oc. R. 267°.
	18	43	6 Gemin. Oc. R. 293°.
26	8	31	J. i Oc. R.
	11	33	*d* Gemin. Oc. R. 208°.
27	13	40	*g* Gemin. Oc.* 351°.
28	6		☿ ☌ ♂, ☿ 6′ S.
	13	30	J. iii Sh. I.
	14	37	J. iii Tr. I.
	16	8	J. iii Sh. E.
	16	48	J. iii Tr. E.
30	13	45	J. ii Tr. I.
	16	21	J. ii Tr. E.
	16	25	J. i Tr. I.

* A near approach.

The angles at Oc. D. and R. are reckoned from the *apparent* N. point towards the right of the Moon's inverted image.

EDITOR.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

No. 42. OCTOBER 1. 1880.

Practical Spectroscopy in 1880.—Part III.

GAS-VACUUM-TUBE SPECTRA.

BUT there is another class of electrical spectra not requiring the above expensive apparatus; and though, from the small size of its sparks (an inch long), it might be inserted between our columns of lamp-flame and 2-inch electric sparks uncondensed, yet it gives more spectral references than all the chemicals burned in both these other methods; while, at the same time, it opens up some magnificent views into both the effects of time and some of the conditions of worlds, stars, suns, and even the general nature of things. This class of spectra is that of the permanent gases enclosed in so-called "gas-vacuum tubes."

By being there almost infinitely attenuated in the vacuum-making process, the constitution of the gaseous residue may be more powerfully looked into, and more signally exercised by a very small electric force, than when in the denser condition of the pressure of the whole atmosphere; while, in so far as the substances examined are permanent gases, they are evidently always in the most suitable state for exact spectroscopy; which discriminates nothing, throughout all its panorama of coloured light, until the introduced substance has been by some means, though only temporary and evanescent, raised into a gaseous condition for the time.

Thanks, then, to the vacuum-tube of Geissler and Plücker, next after the homely facilities of lamp-flame chemical lines, we may now employ for spectrum reference place the three chief lines of Hydrogen—Red Hydrogen or H_α, Glaucous Hydrogen or H_β, and Violet Hydrogen or H_γ, as seen in any vacuum-tube of that gas when rendered incandescent by any, even the smallest possible, induction-spark; and very well, even most respectably, shown, by one mere pint-sized bichromate cell, and suitably little coil, producing a spark in free air of only 0·3 inch long. These three H lines too, though seen thus so easily at the lowest electric tension, have the further recommendation that they are equally or more visible in the condensed electric sparks, and are grand in the Sun itself as the lines C, F, and "near G," their places being thus in the several independently formed lists already referred to:—

Name of spectrum-line.	Spectrum-place in the catalogues of		
	Simple induction-sparks.	Condensed induction-sparks.	The Sun.
Red Hydrogen ……	38 707	38 707	38 706
Glaucous Hydrogen ..	52 255	52 252	52 254
Violet Hydrogen* ..	58 525	58 524	58 525

But it is a long way, "a far cry" from Red to Glaucous H; and how a previous lamp-flame-spectrum observer does miss his old accustomed reference the salt, or Solar D, line at W.N. place =43,108! That line's uniformity of presence in every thing burned in flame led that grand man of his time, the late Fox Talbot, in days preceding the rise of known spectrum analysis, to believe it must be a result of watery vapour in the air, in place of, as subsequently shown by Professor Swan, the presence everywhere of chloride of sodium salt—a material which chemists can keep out of nothing that they make; every thing earthly being salted with that salt, even down to the minutest motes in a sun-beam, or the dust-specks floating in the air.

Nevertheless the salt line is rarely seen in any vacuum-tube; its vapours apparently refuse to rise to the call of the simple electric spark, not even when that excellent workman, Herr Geissler kindly prepared for me, shortly before his death, two hydrogen tubes enclosing a considerable quantity of salt. The want, however, of a reference line about halfway between Red and Glaucous H, was supplied soon afterwards at home in Edinburgh, and in a far more legitimate manner; or thus:—

Finding that all M. Geissler's tubes gave only very faint spectra when viewed, as they must be, transversely to the line or length of their illuminated capillary, and that only a very small part of all that long line could be utilized at one time in the spectroscope, so that they were actually wasting $\frac{88}{100}$ of the light they gave out by sadly expensive galvanic-battery fluid consumption, I had other tubes prepared for my work by M. Salleron, of a shape to be viewed end-on, and thereby utilize in one little disk the whole of the light through all the length of the capillary. No sooner was this done, and the spectrum nicely focused, than the brighter view so obtained immediately revealed, besides the three, or even four, standard hydrogen lines looking brighter than ever, an immense number of what may be called low-temperature lines of hydrogen. Low temperature, because while so conspicuous here with uncondensed sparks of small size, they have not been noticed by any of the greater spectroscopists, who use terrific 6-inch sparks fearfully condensed; nor have they been identified in the superlatively heated Sun. And amongst these low-temperature H lines, all of them sharp, neat, admirable lines for mensuration purposes, some single, some double, and others grouped in varying ways, there is one of surpassing beauty—of so light and citron-coloured a yellow as in its superior brightness to impress the eye with the aspect of white; flanked, too, on either hand by three or four very thin lines, reminding one altogether of "Alcyone," the chief of the "Pleiades group of stars," and suggesting as the name for this line, and new reference, "Lucida H," its place being at W.N. = 43,698, or between the much missed sodium lines and that most inscrutable luminous phenomenon in Nature, the Aurora line at or near 45,640 W.N. place; and than which there is nothing more requiring a trustworthy spectrum-place reference in its immediate neighbourhood.

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* The fourth line of this series, discovered by Angström, is barely within range of uncondensed sparks, but may be described and placed thus:—"Lavender H or Solar *h* in W.N. place =61,935 by condensed spark, and 61,931 in the Sun."

Touching next on Oxygen, the end-on method of vacuum-tube shows us, *first*, all our hydrogen friends over again, though faintly, for they cannot apparently be kept out of the manufacture in more or less degree; and they are living there, happily in perfect peace, with the gas they are supposed to be always ready to explode with, on a light being applied; for the oxygen lines are there too. These oxygen lines are the four low-temperature lines well described by Dr. Schüster in the 'Transactions of the Royal Society of London,' two years ago; but supplemented now by three others of the same kind, which the end-on vision has yielded to the Edinburgh observers. So that an oxygen tube for place reference may afford any one the following convenient mile-stones (p. 558).

But the real spectrum of oxygen, say some of the chief authorities of the day, is the high-temperature, condensed-spark spectrum; and though three of *its* lines have, as above noted, some resemblance in place, they are not its chief lines; and the spectrum, take it all in all, is so entirely different as to have no less than sixty lines between 39,000 and 62,000 of W.N. place. Now we also allow that oxygen has, in good truth, that numerously-lined spectrum at high electric temperature; but then we also find this other spectrum at low temperature; and if it is not the spectrum of oxygen at that temperature, we should be very glad to be told what it is the spectrum of, simple or compound.

Meanwhile the seven chronicled lines remain at their posts, ready

Colour of oxygen line.	Intensity.	Wave-number place in		
		Simple spark.	Condensed spark.	The Sun.
Ultra red	1	32 585	..	0
Red	1·5	36 218	..	0
Scarlet-red	3	39 336	39 367?	0
Orange	5	41 254	..	0
Citron	3	46 729	..	0
Green	5	47 659	47 565 47 720 ?	0
Violet	6	58 156	58 176?	0

for the service of any spectroscopist in his daily work; and we pass on to consider the other great constituent of the atmosphere wherein the earth-globe is wrapt, and in which we live, and move, and have our being, viz. nitrogen; and the moment we look at it in the end-on tube spectroscope, oh! what a spectrum meets us there!

Hydrogen is, of course, present more or less as a most insinuating impurity, but it by no means prevents us from also seeing that the direct effects of nitrogen, under simple spark illumination, are to fill the whole universe *of the spectrum* with light; not in absolute continuity, but in closely following bands,—bands sharp on the left, fading off towards the right; and who shall recognize or distinguish one from another, when there are something like 170 of them?

Fortunately for our guidance, Nature herself has grouped most of them into quartets and triplets through all the red, orange, yellow, citron, and green regions, giving them, under low dispersion, a sort of corduroy effect; but after that, or in the subsequent parts of the spectrum in the direction of greater refrangibility, the bands stand single, far apart, like a row of blue and violet grenadiers, finishing off that end of the spectrum grandly. The previous red end of it begins, according to Thalén, with faint bands of haze, in about 36,970 W.N. place; but the real commencement is much earlier, according to our end-on tubes, or in 33,900 W.N. place; where, as Prof. Alex. Herschel has established in a paper now before the Royal Society of Edinburgh, there is something like an upthrow of four thin, sharp hard lines, and then follow the haze bands in quartet-groups, at the same interior intervals as the lines. And what exquisite things those bands of nitrogen are, when well examined through their brighter regions; so gauzy in texture, just resolving into linelets under high dispersion, but each of them beginning on the left with an exceedingly thin line,—

as good to those whose spectroscopes can show it as any of the orthodox, solitary, solid lines of spectrum-place reference,—until we come to the blue grenadiers, and then each of those bands begins with a Roman Lictor's bundle of thin lines, but offering also, in its central axis, an excellent reference for place. As these more refrangible bands, too, are so easy to separate one from another, and occur in a part of the spectrum where the other elements seldom make many distinct marks, we may as well give their places, *i. e.* the places of the centres of their leading staves, thus:—

The closing bands of Nitrogen's low-temperature spectrum.

The beginning of each.	Intensity.	Wave-number place.
Glaucous band	0·5	49 566
,, ,, ...	0·5	49 846
,, ,, ...	2·0	50 161
,, ,, ...	1·0	50 497
,, ,, ...	3·0	51 656
,, ,, ...	3.0	52 794
,, ,, ...	3.0	53 811
Blue band	3·0	54 456
,, ,, ...	4.0	54 655
,, ,, ...	4·0	55 556
,, ,, ...	4·0	56 607
Indigo band	4·0	57 530
Violet band	4·0	58 311
,, ,, ...	3·0	58 484
,, ,, ...	4·0	59 470
,, ,, ...	4·0	60 457
Lavender band	3·0	61 318
,, ,, ...	2·0	62 006
,, ,, ...	2·5	62 530
,, ,, ...	1·5	63 492
,, ,, ...	0·5	64 296

It is a pity that so important a reference spectrum, and which is to be seen so clearly in every nitrogen vacuum-tube, when illuminated by simple sparks, cannot be allowed to remain always attached by name to so distinguished a gas. But the exacting theories of some physicists have compelled them to raise a cry that it is not nitrogen at all, but an oxide of that gas, their reasons being, apparently, two—*first*, that a spectrum of bands is a characteristic of oxides generally; and, *second*, that the real spectrum of nitrogen is a totally different thing, or an arrangement of a few exceedingly bright lines, and comes out only with the condensed spark.

This latter part of the second objection is to a certain extent true, for every one allows that in the high temperature of condensed electric sparks the nitrogen spectrum is a linear one, exactly as described above; and so it appears, quite as a matter of course, whenever such great spectroscopists as Dr. Huggins, with his enormous batteries and Leyden jars in series, give us the spectrum either of air or of nitrogen. But that cannot limit what Nature has long ago appointed to nitrogen to show under other circumstances, when, too, it can no longer exhibit its high temperature lines; and as to the first objection, we are informed that metallic oxides usually turn their bands just the other way to those we have been describing, viz. with their intenser edges towards the right.

Chlorine, again, under the simple spark illumination, gives out at once a linear spectrum, of which the chief lines are placed nearly thus:—

Orange region	41 420 41 572 41 724
Citron region, a strong quartet, the third line the strongest, the fourth line the weakest	46 540 46 652 46 834 47 105
Green region, a grand double line; forming the α of the whole spectrum	48 647 48 680
Glaucous region	52 695 52 812 52 972

And Iodine makes an extraordinary tube; with more general luminosity than any other, if required for mere vulgar illuminating purposes; but in the spectrum, and low-temperature spark of course, giving a perfect superfluity of lines; for from red to violet there is nothing but lines, lines, lines—almost all of them, too, so very bright, that there is some difficulty in identifying any one in particular.

How different are the tubes containing Carbon in any of its vaporizable forms! That vaporability being an essential condition here; and, therefore, with this most refractory material, when required for the service of the low-temperature, simple, electric sparks which we are now confining ourselves to, we must have its nature modified beforehand, by being introduced into the tube as a compound gas, say either carbonic oxide, cyanogen, or olefiant gas. This, too, being quite a parallel example with that classic case of M. Lecoq de Boisbaudran in his examination of platinum; for his simple, uncondensed sparks, though two inches long, could develop nothing out of pure platinum wire; but the moment he tried the same spark on a chloride salt of the metal, a spectrum of platinum appeared immediately.

Proceeding, then, in this manner, *first*, on carbonic oxide, we find, after its hydrogen impurities, the carbon bands majestic—sharp on the left, and shading off through long spectral ranges on the right; but all the brighter part of the way resolving clearly into the sharpest and most exquisite linelets imaginable, and actually thousands in number. Some theorists, indeed, will still push their way in, and insist that it is not a spectrum of carbon, but of a compound; for the true carbon spectrum is one of eleven lines only, as seen in condensed 6-inch sparks every day. But as they cannot show that spectrum in our low-temperature tubes without introducing condensed sparks, and so raising the temperature, we doubt that theory; and trust we are justified in giving the following little table of "low-temperature carbon-band beginnings;" as a boon, too, for every wanderer in spectrum land, so often amongst other spectra will these bands be met:—

Carbon-band beginnings.	Intensity.	Wave-number place.
Red	1	36 970
Scarlet-red..............	3	38 357
Light red	3	40 327
Orange	4	41 789
Yellow	1	43 665
Citron	4	45 296
Green.................	1	47 059
"	5	48 874
Glaucous	1	50 648
"	3	52 550
"	1	54 076
Blue	4	56 331
Indigo	3	57 805
Violet.................	1	58 887
Lavender	1	60 347
"	2	61 486
"	1	62 627

With the *second* carbon compound, viz. cyanogen, or carbon and nitrogen, we have two gases, each of which yields bands at low temperature; so what a full spectrum they do make together—carbon bands and nitrogen bands intercalating with each other, overlying each other, but never mixing; and, though confusing enough to an unaccustomed eye, yet never altering the spectrum-places of their beginnings by the smallest quantity. But there seem also to be some lines peculiar to the compound—cyanogen, the most notable, being a grand violet line in 59,400, W.N. place; very nearly, but not exactly, the place of the chief line of the high-temperature spectrum of pure carbon, or 59,540, according to M. Thalén.

And, *third*, comes the compound of carbon with hydrogen, represented here by olefiant gas. In this spectrum, carbon bands appear, also pure hydrogen lines; but besides them there now come out most markedly the several mile-stone lines of each of the several bands of the compound "carbo-hydrogen," just as all the world has so long been acquainted with that spectrum in a blowpipe flame of coal-gas urged with atmospheric air. The first line of each of these groups being in W.N. place, the orange in 41,060, the citron in 45,086, the green in 49,182, and the blue in 53,630, they evidently have nothing to do with the pure carbon bands of the low-temperature, uncondensed electric spark given above.

THE UNEXPECTED.

Gentle reader! Imagine, if you please, now, that a couple of years have been spent in examining progressively tube after tube of the different gases; and then, after so appreciable an interval of time, we return to the first example, and go through all their spectra again.

Some, as the hydrogen, are exactly as they were before; but others are so altered, we can hardly recognize them! In some, the carbon bands are burned out, and the material apparently deposited as a dark stain on the inside of the bulbs; but what is this terrible alteration which confronts us in other spectra, and more especially in the tubes of nitrogen, a gas which ought to be such a good constant, as representing the chief part of the air we all breathe?

Its own exquisitely regular quartets of bands are only visible now through a few of the earlier red intervals, and are then lost amid anomalous thickets of hard, unyielding lines, multiplying in number through all the orange, yellow, and citron region. Attentive examination, however, presently shows that the new comers are all of them the low-temperature lines of hydrogen; and our "Lucida H" shines conspicuously among them. There is, indeed, now hardly a tube in my collection which does not show more hydrogen in it, on the second than the first examination; while as to the nitrogen tubes, the effect has been most markedly and regularly of this order, viz. that according to the length of time the electric spark has been passing through them, the nitrogen bands have been fading, and the hydrogen lines waxing in force.

To suggest that the electric current has been changing the supposed absolute element nitrogen into the older, undoubtedly absolute and independent element hydrogen, is, by itself, rather too strong a supposition to set before any one; yet look at what has eventuated, as an accomplished fact, a finished and perfect metamorphosis, in a Hydrochloric-acid tube. At first examination it showed all the chlorine lines well (and our table of them on p. 560 shows how distinct and characteristic they are), together with a little of carbon-bands, and a fair amount of hydrogen lines.

Now the carbon is gone, the chlorine has departed through every one of its lines, and the tube shows nothing whatever but hydrogen lines glowing in strength and brilliancy; indeed so strongly, that besides the three high-temperature lines before alluded to, it now shows the 4th also, or Lavender H, the solar *h* in the far-away and difficult spectral region of 61,932 W.N. place. And then, besides those few and far-between gigantic mile-stones of the spectral way, there are the hundreds and hundreds of the new, low-temperature lines of hydrogen vivid exceedingly. Compared by reference mirror against a pure and bright hydrogen tube, not one of all these lines is wanting, not one of them less bright in this tube, once a tube of good undoubted hydrochloric-acid, though now I can only call it a tube of artificial hydrogen; and I use it sometimes now indifferently for the genuine thing, it is so passing bright in all hydrogenous, so marvellously void of every other kind of lines.

Then what a disturbing idea seems to return upon us here with regard to nitrogen! Chlorine has been changed into hydrogen; and nitrogen, though more slowly, seems to be going the same way. Is nitrogen then, after all, not an element, but a compound? Is hydrogen, in that case, one of its components, and does long roasting in the induction-spark drive off the other element, whatever it may be, leaving the hydrogen alone and master of the situation? Or is nitrogen merely a particular form or state of hydrogen?

The Earth's atmosphere is not roasting yet in electrical heat through all its entirety; wherefore, if we may be permitted for a moment to use such a word, it shows us nitrogen existing through all its length, breadth, and height; but hydrogen appears nowhere, save when that uneasy gas will occasionally burst forth from crevices of rocks in coal-mines, struggling to be free and bounding forth to give mankind a foretaste of its eventual fiery quality. But what behold we in the Sun, where every thing has been roasting in supernal heat long enough?

The spectroscope shows us there a superabundance of hydrogen over all the solar surface and in the gaseous regions above the same; but no nitrogen either in lines or bands, either of its high or low temperature's spectrum. And on turning then to the nebulæ, those slowly coalescing materials just beginning to warm up for the formation of future Suns, the chief line, if we read Dr. Huggins's observations aright, is a nitrogen line; while hydrogen's most visible line, its *a*, or red hydrogen (so gloriously conspicuous in solar red prominences, explosions and disturbances, and also in all the intense cases of variable stars, when they suddenly blaze up at intervals, to the necessary destruction of all known forms of organic life) is entirely absent.

Of course the objection may be made that the mere two years of my experimental observations is nothing to the time the heat of the Sun has been enduring; so that even if the solar orb were proved to have turned all its once nebular supply of nitrogen into hydrogen, my two years of electric sparks could not have worked out any appreciable amount of such a change. But then it may be suggested that the excessive attenuation of any gas in a "vacuum-tube" gives the electric sparking an immense advantage in working out upon it its ultimate effects, the quantity of ponderable material being so vanishing a fraction.

Note, too, the converse of this case in our iodine tube. Every other tube of whatsoever gas that has altered its spectrum gave premonitory symptoms thereof by heating under the spark. But the tube which heated most of all, and became so hot after an hour's sparking through it that it could hardly be touched by the fingers, was the iodine tube; and there, in that tube, there is no change of the iodine lines, and only a barely appreciable increase of its originally very moderate amount of hydrogen *impurity* as I at first assumed it to be. Why, then, is there so little spectral change to show there, in return for so much heating?

Because that tube contains much *solid* iodine—a whole little heap of iodine particles like a pinch of snuff; or such a positive amount of heavy matter, that it may be ages before small low-temperature electric sparks can turn it all into hydrogen. I have, indeed, visions of possibly entering on new and varied experiments during the next winter, for testing this matter from a different side; but, meanwhile, the above is what has been actually arrived at, not only, as I believe, perfectly fairly, but utterly unexpectedly also. PIAZZI SMYTH.

15 Royal Terrace, Edinburgh,
1880, August 7.

On the present Proximity of Jupiter to the Earth, and on the Intervals of Recurrence of the same Phenomena.

IN the present year the linear distance of Jupiter from the Sun is smallest on September 26, with insensible variation for many days, and his linear distance from the Earth is smallest on October 6; and the extraordinary brilliancy of the planet accompanying these two proximities has been matter of universal remark. I have thought that it might be worth the trouble to point out some other years in which the same, or nearly the same, combination of circumstances will recur; and I introduce this by describing the means which I have used for making this prediction.

Some years ago, having often wished to possess the power of discovering the smallest integer numbers whose proportion might approach very near to that of any physical phenomena, I sketched a table, which was prepared by Mr. Ellis, and is now lodged among the manuscripts of the Royal Observatory. The fraction $\frac{m}{n}$ was exhibited (in its vulgar form) for every value of m below 100 and for every value of n below 100, in which m and n are prime to each other. The logarithm of every fraction was prepared; then the table was formed by arranging the fractions and their corresponding logarithms in the order of the magnitude of the logarithms. Thus, when a numerical proportion of any physical phenomena is given, expressed, perhaps, by many decimals, its logarithm is to be compared with the series of logarithms, in the table, which approach nearest to it in magnitude; and among the vulgar fractions which correspond to them will probably be found one or more expressed by few figures, which will be convenient for use.

The mean regular motion of the Earth in a Julian year is 360°·0075; that of Jupiter is 30°·3628. The logarithm of $\frac{\text{Earth's motion}}{\text{Jupiter's motion}}$ is 1·07397. I now refer to that portion of my table which is near to 1·07397. I find

$$\text{Log } \tfrac{47}{4} = 1{\cdot}07004$$
$$\text{Log } \tfrac{59}{5} = 1{\cdot}07188$$
$$\text{Log } \tfrac{71}{6} = 1{\cdot}07311$$
$$\text{Log } \tfrac{83}{7} = 1{\cdot}07398$$
$$\text{Log } \tfrac{95}{8} = 1{\cdot}07463$$
$$\text{Log } \tfrac{12}{1} = 1{\cdot}07918.$$

Any one of these fractions gives an approximate proportion of the mean movements of the Earth and Jupiter; and the first indicates that in 47 years, or when the Earth has described 47 circumferences, Jupiter has described so nearly 4 circumferences that we may properly examine how nearly the relative position of Jupiter and the Earth, after 47 years, agrees with their position at the beginning of that time; and so for the other fractions.

The annual movement of the Earth exceeds that of Jupiter by 329°·6447. Taking the multiple of this by each of the numerators of the fractions above,

In 12 years	the Earth has gained on Jupiter	3956°	= 11 × 360° − 4°
In 47 years	„ „ „ „	15493	= 43 × 360 + 13
In 59 years	„ „ „ „	19450	= 54 × 360 + 10
In 71 years	„ „ „ „	23405	= 65 × 360 + 5
In 83 years	„ „ „ „	27361	= 76 × 360 + 1
In 95 years	„ „ „ „	31316	= 87 × 360 + 4

In 12 years, therefore, the Earth must advance 4°, or nearly 4 days must elapse after opposition, before Jupiter is in the same part of his orbit as at the time of opposition in the present year. Similar remarks are easily applied to the other years.

Royal Observatory, Greenwich,
1880, Sept. 11.

G. B. Airy.

Total Eclipse of the Moon, 1880, *June* 22–23.

The total eclipse of June 22–23, 1880, was observed under the most favourable circumstances; the Moon was near the zenith, the air very clear and nearly calm. The power of the moonlight was very striking: about 9 P.M. I tried the smallest print in the 'Nautical Almanac' and could read it with ease, and the same with the daily papers; taking up, then, Roscoe's coloured pictures of spectra, I found all the shades of colour could be seen very satisfactorily. I mention these as rough tests of the light which I had no special apparatus ready for measuring. At $9^h\ 50^m$ the dark part of the penumbra was very clearly seen; it looked like a dark smoky brown or black shade, and with the telescope a great falling off in the brilliancy of the crater Aristarchus was noticeable. At $10^h\ 4^m$ several stars were in the field with B.A.C. 6161, which was near the limb of the Moon. A power of 500 on the large $11\frac{1}{2}$-inch telescope was used to observe the occultation; with this the star presented a hard round disk, and when the Moon's limb came up to it the star seemed to glide into the Moon, getting smaller as it did so, and then suddenly disappeared at $10^h\ 7^m\ 40^s.4$, Sydney mean time. The whole time of apparent contact with the limb could not have been more than 2 or 3 tenths of a second.

At $10^h\ 10^m$ the penumbral shade was very dark to the naked eye, and seemed to hide the N.E. side of the Moon. With the telescope it looked a smoky-brown colour. At $10^h\ 16^m$ one-third of the Moon seemed darkened by this shade; and at $10^h\ 19^m\ 36^s$ a still darker steel-blue shade appeared on the limb, marking the first contact with the real shadow; it was black to the naked eye, and even under the telescope blotted out most of the features of the Moon, proving that the shadow has a well-defined dark margin, as theory explains; five minutes later it could be seen that the inner part of the shadow was not so dark as the margin, and of quite a different colour. It was in fact a warm yellow, and gave much more light on the Moon than the margin. The reason for these remarks will appear subsequently.

At $10^h\ 20^m$ the shadow seemed to have a dark blue margin $1\frac{1}{2}$ minutes of arc wide, and the inner parts gradually got redder. At this time the width of the blue margin seemed very variable; at $10^h\ 30^m$ it was 3 minutes wide; three minutes later it was reduced to 1 minute. I could see no reason for these changes. They were the same on the bright parts as on the darker parts of the Moon; the colour kept uniform—a steely blue.

At $10^h\ 45^m$ the shadow reached the Mare Serenitatis, and the red parts of it were very conspicuous, the colour being not that of copper with which it was compared, but that of fully ripe oranges.

An 8th magnitude star was now noticed near the limb, and its occultation observed with a power of 100 on the large telescope; it appeared to glide a little way into the Moon, and then disappear, as if with a jump, at $10^h\ 50^m\ 43^s.9$. From this time onwards the red light seemed to get more conspicuous, and yet was so translucent, that all the conspicuous features of the Moon could be seen, even the markings on the inner walls of Aristarchus, and a multitude of small objects which may be said to be strewn over the lunar surface.

Another star now approached the Moon; its magnitude was about 10th, and with power of 100 it seemed to go out instantly as it touched the limb. The power was then increased to 250, and another 10 magnitude star watched during occultation; it seemed to glide a little way into the Moon, and instantly disappear at $11^{h}\ 14^{m}\ 14^{s}\cdot 2$; another star of 11½ magnitude seemed to go out instantly as it touched the limb, at $11^{h}\ 16^{m}\ 47^{s}\cdot 6$.

At $11^{h}\ 20^{m}$ the darkness at the edge of the shadow was much lighter than it had been, and the red more pronounced. It was compared with a scale of colours and found to be redder than orange-red, and certainly very unlike the colour of new copper. A watch was now kept for the reappearance of B.A.C. 6161, and the star was seen to emerge at $10^{h}\ 25^{m}\ 10^{s}\cdot 30$. It did not come out suddenly, but by degrees, yet the time of passage from first light to complete emersion was but a small fraction of a second.

At about $11^{h}\ 30^{m}\ 24^{s}$ (the time was not exactly noted) an 8th magnitude star was occulted; this seemed to hang on the limb in a striking way, as if part passed in and left a knob on the Moon's limb at the point of contact. The time of ingress was about two seconds by chronometer. Possibly the star is double, but too close to be seen with a low power.

At $11^{h}\ 36^{m}\ 30^{s}$ the eclipse became total, and the dark margin of the shadow passed off the Moon, or, rather, the Moon passed wholly into the shadow; but no one would have thought so, for the southern part for one-fifth of the diameter remained comparatively white, as if not eclipsed as viewed with the naked eye, and with the telescope was only faintly red. At this time the deepest colour was at the north point, thence towards the south point, gradually getting less.

The phenomena observed therefore go to prove that during this eclipse the extreme margin of the Earth's shadow cone was darker than any other part traversed by the Moon; and the part immediately within this nearly white, thence towards the centre gradually increasing in colour. At the middle of the eclipse the south point of the Moon was less than half her diameter from the centre of the shadow-cone.

During the totality I made a careful examination of the Moon with the spectroscope, but could not detect any absorption bands which are not present in the ordinary spectrum. The light, however, was so faint, even when concentrated by the 11½-inch telescope, that the spectrum could not be well seen. The only noteworthy fact was, that the red and violet, or ends of the spectrum, seemed to be cut off, as in the instance mentioned by Mr. Pratt (at Brighton) in his account of the total eclipse of August 1877. He observed with a small star-spectroscope, which, in all probability, had no means of determining the length of the spectrum. I observed with a spectroscope provided with measuring appliances, and, before the eclipse, I determined the length of the spectrum; and when during totality the very faint spectrum seemed to have the red and violet cut off, I measured the length, and, to my surprise, found it as long as in full moonlight. Examining more closely I could not see either red or violet; the reason of this probably was, that the whole spectrum was too faint to show the contrasted colours. At $12^h\ 13^m\ 18^s$ the total phase ended, and once more the dark margin of the shadow appeared.

At 12.20 this margin seemed 1½ minutes wide, while from this point the red was graduated to the darkest at the northern limb; and at 12.27 the north pole seemed the reddest place. At this time, also, I noticed that the dark margin seemed to hide more of the details of the Moon than did the deepest part of the red, and, judging by its appearance as it passed over bright and dark parts of the Moon, it was not an effect of contrast.

At $13^h\ 6^m$ the red was still visible to the naked eye, and, looking with the telescope at $13^h\ 11^m$, I noticed that the northern part of the blue margin for fully one-fourth of its length was as well-defined on the inside of the shadow as on the outside, and looked like a ribbon half a minute wide. At all other times the inner margin seemed diffused into the red. At this time the blue was not so dark as at the earlier stages, although the edge was quite as well defined.

At $13^h\ 30^m\ 12^s$ the last contact with the shadow was recorded, and the penumbral haze was not visible much longer.

During totality the Moon was very conspicuous in the sky, and might have provoked the question, what was the red light? if one had not known it was eclipsed; but a glance on the Earth showed almost total darkness, and the stars shone out as if there was no moonlight, which the spectroscope showed to be very nearly the case. A number of meteors were observed during the totality, principally in the east.

H. C. Russell.

The Multiple Star P xxiii. 100 (OΣ 496).

In 1845 Otto Struve suspected the existence of a close companion to the principal star of this group, and in 1851 measured it with the 15-inch refractor. Subsequently it was not seen when looked for, and, so far as I am aware, has never been seen since by any one, until measured by me a few nights ago with the 18½-in. refractor of the Dearborn Observatory. It was very plain and easy of measurement, and, under favourable circumstances, ought to be seen with a much smaller aperture, perhaps 8 inches. There is no evidence of any material change; and it is quite certain that the failures heretofore to see it were due to the atmospheric conditions.

Negative observations of double stars of this class have no significance unless coupled with an exact statement of the seeing at the time.

On the occasion of my measure the night was remarkably fine, and pairs of $0''\!\cdot\!3$ distance were very easy to observe, and good measures could be made where the distance did not exceed 0″·2. During the evening a number of very close and difficult pairs were measured, such as ξ Sagittæ, β Delphini, 20 Draconis, φ Draconis, &c. A few nights later, when the distant companions to P xxiii. 100 were measured, the OΣ star was wholly invisible, as it should have been under the circumstances.

In the following Table the close stars are designated in the usual way by the first four letters of the alphabet, and the distant companions by the small letters. For comparison, I give some of the prior measures with my own:—

	°	″		Mag.		
AB	336·8	1·51	1851·76	5·4	10·0	OΣ
	342·6	1·26	1880·62	6·0	10·5	β
CD	224·2	1.38	1849·64	7·4	8·9	OΣ
	223·8	1·39	1867·94	7·2	8·9	De
	225·3	1·36	1880·62	8·0	10·0	β
AC	269·2	76·10	1849·64	..	..	OΣ
	269·3	75·63	1868·68	..	..	De
	269·0	75·67	1880·64	..	..	β
A*a*	113·9	43·36	1869·76	..	9·7	De
	114·5	43·62	1880·64	..	10·5	β
A*b*	338·5	67·11	1869·76	..	9·2	De
	338·7	67·15	1880·64	..	10·5	β
bc	73·5	10·29	1869·76	..	9·6	De
	74·3	11·06	1880·64	..	10·5	β
C*d*	341·8	25·±	1841·81	..	..	Da
	336·8	27·37	1880·64	..	11·5	β

Evidently the components are relatively fixed. This group figures in several double-star catalogues as follows:—AC=Sh. 355 =OΣ (App.) 247; CD=Da 2; *bc*=H 1887; A*a*=H 1888; C*d*=H 1886.

S. W. Burnham.

Chicago, 1880, Aug. 25.

Double Stars for October.

The following pairs should be looked at and measured occasionally this apparition. 85 Pegasi is given, partly because of its importance, and partly because it is a most difficult object, and may be used as a severe test for large apertures, eye, and sky.

OΣ 456. R.A. 21^{h} 51^{m}·2, Dec. +51° 59′. Mag. 7·8, 8.

1847·7	25°·7	1″·35	OΣ.
1866·6	30 ·7	1 ·51	De.

OΣ 470. R.A. 22^{h} 17^{m}·4, Dec. +66° 22′. Mag. 7, 9·4.

1850·7	353°·5	3″·69	OΣ.
1867·5	357 ·5	3 ·76	De.

Σ 2934. R.A. 22^{h} 36^{m}·1, Dec. +20° 48′. Mag. 8, 9.

1830·1	187°·8	1″·22	Σ.
1874·8	162 ·0	1 ·15	W. & S.

Σ 2943. R.A. 22^{h} 41^{m}·3, Dec. −14° 41′. Mag. 6, 9·2.

1783·6	109°·9	35″·64	H.
1864·5	114 ·6	28 ·49	De.

OΣ 484. R.A. 22^{h} 52^{m}·5, Dec. +72° 12′. Mag. 7, 8, 11.

AB	1851·0	108°·5	0″·41	OΣ.
	1869·5	84±	Elongated	De.
AC	1855·5	255°·4	30″·72	OΣ

OΣ 536. R.A. 22^{h} 52^{m}·5, Dec. +8° 43′. Mag. 7·5, 7·8.

1852·6	345°·0	0″·25	OΣ.

Single in 1859 to OΣ, and to β in 1874, but measured by β in 1877, 161°·5, 0″·47.

OΣ 490. R.A. 23^{h} 4^{m}·9, Dec. +56° 47′. Mag. 7·2, 9·2.

1843·5	313°·7	1″·16	Mädler.
1868·2	303 ·1	1 ·56	De.

85 Pegasi. R.A. 23^{h} 55^{m}·9, Dec. +26° 27′. Mag. A 6, B 12, C 9.

AB	1878·7	274°·0	0″·67	β.
	1880·6	298 ·3	0 ·65	,,

B was discovered by β in 1878. A remarkable binary.

AC	1855·0	105°·0	30″·0	Argelander.
	1878·4	33 ·9	14 ·16	

A has a large proper motion.

Σ 3062. R.A. 23^{h} 59^{m}·9, Dec. +57° 46′. Mag. 7, 7·5.

A pretty binary star, well placed for observation. Schur's period is 112·6 years, Doberck's 104·4 years.

1877·7	297°·7	1″·43	Doberck.
1880·7	301 ·4	1 ·4	Gledhill.

J. Gledhill.

Mr. Edward Crossley's Observatory,
Bermerside, Halifax.

*Meteor Notes for October**.

Observations may be made in the mornings during the first half of the month and in the evenings during the period 1st–10th and 25th–31st. The Moon will be full on the morning of the 18th, so that the shower of Orionids may be observed only with difficulty this year. A few of the more conspicuous meteors will probably be seen, though the occurrence of fireballs is not a marked feature of the October meteor-stream. The proportion of streak-leaving meteors amongst the Orionids is very large, even exceeding that of the Perseids, though the members of the latter shower are finer generally than the former. Meteors are usually very abundant in October, perhaps more so than in any other month of the year, if we exclude special showers; and this abundance is due to the combined operation of a very large number of minor streams, which has the effect of raising the horary numbers to a figure beyond the experience of other months. The mornings of the 13th–15th may be mentioned particularly as likely to afford an unusual exhibition and to give the first intimation of the ensuing shower of Orionids.

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* See 'Observatory,' Vol. i. p. 188, Vol. ii. pp. 203–204, and Vol. iii. pp. 170–171, for further notes on October showers.

Large meteors should be expected on the nights of the 13th, 15th, 17th–18th, 22nd, 24th, and 29th.

A shower of slow brilliant meteors was observed on the 2nd in 1877, from 225° +52°, and on the 3rd–4th two active radiants were found at 133° +79° and 310° +77°, the former a very well-defined shower of swift streak-leaving meteors.

The radiant-point of the Perseids derived from Major Tupman's observations on August 9 was very sharply defined at 44° + 56° with a subradiant, equally well defined though shown by fewer paths, at 44° +60°. On August 11, 1879, Major Tupman found the normal radiant at 45° +56° from 14 or 15 Perseids, but "there was evidence of two centres separated 6° or 7° in declination." This corroborates observations at Bristol during the last few years. In 1878 a double radiant was found at 42½° +54° and 44°+ 59°. The weather was unfavourable in 1879, but the main radiant was determined at 46° +58°. This year the major shower derived from a mean of the several nights of observation was at 44° + 56°, with a distinctly marked, though apparently minor centre at 45° +60°. Several meteors with foreshortened tracks close to their radiants, and with motions in R.A., served to indicate the two points most accurately. It is therefore evident from recent observations (1878–80) of the Perseids that the radiant-point of the chief shower is at 44°–45° +56°–57° on August 10. All the best observations prove this conclusively, and confirm the position previously given by Mr. R. P. Greg, at 44° + 56°, from an average of many different results. And there is a subradiant at 44° + 60°, or the major radiant is elongated in declination. The chief centre advances in R.A. with each night of observation, for while on August 1 the meteors fall from χ Persei, the focus of divergence has moved to a point slightly below B–C Camelopardi on August 12. On August 16, 1877, it was seen at 60° +59°.

At Bristol the chief showers contemporary with the Perseids were observed at 30° +46°, 331° −9°, and 46° +27. The latter supplied some fine streak-leaving meteors and has already been pointed out as a prominent display at this epoch*.

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* Monthly Notices R.A.S. Vol. xl. p. 130 (January 1880).

Of the 254 Perseids observed by Mr. Corder at Writtle between July 29 to August 11, 190 left streaks and 37 were coloured. The summary of Mr. Corder's results in August is as follows:—

Aug. 1. Watched for 2½ hours, $9\frac{1}{2}^{h}$ to 12^{h}. Saw 29 meteors; of these 9 were Perseids from a well-marked radiant at 35° +56°. They were much longer and finer than those noted in July, and doubtless the first set of the true Perseids.

Aug. 2. 1½ hour, from $10\frac{1}{2}^{h}$ to $12\frac{1}{4}^{h}$. 21 meteors, including 7 Perseids.

Aug. 4. 2 hours, from 10^{h} to $12\frac{1}{2}^{h}$. 35 meteors, 13 Perseids. There seemed to be two radiants in Perseus, at 35° +56° and 46° +57°, and a few Cassiopeids indicated a centre at 15° +66°.

Aug. 6. ¼ hour, 10^{h} to $10\frac{1}{4}^{h}$. 5 meteors, 4 of which were Perseids.

Aug. 6. ½ hour, $14^{h}\ 10^{m}$ to $14^{h}\ 40^{m}$. 8 meteors, 2 Perseids, both 1st mag., yellow, with streaks. Radiant 43° +57°.

Aug. 8. 3 hours, 10^{h} to 13^{h}. 59 meteors; 26 Perseids, only two of which were 1st mag. A few well-observed paths gave the radiant near B–C Camelopardi 45° +59°, others may have been from a point N.W.

Aug. 9. 4¼ hours, 9^{h} to $13\frac{1}{4}^{h}$. 120 meteors, 84 Perseids; chief radiant at 45° +58°.

Aug. 10. Hazy and generally overcast. 9^{h} to 12^{h}, 46 meteors, 39 Perseids. The horary number was estimated at 40 with a clear sky, for during the first half-hour between 11^{h} and 12^{h}, which was almost cloudless, 21 meteors were counted, of which 18 were Perseids. At $11^{h}\ 23^{m}$ a fine meteor, probably = ♃, was seen, with an approximate path from 215° +65° to 216° +40°.

Aug. 11. 3 hours, 9^{h} to 13^{h}. 76 meteors, 50 Perseids. Sky clear with a little exception, but meteors scarce, the shower of Perseids evidently much falling off. Chief radiant at 46° +58°. The brightest meteor was = Sirius, at $12^{h}\ 57^{m}$, with a short path from 47° +47° to 45° +43°.

Mr. E. F. Sawyer, of Boston, Mass., writes that he made observations on Aug. 8th and 9th, but clouds interfered on the 10th and 11th. On the 8th the sky was watched from 9^{h} to 12^{h} and 37 meteors recorded, including 16 Perseids. There were other showers in Cepheus at 332° +60° (7 meteors), and N., of Cassiopeia, 5° +75° (6 meteors). On the 9th observations were made from 9^{h} to 13^{h} and 91 meteors seen, including 54 Perseids and 12 Cassiopeids. He estimates the horary number of Perseids on the 8th as 6, on the 9th 16, and of other meteors 9 and 11 respectively The magnitude of those seen on the 9th were:—

	= ♃ or ♀	> 1.	1.	2.	3	4.	5.	Total.
Perseids..	4	5	8	13	9	8	7	54
Others ..	1	0	4	2	7	11	12	37

The radiant-point of the Perseids was very exactly determined on the 9th by several short paths and a 1st-mag. meteor perfectly stationary at 44¾° +56¼°. A radiant in Cassiopeia was well defined at 5° +55°

(12 meteors). Mr. Sawyer also obtained a good observation of the July Aquariads.

A very fine meteor estimated as bright as ♀ was observed by Major Tupman at Cookham on August 12 $8^h\ 18^m$. It fell nearly vertically downwards in the N.W., where the twilight was still very strong. The path was inclined slightly from left to right; motion very slow. It disappeared behind a house, in azimuth N. 61° W., altitude 3°.

The same meteor was seen by Mr. Maunder at Greenwich, time $8^h\ 17^m$, 2 × ♀. Path began 5° from and below ζ Ursæ and ended 10° short of γ Leonis. It had a visible disk, with a slight tinge of green; train green, no streak. The diminution of speed as it approached the horizon was very marked.

At Writtle, Essex, a large meteor was recorded at about $8^h\ 15^m$, August 12, in the N. by W. It descended obliquely from left to right, from the direction of Arcturus to near the horizon, and could not have been a Perseid. It was daylight at the time, but the brightness of the meteor called the observer's attention to it.

These observations obviously relate to the same body, which appears, on projecting the described tracks, to have been a brilliant Aquariad, with a radiant somewhere near 328° −8°. The writer at Bristol observed a shower of meteors from 331° −9° on the nights of August 6–13 this year, which may have been the same; and Mr. Corder had determined a radiant at 331° −10° on July 29–30 ('Observatory,' Vol. iii. p. 537).

A large red fireball is described as passing over Winchester on July 22, at $14^h\ 24^m$. It was as big as a "good-sized cocoanut," and passed gently over the city in a diagonal direction before it finally burst. About 30 sec. afterwards there was a noise like a distant roll of thunder. The streets were brilliantly lit up for a moment, though the Moon was visible. W. F. Denning.

CORRESPONDENCE.

To the Editor of 'The Observatory.'

Visibility of Venus in the Daytime.

Sir,—

Some time ago, in a text-book on astronomy, I saw the fact stated that Venus could not be seen in the daytime, with the naked eye, when nearer than about a month before or after superior conjunction with the Sun. Believing that it *could* be seen when much nearer than that, I have taken the pains to test the correctness of this theory. The results are stated below.

My observations began in May, but I have omitted all previous to June 13.

June 13, $11^h\ 25^m$ A.M. Venus quite faint and sky poor. Apparent magnitude 5½.

June 14, 11^h A.M. Sky very clear. Venus about 3½ or 4th magnitude. Distance from Sun, according to 'Nautical Almanac,' 8¾°.

June 15, 11^h A.M. Sky poor, magnitude 5½.

June 16–18. Weather unfavourable.

June 19, 11^h 10^m A.M. Sky quite poor. Venus just visible. Distance from Sun 7° 20′.

June 20–29. Clouds and haze prevented any observations.

June 30–July 6. No observation possible on account of slight haziness of the sky.

July 13. Conjunction of Venus and Sun (superior).

July 15–24. Sky poor most of the time.

July 25, 10^h 20^m A.M. Venus easily seen; magnitude about 3½ to 4. Solar distance 3° 38′.

July 25, 0^h 40^m P.M. Sky perfect, but Venus not so easily seen as in the morning. Magnitude 4½.

Had the atmosphere been as pure on July 18 as it was on July 25, I have no doubt Venus would have been visible to the naked eye, although it was then only five days past superior conjunction and only 1⅔° from the Sun.

I saw Venus this morning, July 26, at 9.30 A.M., and also four other times subsequently, at intervals of half an hour to an hour. It was last visible at 0^h 40^m. Although not nearly so brilliant as yesterday, on account of poor sky, yet it is very easily distinguishable, much more so than Jupiter generally is in the daytime.

The latter is quite easily visible at present when his position is known.

I am, Sir, yours respectfully,
HARRY L. BALDWIN.

284 Kansas Avenue, Denver,
Colorado, 1880, July 26.

White Spot on Venus.

SIR,—

The interesting letter in your May number (No. 37) from Mr. Trouvelot in reference to the above has induced me to send you the following notes:—

"June 15, 1876. Observed Venus at 5^h 5^m P.M. Power 200, 11½-inch equatoreal, aperture reduced to 6 inches. Definition is bad, yet I can see a white patch on the south pole, very similar to that which was seen during the Transit of Venus in 1874; it is remarkable that it extends *on to* the disk and not outside of it."

The same white spot was seen many times in May and June 1876, and some measures taken, showing the extension of the cusps beyond the semicircle; on June 30 of that year the extension was 14° on each cusp, making the illuminated part cover 208° of the planet's limb; on several occasions the whole outline of the planet could be traced, but it was that of a dark body on a bright field; the part not illuminated in fact appeared darker than the sky background.

In reference to the red spot on Jupiter, I have made many drawings of it since I first saw it in 1876, then involved in the equatoreal belts, but of very much the same shape and colour that it presented in 1879.

I am, Sir, yours faithfully,
H. C. Russell.

Sydney Observatory,
1880, July 5.

Faye's Comet.

Sir,—

Faye's Comet was seen here with the 3-foot telescope on August 2, 8, 9, 10, and 12, the position by circle-reading agreeing with the ephemeris in 'Nature.' When first seen on August 2, 10^h, it was faint, round, and a little brighter towards the middle, and formed the point (N.E.) of a rough equilateral triangle, with two stars of about 10 mag. lying W. and S.E. in a field of 15′.

On the 4th (the night not being very fine) it had moved from this place, and a nebula *sf* some few minutes was mistaken for it. On the 8th, the night being fine, this last nebula was seen, as also another rather large and faint, about 5′ following the place of the comet on the 2nd, and much fainter. The comet's position was, by circle-reading, 23^h 16^m 0^s, +10° 47′, and there was another nebula very like the comet, and about 15′ preceding it.

On the 9th the comet was very close to a small star, so close that the edge of the comet must have overlapped the star. The brighter part of the comet was seen *sf* the star, and was noted as about 4″ distant, an estimation, I now think, too small. On the 10th and 12th it had moved in the line of the 8th and 9th, but more towards the preceding side, so as to be nearer to and N. of the nebula that preceded it on the 8th, and which it was, as already stated, then very like in point of size and brightness. On the 12th it is noted that the comet is like this nebula, *but* brighter in the middle. No measures were taken.

The comet was looked for on several nights since June 11, but the weather was not favourable. During the search several nebulæ not in Herschel's or Dreyer's Catalogues were found, besides those already mentioned as near the comet.

I am, Sir, yours faithfully,
A. Ainslie Common.

Ealing, Aug. 21, 1880.

P.S.—On Aug. 12, at 10^h 23^m, I looked to the north-west and saw a brilliant aurora; at 10^h 26^m it glowed like the setting Sun, getting very bright on the horizon under Ursa Major. At 10^h 28^m it became more diffused; at 10^h 31^m a ray shot up to Mizar, and was persistent for two minutes; after this it faded away. The colour was greenish yellow, the bands broad, extending up to 50° altitude, and the motion to the W.; the extent very nearly from N. to W.

The Aurora of August 12.

Sir,—

The finest display of the Aurora Borealis that I have ever seen was witnessed here last night. I was at the house of a friend in the early part of the evening, and a gentleman who came in said he had seen some extraordinary columns of red light, apparently rising from the Sun, which had fully set, and standing out on a yellow background. He described them as being about as "broad as a rainbow," four or five in number, and remaining unchanged while he looked at them.

As I walked up through the town, about half an hour afterwards, nothing noticeable was to be seen, but on going into the garden at about $10^h 30^m$ I saw at once a most magnificent aurora.

At first we saw a bright patch in the west, which sent up rays of light, constantly varying in length and intensity, and reaching at times nearly up to the zenith. Suddenly an arch of white light formed, reaching from about W.N.W. to nearly N., and extending in height nearly to the seven bright stars of the Greater Bear, which seemed to rest upon it. From this arch tongues of light shot upwards. Then rays from all parts of the horizon between W. and E.N.E. or E. rose up, moving slowly as if blown by a wind, and met near the centre of the cross of Cygnus, forming something like a tent of light over our heads. Near the southern edge of this "tent," and about parallel to it, ran a black rift all across the upper part of the heavens, the whole "tent" waxing and waning in brightness in regular pulsations, and occasionally waving as if blown by the wind.

At one time I was struck by a sort of crackling noise, which at first seemed connected with the Aurora; but on my paying attention to it, and listening carefully, it ceased, and I could hear nothing. Perhaps it might have been a rustling of the leaves on the trees, but it sounded much more like a succession of sparks from the conductor of an electrical machine.

The stars were wonderfully bright and shone clearly through the aurora, except in certain patches and streaks of special brightness, where they were outshone by its great light.

At about $11^h 0^m$ I had to leave for an hour, and on my return a few minutes before midnight the whole appearance had changed. We now saw a double arch of light, extending from W.N.W. to N., the lower arch being the most brilliant. At the north end it curved inwards and then outwards again, forming a smaller arch at that end. At the west end it doubled on itself and threw up a straight brilliant wisp of white light, forming a loop at that end of the arch. The whole of this appearance seemed to be very distant; and apparently much nearer to us, there was a quick throbbing of patches of light like summer lightning, flashing in 18 or 20 places simultaneously, and independent of the more distant tongues which arose from the arches of light, the more distant display being seen *through* the nearer one.

The whole of the light was white without any tinge of red; and I much regretted that I had no spectroscope or instruments of any kind at hand. I am, Sir, your obedient servant,

Banff, W. J. B. Richards.
Aug. 13, 1880.

Sir,—

A very fine Aurora commenced playing on August 12, 10^h 15^m. In one minute it completely spread over the northern sky, throwing up brilliant coruscations, a particularly bright one being observed over α and β Ursæ Majoris. The aurora extended from Arcturus to Capella and reached upward nearly to Polaris. It began to fade at 11^h and died out about 11^h 35^m. It was of a pure milk-white colour. There was a brilliant flight of meteors immediately before the Aurora began.

I am, Sir, yours faithfully,

Worcester, W. E. Cooper.
1880, August 14.

The August Meteors.

Sir,—

We observed the shower of Perseids on 5 days in July and August, and saw only 134 meteors, distributed thus:— July 26, 7 meteors; July 27, 19 meteors; July 29, 25 meteors; Aug. 8, 31 meteors; Aug. 9, 52 meteors. Of these 134 meteors 1 was as bright as Venus, 19 were of the first magnitude, 34 of the second, 34 of the third, 32 of the fourth, 10 of the fifth, and 4 of the sixth magnitude.

I myself observed also a few with the spectroscope as follows:—

Aug. 8 (No. 6 in the observing book). A very faint continuous spectrum without any lines; the blue and violet part of the pectrum was not visible at all. Magnitude 2.

Aug. 8 (No. 8). This spectrum also was very faint, but well observed. On the continuous spectrum the bright D line was visible, and no other. Magnitude 3.

Aug. 8 (No. 24). Spectrum very faint, without any line. Magnitude 2.

Aug. 9 (No. 38). The meteor was larger than Jupiter. The spectrum was splendid, from the extreme red to the violet. On the continuous spectrum I saw an extremely bright sodium line. Beside this splendid line the lines of lithium were also very well observable, as also many other metallic lines in the green and the blue portion of the spectrum.

I am, Sir, yours faithfully,

O' Gyalla Observatory, N. de Konkoly.
Hungary, 1880, Sept. 10.

Sir,—

On the night of August 10, while waiting for a higher altitude of Jupiter, I was struck with the appearance of a few meteors following in rather rapid succession, and counting the number during the time, $9^h\,30^m$ to $10^h\,30^m$, I made 39 in all. None of them exceeded the ordinary size. I should say about 12 of them were very small, with a short path. With the exception of 4 meteors which originated about and north-east of Polaris, having a nearly perpendicular path, the little inclination being to the west, all the others had paths parallel to each other and to that portion of the Galaxy south of Vega. Nearly all of them were to the east of Vega and confined to a space of two hours R.A.; the greater number arose about 25° less declination than the most southern of the principal stars of Cassiopeia, passing Vega about 20° to the east, and, as I said above, parallel with each other and with the Galaxy.

I am, Sir, yours truly,
JOHN ROBERTSON.

Coupar Angus,
1880, August 18.

SIR,—

On August 12 $8^h\,20^m$ a large meteor passed over Corona Borealis and ε Boötis, continuing about 10° west, when it suddenly vanished, leaving a luminous beaded trail. Although not yet dark it looked 10 times as brilliant as Jupiter when seen in opposition at midnight. The positions of the stars were taken afterwards.

I am, Sir, yours faithfully,
W. E. COOPER.

Worcester,
August 14, 1880.

The Colour of α Lyræ.

SIR,—

In the abstract of Prof. Holden's article on page 519 of the August number of 'The Observatory' the following statement occurs:—"(2) We do not find *isolated* stars of decided green, blue, or purple colours. A few such have been recorded, but in most cases erroneously;" with a reference to a footnote, which reads thus:—"For example, Admiral Smyth calls α Lyræ a 'green' star.—E. S. H." Surely there must be some mistake here. In the 'Bedford Catalogue' the colour of α Lyræ is described as "pale sapphire;" and I cannot find that Smyth has elsewhere called it "green." It would seem therefore that Prof. Holden's "example" is a singularly unfortunate one, as it is not Admiral Smyth who is in error, but the Professor himself!

As an instance of an isolated star of a decided green tint I may mention β Libræ, which is not inaptly called "pale emerald" by Smyth. The tint is so marked that the Rev. T. W. Webb, in his 'Celestial Objects,' states that the star is "inserted for its beautiful green hue;" and he kindly allows me to quote the two following extracts from his note-books respecting it:—

"1852, May 19. Beautiful pale green. 3·7 inches achromatic, power 80."

"1872, May 9. Fine pale green. 5·5 inches achromatic, power 65."

The pale green tint was distinctly marked when I observed the star with my 7½-inch refractor on August 10, though the star was three hours past meridian and the sky rather hazy.

At the close of the abstract referred to you remark "eye-estimations are notoriously untrustworthy." To a large extent perhaps this may be true. In colour-estimations one might expect to find strong marks of "personality;" and undoubtedly such are to be met with. For instance, I have been surprised that Struve does not appear to have remarked the decided blue tint of the companion of ν Ursæ Majoris, and that neither O. Struve nor Dembowski have noted the fine tint, a deep indigo-blue, of B of 68 Herculis. I have found, too, that on some nights (not always those on which definition was most perfect) the star-colours were more vivid than on others. Still, notwithstanding exceptions, there appears to be so fair an agreement in colour-estimations, even under a somewhat arbitrary and unscientific nomenclature, that the results of different observers are, on the whole, fairly comparable.

Perhaps some of your readers may be disposed, with your permission, to give the results of their experience in "Sidereal Chromatics." I am, Sir, yours faithfully,

Knowles Lodge, Cuckfield, **George Knott.**
1880, August 20.

On the Observation of Star-colours and the Colours of δ Cygni.

Sir,—

Having lately had to spend some little time in looking over the literature of Star-colours, and well knowing, in common with other observers, how difficult it is to estimate these colours in any useful way for want of a good method and special training, I venture to call the attention of observers to this subject. No doubt the abstract of Prof. Holden's paper which you gave in the 'Observatory' for August last, and the remarks made thereon, have already attracted the notice of many of your readers. Unfortunately I am able to do nothing more than call upon those who are interested in this matter, and who feel able to deal with its difficulties, to come foward either with some well-considered plan or at any rate with a little sound advice for the guidance of observers. For my own part I only know that it is only in good states of the sky that I can feel any satisfaction in attempting to form a judgment of the colours of double stars. And it is perhaps more than probable that those who use a bright field ought to allow the eye to rest at least some minutes before trying to record in a *dark* field the delicate colours of such objects.

All I can pretend to do here, then, is to say that I have no scheme to propose; and this arises from want of special training of eye, want of a standard scale of colours, tones, shades, &c., and want of a corresponding number of names for the colours, tones, shades, &c.

In other words these three things at least seem to be essential to the successful observation of the colours of the stars:—

1. A standard scale of colours, hues, tones, &c.
2. A corresponding series of colour-names.
3. A careful study of these colours and names.

Other important points might be named, *e. g.* the observer would have to test his eye for delicacy and colour-blindness; the instrument used might influence his estimates; the proper kind and intensity of light used in the observatory to illuminate the scale would have to be considered; the colour of the illuminated field of the telescope and its effect on the sensitiveness &c. of the eye would be another matter demanding consideration; in the case of double stars one should probably be put behind a bar in the field while the colour of the other was examined, and so on.

Surely some of your readers who are accomplished artists as well as practised observers could give a little help in this matter.

The Colours of δ Cygni.

Perhaps the following contribution to the observed colours of this fine star may be of use to your correspondent Mr. Kirk:—

Struve……	A subviridis, viridis; B cinerea, rubra.
Dembowski..	A blanche; B cendré.
Secchi……	A flava, subflava, alba. B subpurpurea, cærulea, violacea.
Dawes……	A white, yellowish, yellow, pale yellow, light yellow, white, white, pale yellow. B slightly blue, blue, blue, blue, blue, pale blue, purplish, blue.

I am, Sir, yours faithfully,
J. Gledhill.

Mr. E. Crossley's Observatory,
Bermerside, Halifax.

Smyth's "Comes" to Sirius.

Sir,—

In the last number of the 'Monthly Notices,' p. 514, Mr. Burnham has kindly mentioned my name in connection with Smyth's distant comes to Sirius; and as the printer has wrongly introduced a decimal point, thereby making nonsense of the context, I hope to be allowed to make a few remarks on the subject. The distance instead of being 10″·44, as printed, ought to be 1044″±.

Early last year a friend asked me to measure Smyth's comes. I could only find one star at all answering to Smyth's position-angle, other stars in the neighbourhood being only minute telescopic stars with my 8-inch refractor.

Three careful and accordant measures gave Position=47°·50. But, to my surprise, the distance was far too great for the screw of my micrometer, and I could only make a rough approximation by placing the "comb" parallel to and close to the line joining the two stars.

By counting the teeth (58) I made a rough distance = 1044″.

On a subsequent occasion, I fixed one micrometer-wire exactly at 30 revolutions from zero, and bisecting Sirius with it, found that I had to move the other wire more than 28 revolutions from zero in the opposite direction. The mean of 4 careful measures resulted in 58·748 Rev. = 1061″ +, Epoch 1880·14.

I regret that this more accurate measure was not made in time to be inserted in Mr. Burnham's paper.

In thus employing the wire micrometer in measuring distances for which it is not adapted, I only claim for these results that they show conclusively the distance is more than 1000″.

I made a careful diagram at the telescope of the principal stars in the neighbourhood of Sirius, and with a friend, a well-known F.R.A.S., compared my diagram with Smyth's original, in the Society's Library.

There are three stars in the original diagram (besides Sirius), and these I at once identified with three in my diagram, one of the three being the star measured above; and my friend said, no doubt I had measured Smyth's star. To conclude, I have taken up this subject with reluctance, as I ever look upon the 'Celestial Cycle' with feelings of the greatest respect.

I am, Sir, yours faithfully,
GEORGE HUNT.

Hopefield, West Dulwich,
1880, Sept. 3.

Meteorites and the Sun.

SIR,—

In the very able address of Prof. W. G. Adams to the Mathematical Section of the British Association, he calls attention incidentally to the occurrence of three magnetic metals, iron, nickel, and magnesium, alike in meteorites and on the Sun's surface, with the view of suggesting an explanation of the close relationship between terrestrial magnetic phenomena and the movement of the photosphere.

I am not aware that it has been pointed out, except by myself, that the connection between the material constituents of meteorites and of the Sun is much closer than this. Of the 16 metals which have been detected in the various classes of meteorites, 14 are known constituents of the solar envelopes, the two not so found (tin and lithium) being exceedingly unimportant components, by which I mean that they are found in only very minute quantities.

The 14 metals thus common to these bodies, moreover, comprise nearly the whole of the substances found in the Sun; and though most of them are plentifully distributed upon the Earth, it is singular, if the correspondence could be held to be merely accidental, that so rare a metal as titanium should appear in either list.

The remaining elements which go to make up meteoric masses are the metalloids oxygen, sulphur, silicon, chlorine, carbon, and phosphorus, none of which, of course, if we except the first, whose presence in the Sun is disputed, have been detected up to the present. That some or all of them do not exist in the Sun probably few will be disposed to assert, the opinion of spectroscopists being generally in favour of their presence in some more elementary form than that with which we are acquainted upon the Earth.

Whether we regard meteors as bodies that are slowly falling into the Sun, or as having been originally thrown out from that body in its frequent eruptions (an idea that was first suggested by Dr. Sorby), the above identity of material was to be anticipated, and it seems to me possible that it may even tend to support each of these hypotheses. At least it must be held satisfactory that the labours of chemists and spectroscopists are thus far mutually confirmatory. I am, Sir, yours faithfully,

JOHN I. PLUMMER.

Orwell Park Observatory,
1880, Sept. 20.

5 *Persei.*

SIR,—

I have recently detected a rather faint companion to 5 Persei, and made the following measures:—

P=275°·4	D=5″·32	1880·589
271 ·1	5 ·85	·592
274 ·2	5 ·62	·613
273 ·6	5 ·60	1880·60

The magnitude of the satellite is not less than 12·5 of the Struve scale. I am, Sir, yours faithfully,

S. W. BURNHAM.

Chicago,
1880, Aug. 29.

NOTES.

THE DOUBLE STAR 85 PEGASI *.—This star, remarkable for its large proper motion, was discovered by Mr. Burnham to be double in July 1878, and a second series of measures made about nine months after the first shows beyond all question that the two stars have a common proper motion, and are physically connected. The comparison stands thus:—

Pos. angle 274°·0	Distance 0″·67	1878.73
284·6	0·75	1879·46

indicating rapid orbital motion, though allowance must be made for uncertainty in the measure of such a difficult object. The annual proper motion of the large star is 1″·3 in the direction 138°; and

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* Astron. Nachr. No. 2319.

Mr. Burnham thinks this will prove to be one of the most interesting double-star systems known. There are but three stars of this class now known—85 Pegasi, η Piscium, and β Scorpii. The first is much the easiest of the three, but will require an aperture of at least 12 inches, as the small star is of about 12 mag. of Struve's scale. They were all discovered with the Chicago 18½-inch refractor.

OBLIQUITY OF THE ECLIPTIC.—A new determination of the inclination of the ecliptic is contained in a doctor-dissertation by E. F. van de Sande Bakhuyzen, of the Leiden Observatory[*]. The author discusses the declinations of the Sun observed at Leiden in the years 1864–76, and enters into all details relating to the instrument used, &c. He finds that the inclination for 1870 as given in Leverrier's tables should be diminished by about 0″·30. This result agrees well with Oppolzer's inclination for 1815 brought down to 1870, with a secular diminution as found by means of the latest values of the planetary masses.—*J. L. E. Dreyer.*

SPECULATIONS ON THE SOURCE OF METEORITES.—A paper by Dr. Ball, with this title, is published in the Proceedings of the Royal Irish Academy (2nd series, iii. p. 227). The author starts from the result of M. Tschermak's investigations, that meteorites have had a volcanic source on some celestial body. He shows, first, that they cannot have come from the Moon, as a projectile thrown from the Moon, and once missing the Earth, can never fall thereon, so that we should have to suppose the lunar volcanoes to be still in activity if the meteorites falling on the Earth at present should have come from the Moon. In order to get over the large initial velocity, which would be necessary to overcome the gravitation of a large planet, it seems then natural to inquire if a volcano on one of the minor planets could accomplish the task. Supposing a volcano of sufficient power (and if the projectile should cross the Earth's track, the initial velocity could not be much less than that required to throw a projectile away from the Earth) was placed on Ceres, for instance, would it be likely that a projectile driven therefrom would ever cross the Earth's track? If the total velocity with which it leaves the orbit of Ceres be less than eight miles per second, then the projectile will fall short of the Earth's track; on the other hand, if the total initial velocity exceeds sixteen miles, the orbit will be hyperbolic, and though it may cross the Earth's track once, it will never do so again. Taking a mean between these extreme velocities, suppose that a projectile is discharged from a point in the orbit of Ceres, with an initial velocity of twelve miles, in a random direction, what is the probability that its orbit shall cross the Earth's track? When this problem is solved in accordance with the calculus of probabilities, it is found that the chances against that occurrence are about 50,000 to 1. But while none of the other planets, large or small, are likely to be the source of the meteorites, one planet, the Earth itself, has special claims to consideration, as *every* projectile launched into space by primeval powerful volcanoes must continue to revolve round the Sun in orbits which cross the Earth's track in the point from which the projectile was originally discharged. At the point of crossing, of course a meeting of such a body and the Earth will often happen. Assuming, therefore, that the meteorites have had a quasi-volcanic origin on some celestial body, Dr. Ball is led to agree with those who believe this body to be the Earth.

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[*] Bepaling van de Helling der Ecliptica. Leiden, 1879, 8vo.

It is interesting to compare this result with the recent researches on the structure of meteorites by M. Meunier, who finds that the analogy between these bodies and the lower strata of our globe, which Daubrée already had pointed out, is not confined to mineralogical constitution, but is extended to the relation which these cosmical materials present when compared amongst themselves, as is done for the constituent rocks of the Earth*.—*J. L. E. Dreyer.*

Two new Minor Planets have been discovered—one (No. 217) by M. Coggia at Marseilles on Aug. 30, and the other (No. 218) by Herr Palisa at Pola on Sept. 4. EDITOR.

G.M.T. of Transits of Red Spot across Central Meridian of Jupiter.

	h		h		h
Oct. 1..	14·0	Oct. 11..	12·2	Oct. 21..	10·4
2..	9·9	12..	8·1	22..	6·3
3..	15·7	13..	13·9	23..	12·1
4..	11·5	14..	9·7	24..	7·9
5..	7·4	15..	15·5	25..	13·7
6..	13·1	16..	11·3	26..	9·5
7..	9·0	17..	7·2	27..	5·4
8..	14·8	18..	13·0	28..	11·2
9..	10·6	19..	8·8	29..	7·0
10..	6·5	20..	14·6	30..	12·8
				31..	8·6

Astronomical Memoranda, 1880, *October.*

Sun. Oct. 1, sets 5^h 37^m, rises 18^h 4^m; Oct. 11, sets 5^h 14^m, rises 18^h 21^m; Oct. 21, sets 4^h 53^m, rises 18^h 38^m; Oct. 31, sets 4^h 33^m, rises 18^h 56^m.

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* For these researches M. Meunier received the Lalande prize in 1879.

Equation of Time:—Sun *before* Clock, Oct. 1, $10^{m}\ 31^{s}$; Oct. 11, $13^{m}\ 21^{s}$; Oct. 21, $15^{m}\ 23^{s}$; Oct. 31, $16^{m}\ 18^{s}$.

Sidereal Time at Mean Noon:—Oct. 1, $12^{h}\ 42^{m}{\cdot}4$; Oct. 11, $13^{h}\ 21^{m}{\cdot}8$; Oct. 21, $14^{h}\ 1^{m}{\cdot}2$; Oct. 31, $14^{h}\ 40^{m}{\cdot}7$.

Moon.	*rises.*			*sets.*			*rises.*	
	h	m		h	m		h	m
Oct. 1..	16	1	Oct. 11..	11	13	Oct. 21..	6	18
2..	17	17	12..	12	33	22..	7	7
3..			13..	13	53	23..	8	3
4..			14..	15	11	24..	9	5
5..	*sets.*		15..	16	27	25..	10	11
6..	5	59	16..	17	42	26..	11	19
7..	6	39	17..	*rises.*		27..	12	28
8..	7	31	18..	4	39	28..	13	39
9..	8	37	19..	5	5	29..	14	53
10..	9	53	20..	5	38	30..	16	9
						31..	17	30

New Moon, Oct. 3, $16^{h}\ 43^{m}$; First Quarter, Oct. 10, $12^{h}\ 35^{m}$; Full Moon, Oct. 17, $16^{h}\ 26^{m}$; Last Quarter, Oct. 25, $19^{h}\ 0^{m}$.

Mercury is an evening star, setting soon after the Sun.

Venus is an evening star. Diameter:—Oct. 1, $10''{\cdot}6$, Oct. 31, $11''{\cdot}7$.

Oct. 1,	R.A. $13^{h}\ 52^{m}{\cdot}4$,	Dec. 11° 6′ S.,	tr. $1^{h}\ 10^{m}$,	sets $6^{h}\ 16^{m}$
31,	16 20 ·3	22 25 S.,	1 40	5 40

Mars is in conjunction with the Sun on Oct. 25.

Jupiter is in Pisces, in opposition on Oct. 7. Diameter:—Oct. 1, $46''{\cdot}8$; Oct. 31, $45''{\cdot}6$.

Oct. 1,	R.A. $0^{h}\ 58^{m}{\cdot}9$,	Dec. 4° 31′ N.,	tr. $12^{h}\ 14^{m}$
31,	0 44 ·4	3 5 N.,	10 2

Daily rate of rotation $870^{\circ}{\cdot}71 = 2^{r}\ 150^{\circ}{\cdot}71$; Hourly rate $36^{\circ}{\cdot}28$; Period $9^{h}{\cdot}923$.

Saturn is in opposition on Oct. 18, situated between Pisces and Aries, about 12° E. by N. of Jupiter.

Oct. 1,	R.A. $1^{h}\ 43^{m}{\cdot}6$,	Dec. 7° 45′ N.,	tr. $12^{h}\ 59^{m}$
31,	1 34 ·8	6 55 N.,	10 52

	Outer Ring.		Inner Ring.		Ball.
	Maj. Axis.	Min. Axis.	Maj. Axis.	Min. Axis.	Diam.
Oct. 1 ..	$44''{\cdot}83$	$11''{\cdot}36$	$29''{\cdot}81$	$7''{\cdot}55$	$18''{\cdot}0$
21 ..	45 ·06	10 ·96	29 ·97	7 ·29	18 ·0

The south side of the rings is visible, the elevation of the Earth above their plane being 14° 41′ S. on Oct. 1, and 14° 4′ S. on Oct. 21, and of the Sun 14° 12′ S. and 14° 29′ S.

Neptune may be found about 30′ S. of σ Arietis (R.A. $2^{h}\ 44^{m}{\cdot}8$, Dec. 14° 35′ N.).

Oct. 1,	R.A. $2^{h}\ 47^{m}{\cdot}6$,	Dec. 14° 15′ N.,	tr. 14^{h} 3
31,	2 44 ·6	14 0 N.,	12 2

Phenomena.

	G. M. T. h	m	
Oct. 1	13	29	J. i Ec. D.
	15	49	J. i Oc. R.
2	6	14	J. iii Oc. R.
	8	14	J. ii Ec. D.
	10	51	J. i Tr. I.
	11	5	J. ii Oc. R.
	13	4	J. i Tr. E.
3	7	57	J. i Ec. D.
	10	15	J. i Oc. R.
4	7	30	J. i Tr. E.
7	16	0	J. ii Tr. I.
8	15	20	J. i Oc. D.
9	7	15	J. iii Oc. D.
	9	50	J. iii Ec. R.
	10	40	J. ii Oc. D.
	12	35	J. i Tr. I.
	13	25	J. ii Ec. R.
	14	48	J. i Tr. E.
10	4	45	[6524] Oc. D. 64°.
	8	14	[6561] Oc. D. 171°.
	9	46	J. i Oc. D.
	12	3	J. i Ec. R.
11	7	0	J. i Tr. I.
	7	46	J. ii Tr. E.
	9	13	J. i Tr. E.
12	6	32	J. i Ec. R.
15	6	23	16 Pisc. Oc. D. 141°.
	12	55	19 Pisc. Oc. D. 115°.
16	7	9	45 Pisc. Oc.* 208°.
	10	30	J. iii Oc. D.
	12	53	J. ii Oc. D.
	13	51	J. iii Ec. R.
	14	18	J. i Tr. I.
	16	0	J. ii Ec. R.
	16	31	J. i Tr. E.
Oct. 17	11	30	J. i Oc. D.
	13	58	J. i Ec. R.
18	7	25	J. ii Tr. I.
	8	44	J. i Tr. I.
	10	2	J. ii Tr. E.
	10	57	J. i Tr. E.
19	5	56	J. i Oc. D.
	8	27	J. i Ec. R.
	17	3	ζ Ariet. Oc. D. 137°.
	17	56	ζ Ariet. Oc. R. 238°.
20	11	31	32 Tauri Oc. R. 266°.
21	6	57	τ Tauri Oc. D. 58°.
	7	38	τ Tauri Oc. R. 320°.
22	9	52	[1774] Oc. R. 250°.
	11	59	[1801] Oc. R. 291°.
23	13	46	J. iii Oc. D.
	15	7	J. ii Oc. D.
	16	2	J. i Tr. I.
24	13	14	J. i. Oc. D.
	15	53	J. i Ec. R.
25	9	42	J. ii Tr. I.
	10	28	J. i Tr. I.
	12	20	J. ii Tr. E.
	12	41	J. i Tr. E.
26	7	40	J. i Oc. D.
	10	22	J. i Ec. R.
27	5	58	J. iii Tr. E.
	7	7	J. i Tr. E.
	7	53	J. ii Ec. R.
29	16	32	*ε* Leo Oc. R. 279°.
31	15	0	J. i Oc. D.

* A near approach.

The angles at Oc. D. and R. are reckoned from the *apparent* N. point towards the right of the Moon's inverted image.

EDITOR.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

No. 43. NOVEMBER 1. 1880.

The late Mr. William Lassell, LL.D., F.R.S.

All life is full of teaching, by much of which we fail to profit; but there are few eyes so gross that they fail altogether to catch the beauty and the meaning of a well-spent human life. Such a life was that of Mr. William Lassell; and this simple record of it has been prepared in the hope that it may suggest (especially to astronomers) some valuable lessons, and also that it may serve in some sort as an expression of admiration and of deep regret.

The moral and intellectual parts of man's nature are curiously and closely connected. It is not always the case that in the individual both are equally worthy of admiration, and it may happen that the moral and intellectual parts of the individual, although both high, do not support each other. When, however, the moral and intellectual powers are equally high and work harmoniously in mutual support, we have an individual capable of advancing knowledge and of working for good in a high degree.

Astronomy is of purer eyes than to behold contentedly inferior natures. From immemorial sages, from Thales downwards, her children have been for the most part distinguished, not only for grandeur of intellect, but for the fine sympathy that understands an age's inarticulate cry for more light, for the courage that can face difficulties, for the eternal patience which loses not heart, and for the truthfulness and self-renunciation which are content to work for the work's sake. The calling of the astronomer is not an easy one; it is remote from the sympathy and comprehension of ordinary men, and is devoid of the showy qualities which command general attention.

The subject of this memoir, Mr. William Lassell, illustrates these remarks. His sympathy, honesty, quietness, constancy, and zeal, in combination with those intellectual gifts which constitute a keen observer, a faithful reasoner, an ingenious mechanist, all fitted him to be an observational astronomer. And these natural aptitudes carefully cultivated, fitted him to be an astronomical discoverer. For discoveries are not chance matters. Those who wish to discover must have their natural fitness developed by discipline. Original work must be prepared for with wise and patient energy.

Mr. Lassell was born at Bolton in Lancashire, June 18, 1799. Engaged in business, and with no large means, he began as early as 1820 to construct reflecting telescopes for himself.

How many distinguished men in early life have been beset by these difficulties of want of means and of time! Does not the struggle with such difficulties develop, as nothing else can, a thorough appreciation of the value of every opportunity, and a certain sublime strength to fight against heavy odds of circumstance? There can be little doubt on this point, and educationalists will do wisely to beware of making every thing too easy.

Here it may be remarked that the reflecting telescope, in the history of which the name of Lassell must always be associated with those of Herschel and the late Lord Rosse, is an exclusively British instrument in its invention and improvement, and that we owe the earlier reflecting telescopes to amateurs.

The first telescopes made by Mr. Lassell were a Newtonian of 7 inches diameter and a Gregorian of the same size. His next step was to make a Newtonian of 9 inches aperture of very great excellence. Equatoreal movement was given to this telescope in an ingenious and original manner, and the instrument on this account may be said to form an epoch in the history of reflectors (Memoirs Royal Astronomical Society, Vol. xii.).

In 1840 Mr. Lassell erected an observatory at his residence near Liverpool, happily named Starfield, in which the 9-inch Newtonian was mounted. With it he diligently observed, among other things detecting, without knowledge of its existence, the sixth star in the trapezium of Orion.

To the observatory at Starfield came often the late Mr. Dawes, whose delightful observatory books in my husband's possession (model observatory books in selection and fulness of detail, and in lucidness of expression and illustration) give many charming pictures of these astronomical friends in council. Many are the delicate tests for figure recorded to which the 9-inch was subjected, and many evidently were the discussions of observations and of performance of apparatus. Such sympathetic friendship must have been most useful and inspiring to both astronomers.

After a time Mr. Lassell conceived the bold idea of constructing a reflector of 2 feet aperture and 20 feet focal length, to be mounted on the same principle as the 9-inch Newtonian. Before beginning this work Mr. Lassell visited Lord Rosse at Birr Castle and satisfied himself as to the excellent performance of the 3-feet telescope. As a further preparation he contrived a polishing-machine to imitate as closely as possible those movements of the hand by which he had been accustomed to produce perfect surfaces on smaller specula. The essential difference between this machine and the polishing-machine of Lord Rosse, as stated by the Astronomer Royal, is that "in Lord Rosse's apparatus every stroke is very nearly straight, while in Mr. Lassell's apparatus there is no resemblance to a straight movement at any part of the stroke."

In the many contrivances connected with this machine and with the mountings of his telescope Mr. Lassell displayed that mechanical ingenuity and versatility which Bessel so pithily eulogizes in saying "an astronomer should be able to do something with a cart-wheel and a gun-barrel."

In carrying into practice the plans for the polishing-machine he was happy in having the assistance of Mr. Nasmyth, a true artist in such work.

Mr. Lassell's earlier speculum metal differed from that employed by Lord Rosse in that it contained a small quantity of white arsenic in addition to copper and tin. I believe, however, that he omitted the arsenic in the case of his 4-feet mirrors. He seems to have considered that the perfection of the speculum metal depended upon the accurate relative proportion of the copper and the tin, and that this could only be satisfactorily ascertained by a series of testings while the metal was in the pot. The polish and whiteness of his specula were remarkable. The writer has seen some of these specula which even after remaining untouched for twenty years looked as if they had just come from the workshop. With the 2-feet reflector Mr. Lassell discovered, September 1847, the satellite of Neptune, and in September 1848, simultaneously with Professor Bond in America, an 8th satellite of Saturn (Hyperion). In 1851, after long and careful search, he discovered two additional satellites of Uranus (Umbriel and Ariel) interior to the two discovered by Sir W. Herschel in 1787.

In 1852 Mr. Lassell took out this telescope to Malta and observed there through the winter of that year. Among the observations made, I would mention those of the nebula of Orion, and of various nebulæ and star-clusters, accompanied by careful drawings, which are recorded in the Memoirs of the Royal Astronomical Society (Vol. xxiii.).

Not remaining content with the 2-feet telescope Mr. Lassell undertook the construction of a much larger one, of 4 feet aperture and 37 feet focus, mounted equatoreally. This magnificent instrument was erected at Malta in 1861, and the work done with it by Mr. Lassell, assisted by Mr. A. Marth, during the next four years is fully described in the Memoirs of the Royal Astronomical Society (Vol. xxxvi.). It consists of numerous observations of the satellite of Neptune and of the satellites of Uranus, of observations of nebulæ and of the planets, and of a catalogue of the places of 600 new nebulæ discovered with the 4 feet at Malta. One cannot help regretting that the time spent with this telescope at Malta should have been so comparatively limited; for although it is wonderful how much British astronomers have done considering the unfavourableness of our climate for celestial observation, they are only too often obliged to "feed on glimpses" and to fill up long intervals between them by "guarding the Moon from wolves," like the officers of Queen Quintessence in the old French story. Had Mr. Lassell continued to observe at Malta, the discovery of the satellites of Mars might not have been left for America.

It is surely also to be regretted that the magnificent 4-feet telescope no longer exists.

After his return from Malta, Mr. Lassell purchased an estate at Maidenhead and erected there an observatory (in which he placed his 2-feet equatoreal) and an excellent workshop.

His experience in repolishing his 4-feet mirrors suggested to him some alterations in his polishing-machine. He carried out experiments in connection with these alterations in his workshop at Maidenhead and succeeded in constructing an improved form of polishing-machine, which is fully described and illustrated in the Transactions of the Royal Society (1874).

Owing to failing sight, Mr. Lassell has not observed of late years; but his interest in his favourite study never flagged. Notwithstanding his great age he was alive to the importance of *new departures* and sympathized much with the great development in astronomical physics which has taken place during the last twenty years.

Mr. Lassell died at Maidenhead, without much suffering, October 5th, in the 82nd year of his age.

Those who knew him mourn the loss of a valued friend, as well as of an astronomer.

In the calendar of science his name is entitled to an honourable place; and the story of his life may well stir many to new enterprise for the advancement of the world's learning.

In 1839 Mr. Lassell was elected a Fellow of the Royal Astronomical Society, received its gold medal in 1849 and in 1870 was elected its President. In 1849 he became a Fellow of the Royal Society, and received one of its Royal medals in 1858. He was also an honorary Fellow of the Royal Societies of Edinburgh and of Upsala, and received an honorary degree from the University of Cambridge.

MARGARET LINDSAY HUGGINS.

On the Great Southern Comet of 1880.

THE elements of the great comets of 1843 and 1880 are so nearly identical as to render it almost certain either that the latter was a return of the former, or that the two are fragments of one original comet. On the former hypothesis the perihelion passage of 1843 was probably the first, as it is incredible that frequent returns of so brilliant an object should have escaped observation.

The identity of the comet of 1843 with that of 370 B.C. was suggested by Valz and is regarded by Cooper* as highly probable. The elements of this ancient comet (assigned by Pingré from the account given by Aristotle) seem not inconsistent with such a supposition. This comet is said to have separated into two parts. Granting the truth of this statement, may not the periods of the separate comets have differed to such an extent that the times of perihelion passage are now 37 years apart? Prof. Hubbard's elaborate discussion of the observations of the comet of 1843 gave a period of 530 years—liable, however, to considerable uncertainty. The interval between B.C. 370 and A.D. 1843 is 2212 years. This is not a multiple of 530; but if we assume it equal to 5 periods of the comet we obtain for the interval between two consecutive perihelion passages 442 or 443 years. The last return previous to 1843 ought in this case to have occurred about 1401 or 1402. In November 1399 a brilliant comet was seen, and in 1402 (only a year from the hypothetical epoch) two comets appeared, the first on February 8, the second in June, both of extraordinary splendour, and both visible in the daytime.

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* Cooper's Cometic Orbits, p. 164.

The previous apparition, with the period assumed, was about 959. A comet was seen in that year from October 17 to November 1.

The year 519 was signalized by the appearance of a large comet. This was within two years of the date assigned by the hypothesis. For the return about A.D. 72 we have no recorded comet, or at least none whose path was nearly the same as that of 1843 I.

If the comets of 1843 and 1880 were both derived from that of 370 B.C., the mean period of Gould's comet has been 450 years, or between 7 and 8 years greater than that of the other fragment. The previous apparition would therefore have been due about 1430. Two comets are recorded in this, and one in the following year. A comet appeared also in the autumn of 981, very nearly at the epoch of the last preceding return. In 530 or 531 a large comet was visible in Europe, which Hind thinks was that of Halley. But "it is not impossible," Mr. Chambers remarks, "that there was a comet in each of the above years, a theory which might perhaps remove some of the discrepancies which exist on the assumption that there was but one." At the time for the apparition of 80 A.D. we have only the fact that in the year 79 "a comet was visible for a long time during the illness of Vespasian."

The perihelion distance of the comet of 370 B.C. was very small. Now it is sufficiently obvious, without any precise analysis, that in the case of a comet of small perihelion distance and considerable diameter, the mass, unless firmly held together by cohesive force, would be disrupted in perihelion by the difference between the Sun's attraction on the central and the superficial parts. The fragments would thus be compelled to move in somewhat different orbits, like the meteoric streams of August and November. The disruption, therefore, of Aristotle's comet, as affirmed by the Greek historian, is not in itself an improbable occurrence. Besides the separation of Biela's comet, other instances of a similar nature might readily be specified*. In short, either of the hypotheses above suggested seems less improbable than that such a comet as 1843 I. or 1880 I. should have made frequent returns in modern times without being observed.

Should Gould's comet return about 1916 or 1917 we may conclude that it is identical with that of 1843; if not, the hypothesis

* See the author's 'Comets and Meteors,' Chap. VI.

of a common origin at a remote epoch may be regarded as probable.

Bloomington, Indiana, U.S.A.
Sept. 1880.

DANIEL KIRKWOOD.

Spectroscopic Observation of Hartwig's Comet (1880 d).

1880, Sept. 30, 7^h 30^m. After receiving the telegram from the Academy of Vienna I found the comet after a short search for it. It appears in the telescope very bright, with distinct condensation and a train of nearly 2°. On finding it I looked at the spectrum with a small spectroscope by Merz, and as I saw that the well-known cometary spectral lines are bright enough, I changed the spectroscope for a larger one made with Browning's prisms, "à vision directe" by Heustren at Kiel, after the model of Prof. Vogel of Potsdam.

I saw three lines and a faint continuous spectrum. The lines were very diffused on both sides, and the relation between their 10 / light (beginning with that near the yellow) was as 3, ~~1~~, and 4.

I made 3 micrometrical measures of the position of the lines, the mean of which reduced to wave-length is:—

(1) 5610, (2) 5163, (3) 4856 tenth-metres.

The continuous spectrum was observed from 5628 to 4497 tenth-metres of wave-length. I remarked also a very faint line near the yellow, but so faint that it was not possible to make micrometrical measures of its position.

1880, October 1, 7^h 50^m. After setting the comet in the telescope I measured the position-angle of the train, which was very large, and obtained the following result:—$u = 129° \ 22'{\cdot}2$.

I then measured the lines in the spectrum with a spectroscope provided with an excellent Iceland-spar prism by Schmied and Haensch, of Berlin, and a fine bright-line micrometer by Mr. Browning. With these instruments I saw very well the 4th line, of which I had only suspected the presence the day before. From 5 measures of each line I found their positions:—

(1) 5610, (2) 5492, (3) 5163, (4) 4856.

The continuous spectrum was observed from 5708 to 4363 tenth-metres of wave-length.

After the spectroscopic observation I observed the comet's light with the polariscope. The first time I used a Savart polariscope, with an excellent green tourmaline by Steeg, of Homburg, but no results were obtained; with a much more sensitive polariscope, viz. that of Prof. Vogel (containing a Nicol and an oscillating quartz plate), the cometary light was found to be polarized. The polarization of the light was seen also by M. Kobold and M. Lakics.

I have also compared the spectral bands of the comet with a Geissler tube containing C_2H_2. The brightest line was not so refrangible as F, and coincided with the green line of hydrocarbon; that near the red end of the spectrum was much more refrangible than C, coinciding with the yellow line of C_2H_3; and, finally, the blue coincided with the brightest blue band of this gas.

1880, October 5, 8^h 40^m. Up to this day it was very cloudy, and it was not possible to observe the comet. It appeared much fainter than I expected, but only in the telescope. The spectrum was not at all fainter, because the condensation was more determined, and gave nearly the same spectrum. The train was still very small and the condensation was excentrical on the side of the Sun. During the observation, the comet occulted a star of 8–8·5 magnitude.

The appearance of the comet was very diffused and irregular; it was not possible to fix sharp limits to it in the telescope. Using an achromatic eyepiece magnifying 208 times, the comet was extremely granulated, but no nucleus was visible.

I observed the spectrum with the Iceland-spar prism spectroscope, and when the air was very steady I could measure all four lines in the comet's spectrum. Each band was measured four times with the micrometer.

It was very difficult to measure the faint line, which is the second from the red end, because on illuminating the micrometer the faint band disappeared. For the illumination of the micrometer, I used a Geissler tube of ordinary glass, or of uranium glass (green), containing nitrogen. The following are the results of the micrometer measures:—

(1) 5609, (2) 5492, (3) 5169, (4) 4859.

The continuous spectrum was also measured, and I found it from 5733 to 4315 tenth-metres of wave-length.

After the observation I made a few measures with the same prism on the brighter bands of the Geissler tube filled with C_2H_3, and found the lines corresponding to those of the comet spectrum as follows:—

(1) 5588, (2) 5492, (3) 5153, (4) 4859 tenth-metres.

The observations were made with an excellent reflecting telescope, by John Browning, of 10¼ inches aperture, mounted equatoreally (Lord Lindsay's model). But I found many inconveniences in the mounting, which I removed myself with my engineer. I made the clamp in R.A. and declination to act from the eyepiece-end of the telescope, and the motion independent of the clockwork to work with an endless line from the eye-end. The rotating telescope-body is also a serious difficulty. It is very hard to turn this in the winter, and as I turn it, the star does not remain in the field of view, because it requires much force to turn it. I got over this difficulty by adopting a toothed wheel on the cradle and a pinion on the telescope, turning it with a fine steel rod from the eye-end of the telescope. I adopted also an instantaneous gearing for the clockwork with the endless screw, so that I can with one line work without clockwork, and in a moment, clamping the clockwork with the screw, work with the independent motion line. It is very agreeable to be able to make transit observations with the instrument at one moment, and in another to put it in gear with the clockwork, and to make declination measures with the filar micrometer. Arranged in this manner, it is the most convenient instrument for work that I have ever seen.

O'-Gyalla Observatory (Hungary), 1880, October 16. NICOLAS DE KONKOLY.

The Advance of Astronomy *.—Part I.

ASTRONOMY, in some of its forms, reaches back to the most distant historical epochs, and the changes that it has undergone during this long lapse of time give to this science a peculiar interest. In no other branch of human knowledge have we such a long and continuous history of the search after truth, of the painful struggle through which men have passed in freeing themselves from theories approved by the wise of their own times, and in overthrowing beliefs which had become incorporated into the life and culture of those times. Perhaps the grand array of the heavens, and the vast phenomena which they display, naturally led men to the invention of complicated theories; but these passed away at last before the test of observation and the criticism of sceptical men; and the Copernican theory of our solar system, Kepler's laws of elliptical motion, and the Newtonian law of gravitation, gave to astronomy a real scientific character.

The discovery of the laws that govern the motions of the heavenly bodies, and the construction of the theory of these motions, demanded from practical astronomy better observations and a more accurate determination of the orbits of the planets and the Moon, or of the constants that enter into the problems of celestial mechanics; and this demand led to an improvement in the instruments and in the art of observing. The astronomers and instrument-makers of England and France led the way in these improvements. The great national observatories of those countries were established, and in England, Flamsteed and Sharp, Bird and Bradley, were foremost in raising practical astronomy to the condition of satisfying the demands of theory. But theoretical astronomy was soon to receive a wonderful advancement. Perhaps no one contributed more powerfully to this progress than Lagrange. The writings of this man are models of simplicity and elegance; and yet so complete and general are his investigations, that they contain the fundamental theorems of celestial mechanics. By the invention and perfection of the method of the variation of the arbitrary constants of a problem, and by the establishment of the differential equations of a planetary orbit depending on the partial differential coefficients of a single function, Lagrange reduced the question of perturbations to its simplest form, and gave the means of deducing easily the most interesting conclusions on the past and future condition of our solar system. To supplement this great theorist, there was needed another kind of genius. Combining the highest mathematical skill with unequalled sagacity and common sense in its application, Laplace gathered up and presented in a complete and practical form the whole theory of celestial mechanics. Besides his numerous and brilliant discoveries in theoretical astronomy, Laplace gave us some of the finest chapters ever written on the theory of attraction*, and a complete treatise on the calculus of probability.

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* Address of Prof. Asaph Hall to Section A of the American Association for the Advancement of Science, August 1880. Communicated by the Author.

By such labours as these, the questions of astronomy were brought into order and classified, and the attention of astronomers was directed better than ever before to the determination of the quantities which must be found from observation. Moreover, the refinement of analysis and the completion of theory brought out new and more delicate questions, not less interesting, and requiring more complete investigation and more powerful instruments. The careful examination and study of the instruments and methods of observation became necessary, as well as complete and rigorous methods of reduction; and finally there was needed a critical and satisfactory method for the discussion of observations. For these last improvements in astronomy we are indebted chiefly to the astronomers and mechanics of Germany..

Among those who contributed by means of their optical and mechanical skill to furnish astronomy with the instruments necessary for its further advancement, no one holds a more honourable place than Joseph Frauenhofer. This man began his scientific work at the age of twenty-two and died at thirty-nine; and yet in those seventeen years he gave to astronomy great improvements in the manufacture of optical glass, driving-clocks for equatoreals, and telescopes and micrometers, that in the hands of Bessel and Struve gave to observations a degree of accuracy hardly thought of before. To such men as Frauenhofer and his co-workers, who have carried on and improved the construction of instruments of precision, practical astronomy owes much; and yet, after all, the principal thing in a science is the man himself. No matter how excellent the instruments may be, the question whether they shall be used for the advancement of the science, and shall contribute the full value of their peculiarities to help towards increasing the accuracy of astronomical determinations, depends wholly on the astronomer. Again, astronomy is now so completely a science, and all its operations are so closely connected with theory, that no one is fit to have charge of an extended series of astronomical observations who has not a fair amount of theoretical knowledge. Without such knowledge his labour is apt to be thrown away, and is never so effective.

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* "Ein schönes Document der feinsten analytischen Kunst."—Gauss.

As a good example of what the modern astronomer should aim to be, we may take Bessel. To this man we owe a large part of our best methods for the examination and determination of the errors of our instruments, and the introduction of complete and rigorous methods for the reduction of observations. Bessel's reduction and discussion of Bradley's observations was a masterpiece of its kind, bringing out the value of Bradley's work, which had lain unnoticed for more than half a century, and forming a starting-point for sidereal astronomy. This work was continued and perfected in his tables for the reduction of astronomical observations, published twelve years afterwards—a work that has done more than any thing else to introduce order and system into practical astronomy. In the discussion of instruments and the determination of their errors, Bessel's conception of an instrument was that of a geometrical figure, and the positions of the lines and divisions of this instrument were considered with corresponding rigour. Although devoted almost entirely to astronomy, yet Bessel was an able mathematician, and of this he has left abundant proof. It seems to be necessary that a man should die and be forgotten personally before his work can be fairly estimated; but time adjusts these matters at last, and I know of no astronomer whose work promises to endure the judgment of the future better than that of F. W. Bessel.

It has been said that for producing the most puzzling compound of metaphysics and mathematics, something which has neither height nor depth, nor length nor breadth, and which no one can understand, the German mathematician is unequalled. And at the same time it must be said that, for clearness of conception, and beauty and precision of expression, Germany has produced in Gauss a mathematician who is unsurpassed, and who is worthy a place by the side of Lagrange. Omitting all reference to the works of Gauss in theoretical astronomy and in geodesy, which are many and important, I refer here only to his method for the discussion of observations, and of deducing the most probable values of our constants. Almost the entire work of astronomy is a vast system of numerical approximation, in which the first steps are obvious and easy, but where the theory soon becomes complicated and the labour enormous. Thus the calculation of the approximate orbit of a planet or of a comet is the work of only a few hours; but the computation of the perturbations, and the correction of the elements from all the observations, may be the work of months and years. It is therefore of the highest importance that we should have a method for the discussion of observations that will give us the best result, and which will introduce order and system into this department of astronomy. Such a method is that of least squares. For the complete theory of this method, and for nearly all the arrangements and algorithms necessary for its practical application, we are indebted to Gauss. The invention and application of this method to the discussion of observations of all kinds seems to me one of the greatest improvements of modern times, and its proper use will lead to a steady progress in astronomy. We must remember, however, that this method does not undertake the improvement of the observations themselves, as some have seemed to think; but, when rightly used, it produces simply the best result we can hope for from a given series of observations. It does not, therefore, dispense with skill and judgment on the part of the astronomer; but one is tempted to say that, if he has not these prime qualities, then the next best thing for him to have is the method of least squares. The use of this method has become one of the chief characteristics of modern astronomy; and if we compare the results of its application with those of the older methods, we shall see its superiority. Thus, for example, no astronomer of to-day, who is accustomed to the modern methods of discussion, would be satisfied with the manner in which Bouvard represents in his tables the observations of Jupiter and Saturn, but would suspect at once some error in his theory of the motions of these planets.

The present condition of astronomy is the result of the continued labours of our predecessors for many generations; and to this result the lapse of time itself has largely contributed. For the full development of the secular changes of our solar system, for an accurate knowledge of the proper motions of the stars of our sidereal universe, and of the great changes of light and heat that are going on among them, the astronomer must wait until future ages. It is his present duty to prepare for that future by making the observations and investigations of his own day in the best manner possible; and to do this needs a careful consideration of the present condition of the science. Although the objects for observation have become so numerous, and the range of investigation so wide, that there is room for the most varied talent and skill, yet there is danger that there may be a waste of labour, either in duplicating work or in doing it in an improper manner. Especially may this happen in observations of the principal planets of our system and of the fixed stars. In the case of the planets the observations are abundant, and the orbits are already well determined, except that of Neptune, for which, on account of its slow motion, we must of necessity wait for time to develop its small peculiarities, if such there be. For all these planets the observations at one or two observatories are amply sufficient, and even then the observations ought to be confined to a short time near the opposition, or at quadrature, and so made that they may be easily combined into a single normal position, which will suffice for the theoretical astronomer. To scatter such observations over a period of several months is to throw away one's labour, and to leave to the computer the disagreeable duty of rejecting a part of the observations as useless. It seems to me, therefore, unwise for several observatories to continue heaping up observations of the four outer planets of our system, when ten observations a year of each planet will give all the data that are needed. Again, for all the principal planets, observation is now in advance of theory, except, perhaps, in the case of one or two of them. Thus, for Saturn, all the tables are decidedly in error, and, although an attempt has been made to accuse the observations of this planet, it is quite certain that the trouble lies in the theory; for in the case of Jupiter and Saturn we have the most complicated planetary theory of our system, and one that has not yet been completely developed. It seems to me also that observations of our Moon might well be confined to one or two observatories. Here again observation is far in advance of theory, if indeed there be now in use anywhere a pure lunar theory. All the lunar ephemerides that we have are affected with empirical terms, and the lunar theory itself remains an unsolved mystery. In this case there is no attempt to impeach the observations. The trouble seems to be with the perturbations of long period, and this does not call for numerous observations during each lunation. By a proper consideration of these matters astronomers may, I think, save themselves much useless labour.

Observations of the fixed stars are of the utmost importance in astronomy, since the positions of the stars are the fundamental points on which depends our knowledge of the motions of the planets, the Moon, and of the stars themselves; and it is on account of this fact that Bessel's tables, published in 1830, were of such great service, since they introduced correct and elegant methods of reduction, and clearly defined all the constants and epochs. We now have the positions of several hundred stars so well known that they may be safely used in the reduction of observations; and for these accurate positions we are largely indebted to the astronomers of the Pulkowa Observatory, who have made such absolute determinations a special work. There is still an opportunity for the improvement of these positions, and every well-executed determination will be of value; but it is doubtful if crude and irregular observations can add any thing to our knowledge of the positions of these stars. Neither can the routine, mechanical style of observing, that is apt to prevail in large observatories, be of much use here. It would be better in most cases for such observatories to assume the positions of the fundamental stars, and to leave the further improvement of their places to skilful astronomers who understand the theory of such work, and who carefully study and become masters of their instruments. In these refined observations the refraction of light by our atmosphere also plays an important part, and this question will need to be examined at every observatory that undertakes to do independent work. It is true that every new and good meridian instrument may, and perhaps ought, to contribute something towards removing constant errors, and giving us a more accurate knowledge of a star's position; but when this position is very well known, the only way for further improvement is through complete and careful observations, and their thorough reduction and discussion.

In the observations of double stars but little had been done before the present century, and the labours of W. Struve form the real starting-point in this branch of astronomy. These labours have been ably continued by his son, the present Director of the Pulkowa Observatory, and the observations of these two astronomers, extending over a period of nearly sixty years, are of the greatest value for our knowledge of the motions of the double stars. This is a branch of the science into which irregular workers are apt to enter, and where some of them have done good service; but if any amateur astronomer will compare his own work with that of the Struves, and will study the methods followed by them in determining their personal and instrumental errors, and will emulate the steadiness with which they have followed out their purpose, he can do much to enhance the value of his labour. Here the observations are simple and easily reduced, and the chief requisites are skill and patience on the part of the observer. He should not be discouraged because he obtains no immediate or great reward for his work, or public notice, or because some one who rants about the nebular hypothesis and kindred subjects of which he knows nothing is for a time the great astronomer of the day. The observer will learn finally that a good observation of the smallest double star, or of the faintest comet or asteroid, is worth more than all such vague talk. The observation has a positive value, however small, but the physical theories of the universe, of which modern popular science is so productive, are generally worse than useless.

The first step towards a rational and trustworthy knowledge of our sidereal universe must come from a determination of the distances of the stars. The solution of this problem was attempted soon after the Copernican theory of our solar system was established, when it was seen that we have a long base line for our measures, or the diameter of the earth's orbit, and it was supposed that the solution would be easy. These early trials were all failures; but they led to some very interesting and important discoveries, such as Bradley's discovery of the aberration of light; to the knowledge of the fact that the determination of the parallaxes, or the distances of the stars, although simple in theory, is practically a difficult question; and then to an improvement in the instrumental means of observation, to a careful study of the methods of observation and the instruments, and to a recognition of the necessity of a complete and rigorous reduction of the observations. An examination of these early attempts is an instructive study. It is only about forty years ago that the solution of this problem was at last attained, and then only by the application of the most powerful instruments and the best observing skill. An interesting result of the determinations of stellar parallax is obtained at once in the check it puts on speculations concerning the structure of the sidereal universe. The first astronomers who considered the parallaxes of the stars very naturally assumed that the bright stars are nearer to us than the faint ones, and therefore they observed the bright stars for parallax. Now, while this assumption may be true as a general statement, the actual determinations of parallax show that some of the faint stars which are not visible to the naked eye are much nearer to us than the brightest stars of our northern sky. Again it was assumed that a large proper motion is a certain index of a star's nearness to us; but observation shows that this also may be an erroneous assumption. This is a problem whose solution is only just begun, but already we know enough of its difficulties to see that we need the most powerful micrometrical apparatus that can be brought into use. The invention of some micrometer that, while as accurate as the present filar micrometer, would give the observer a much greater range of observation, and enable him to select suitable stars of comparison, is something much to be desired. At present the heliometer seems to be the best instrument for observations of this kind. Formerly it was thought that photography would furnish a good method for such delicate determinations; but so far the photographic methods have not given the necessary degree of accuracy in the measurements, and the astronomical use of photography is confined mostly to descriptive astronomy, where, especially in solar eclipses, it has rendered excellent service. Closely connected with the parallaxes of the stars and their proper motions is the interesting question of determining their motions to or from our sun according to the theory of Doppler. Here likewise the numerical determinations are so discordant that we cannot have much confidence in the results. In both these cases we need more powerful apparatus, and a complete and thorough investigation of the methods of observation. Perhaps some of the large instruments now constructing may be employed in these methods, and we may soon have better results.

A great advance has been made in cataloguing the fainter stars. This work was begun by the French astronomers nearly a century ago, and was continued by Bessel, Argelander, and others. An important step towards the completion of this work was taken by Argelander and his assistants in their great catalogue of the approximate positions of 324,198 stars, which was finished in 1861. This census of the stars will soon be extended, we hope, over the whole heavens; and it already forms the groundwork for the great zone observations of stars now going on in Europe and in this country, and which must be nearly finished. These observations will doubtless reveal many interesting cases of the proper motion of the stars, and will certainly form the basis for a knowledge of the motion of our solar system in space, and for sidereal astronomy generally, such as we have never had before. Our American observatories can render a good service by observing stars of southern declination, since our observatories are ten or twelve degrees further south than those of Europe, and thus have an advantage of position which ought to be made use of, and which may serve to unite into a harmonious system the observations made in the northern and southern hemispheres. The work of mapping the very faint stars near the ecliptic has also been greatly extended, and it is to this extension that we owe the rapid increase in the number of the small planets between Mars and Jupiter. But besides aiding in the discovery of the asteroids, accurate charts of the small stars have a permanent value in giving us a knowledge of the heavens at their epoch, and also some idea of the distribution of the stars in space.

It is an interesting question whether, among the thousands of nebulæ that are scattered over the heavens, any of them show changes of form or of brightness. These objects seem to be at least as distant as the stars, and as they have sometimes an area of several degrees, they must be bodies of an enormous extent. That changes are going on in these bodies seems probable, but to be visible at such distances the changes must be very great. In this case there is need of much caution in the discussion of the drawings made at different epochs and by different astronomers with telescopes of different power; since the nebulæ change their appearance with the telescope used, with different conditions of the air, and with a variation of their altitude above the horizon. Here the excellent photometers that have been recently invented, and which are being so well applied to the determination of the brightness of the stars, may give us assistance. Perhaps also new drawings of the nebulæ, and their criticism and discussion, and a full recognition of the difficulties of making such drawings, will soon lead to a decision of the question of their change of form. Since the study of the light of the stars with new and improved photometers has now become a speciality, we may look for more exact and continued observations of the variable stars. This is a matter of which we know but little, and it is one where a persevering observer may do good service. Although he may not find any immediate encouragement in the discovery of remarkable relations among these stars, or the probable cause of their variability, he will be collecting observations that must form the test of every theory. As examples of the result of intelligent and persevering observation, we have the case of the sun-spots, which led directly to the discovery of their period and its singular variability; and that of the shooting-stars, which has shown us a very curious relation between these meteors and the comets, and one which may open to us the most extensive views of the relations between our own solar system and other systems in space.

Asaph Hall.

[To be continued.]

The variable (?) *Star h No. 78 near the Trapezium of Orion.*

In the years 1848, 1850–52, 1857–64 the Bonds (W. C. and G. P.), using the 15-in. refractor of the Harvard University, examined this and the neighbouring small stars on very many occasions, and were quite convinced of its variability. The detailed observations are given in the 'Annals of the Astronomical Observatory of Harvard College,' Vol. v. 1867. The star is there called "Sir John Herschel's star," "*h*, No. 78" [Sir John's number for it in his Cape Catalogue]. The observations may be thus summarized:—The star *was seen* once in Feb. and six times in March 1850; once in March 1858; twice in Jan. and once in April 1864; it *was not seen* on two nights in Feb. 1850, on one night in Jan. and one in Feb. 1851, on one night in Feb. and one in March 1859, on one night in Jan., one in Feb., one in March 1861, on one night in Feb. 1862, on one in Jan., one in Feb., and two in Dec. 1863, on three in Jan., one in Feb., two in March, and one in April 1864, "under circumstances which seem to leave no doubt as to its invisibility from faintness; and that it disappeared between the dates 1850, Feb. 1 and Feb. 7, reappearing again before March 2, and continued visible until the 15th of the same month; that between Dec. 30, 1863, and Jan. 3, 1864, it reappeared, disappearing before the 17th of the latter month; and that, invisible April 14, 1864, it was plainly seen on April 16."

For comparison with these, O. Struve's dates may be given:—

At Poulkova, with the 15-in. refractor, the star was visible on March 24, 1857; Feb. 23 and Oct. 28, 1858; Feb. 24, 1859; March 6, 1862: decidedly invisible Oct. 30, 1856; March 18, Sept. 2 and 24, Oct. 4, 1857; Feb. 1, 1858; Feb. 27 and 28, 1859; March 5, 1860; March 9, 15, and 27, April 4, Sept. 27, 1861; March 21, 1862.

A comparison of dates shows that while the star was "invisible at Cambridge on Feb. 23, 1859, it was visible at Poulkova on Feb. 24, and again disappeared before the 27th of the same month."

The kind of attention which was bestowed on the observations by the Bonds will be seen from such expressions as the following, extracted from the note-books:—"Very fine vision; the small stars easily seen, but the Herschel star has disappeared." "Bad seeing, but the variable star is now visible wherever the air is not violently disturbed." "Very fine seeing, and the '*h*' star (variable) certainly not to be seen;" "I have been in the constant practice of looking for it (1864, April 16)."

"1864, March 14. No '*h*' star since Jan. 4th, though always looked for."

The following extracts from O. Struve's 'Observations de la Grande Nébuleuse d'Orion' will enable the reader to see how carefully and under what circumstances *h* No. 78 was watched:—

"1857, Mars 18, 7^h 30^m. Aucune trace de 78."

"1858, Févr. 1. Par un ciel imparfaitement transparent etc., aucune trace de 78."

"1858, Févr. 23, 5^h 35^m. Images ondulantes," and "1859, Févr. 24. Très-mauvaises images." Yet 78 was seen on both nights.

"1859, Févr. 27, 7^h. Bonnes images. 78 invisible."

"1861, Mars 15, $6^h\,40^m$. Bonnes images. Aucune trace de 78."

"1861, Mars 27, $8^h\,10^m$–$9^h\,10^m$. Images tranquilles. Aucune trace de 78."

"1861, Avr. 4, $9^h\,15^m$. Par un ciel extraordinairement transparent, 78 invisible."

"78 enfin est d'ordinaire invisible chez nous et ne se montre distinctement que peu de jours de suite." And the limits of the changes are given as 12·5 mag. to invisibility.

The meridian altitude of the nebula at Poulkova is about 25°, at Cambridge (U. S.) about 42°, and at the Cape of Good Hope 60°.

The position of this small star will be readily understood from the following description:—Draw a line from γ to δ of the trapezium and produce it as far again; about this point, just near the edge of the nebulous matter, but really in the dark space beyond, *h* No. 78 will be found. It will be remembered that γ and δ are the two following stars of the trapezium, γ being the more northern of the two.

It may be asked why special attention is drawn at the present time to an object so difficult as "No. 78." Perhaps the following reasons may be deemed sufficient justification:—Firstly, all the stars, whether large or small, which lie apparently in or near the wonderful nebula of Orion will probably for a long time to come be extremely interesting objects to the student of sidereal astronomy, and accurate information respecting their places, proper motions, orbital motions, variability, &c. cannot fail to be most valuable. Secondly, such observations, if made by different observers in different climates, will enable astronomers to estimate more correctly than has hitherto been possible the value to be assigned to such work in this and similar cases. In other words, before much reliance can be placed in the results of observations of such difficult objects we must have many series by competent observers working with suitable instruments under different conditions as to climate. Thirdly, it is well known that the Orion nebula is being systematically studied at Washington with the great 26-inch refractor; simultaneous observations at other places would have, therefore, special interest and value. Fourthly, by careful observation, measurement, and drawing it may be hoped that ere long many of the strange discrepancies which are revealed by a comparison of the drawings and descriptions given by Sir W. and Sir John Herschel, Lamont, Cooper, Secchi, Rosse, &c. may receive a complete explanation. Lastly, this faint star is not an unsuitable test of the performance of large telescopes as regards light; a glance at this object would also enable the observer to form a fair opinion as to *quality* of the night for certain kinds of work.

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Three Test-objects for Telescopes of Large Aperture.

It is needless to insist on the convenience of having well-recognized objects suitable for testing the definition and light-gathering powers of the large refractors and reflectors which are almost yearly being constructed, either by the professional optician or the wealthy and skilled amateur. No doubt many such test-objects have already been selected; but it cannot be said that all of them have a special interest independently of their extreme faintness. Of at least one of the three objects described below it may be safely said that it will probably prove one of the most interesting to the student of binary stars. As will be seen, it is not that the stars alluded to are of the most extreme faintness which renders them so difficult and so valuable as tests, but rather that they are faint stars in more or less close proximity to bright ones.

85 Pegasi.

R.A. $23^{\mathrm{h}}\ 55^{\mathrm{m}}{\cdot}9$, Dec. +26° 27′. Mag. A 6, B 9, C 12.

The wide pair A C is interesting from the fact that Brünnow has used C for the purpose of finding the proper motion of the principal star. The following will sufficiently describe this pair:—

A C	77°·0	16″·00	1870·00	Brünnow.
	49 ·8	14 ·00	1877·94	Burnham.
	29 ·1	15 ·21	1879·61	„

B was detected by Burnham; and his measures are:—

A B	274°·0	0″·67	1878·73	Burnham.
	284· 6	0 ·75	1879·46	„

The discoverer of this difficult object says it "will require an aperture of at least twelve inches to show it." And it may of course be added that other essential conditions of success in dealing with it will be a fine sky and a high power.

η Piscium.

R.A. $1^{\mathrm{h}}\ 25^{\mathrm{m}}{\cdot}1$, Dec. +14° 44′. Mag. A 4, B 11.

Of this star Mr. Burnham writes:—"There is no known pair among stars of this magnitude, or brighter, with so close and minute a companion. I found the highest power necessary to show it satisfactorily....The small star appeared decidedly blue."

12°·9	1″·02	1878·7	Burnham.

β Scorpii.

R.A. $15^{\mathrm{h}}\ 58^{\mathrm{m}}{\cdot}5$, Dec. −19° 58′. Mag. A 2, B 10, C 4.

For the wide pair we have

A C	22°·4	13″·10	1823·3	Σ.
	26 ·7	12 ·72	1879·7	Burnham.

In 1879 Mr. Burnham found that the principal star was double: he has:—

AB	87°·0	0″·73	1879·4.

As the above are objects which large apertures alone can deal with, it is to be hoped that at least an occasional careful measure will be made by the fortunate possessors of such instruments. One thing is quite certain, these stars will not be neglected by their discoverer. JOSEPH GLEDHILL.

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The Measures of certain wide Double Stars.

IN his catalogue of Double Stars, Vol. xxxv. Memoirs R. A. S., Mr. Dawes, in the notes, mentions certain double stars which were selected by M. Otto Struve "for comparison of my (Mr. Dawes's) results with his own and his father's." These are Σ 1399, 1694, 1931, 53 Ophiuchi, ζ Lyræ, OΣ 346, β Lyræ. I have from time to time measured five of these most carefully, and venture to think the results may not be uninteresting to some of your readers. I much wish the measures were more satisfactory, though I consider they are fairly good on the whole. The measures of these wide and relatively fixed stars are only of interest as comparisons with the standard measures here quoted, and with one exception are *all* the measures I have made of these particular stars, so that they are at least honest and unselected.

My instrument is an equatoreal by Cooke and Sons of 8 ins. aperture, and the micrometer is a fine old one by one of the Dollonds. I have most carefully studied and endeavoured to practise the valuable hints Mr. Dawes has given in pages 152–153 of his catalogue. In the position-angles I always "derange the measures as soon as read off, alternately in opposite directions," so as to eliminate, as much as possible, all *bias*. With the same object in view, in the distance-measures, the wire has been brought up to the star, alternately from too open and too close a distance, and double measures on each side of the fixed wire have invariably been made. The number quoted from Mr. Dawes's catalogue is that of his first measure of the star, and is also the number of the note where all the particulars will be found. The places and magnitudes of the stars are taken from Lord Lindsay's edition of the 'Micrometricæ Mensuræ.' I have preferred exhibiting each night's work to merely giving the mean of all the nights. The figures in parentheses denote the number of observations.

Σ 1399 = Dawes 1293. Mags. 6·8, 7·8.
R.A. $9^{h}\ 50^{m}{\cdot}2$; Dec. $+20^{\circ}\ 20'$.

Pos.	Distance.	Epoch.	
°	″		
175·07	30·14	1828·76	Σ on 4 nights.
174·82	30·37	40·32	OΣ on 4 nights.
174·60	30·30	48·82	OΣ on 2 nights.
175·01	29·97	51·29	Dawes on 2 nights.*
175·23 (10)	30·75 (5)	78·27	Hunt.
174·95 (6)	30·86 (4)	79·28	„
..	30·52 (5)	79·34	„
175·50 (4)	30·004 (6)	79·38	G. Knott.
175·09	30·71	..	Mean, Hunt.

Not being satisfied with my distances, I asked Mr. G. Knott to measure the star, and he very kindly sent me the measure quoted above.

Σ 1694 = Dawes 1549. Mags. 4·9, 5·4.
R.A. $12^{h}\ 48^{m}{\cdot}3$; Dec. $+84^{\circ}\ 4'$.

Pos.	Distance.	Epoch.	
°	″		
327·24	21·75	1832·51	Σ on 4 nights.
327·4	22·11	40·51	OΣ on 1 n. at Pulkowa.
327·40	21·83	48·52	Dawes on 2 nights.
326·33 (10)	21·82 (5)	78·41	Hunt.
326·57 (6)	21·81 (4)	79·41	„
327·94 (7)	..	79·38	„ Discordant.
327·17 (7)	..	80·35	„
327·12 (6)	..	80·36	
327·03	21·815	..	Mean, Hunt.

Being much dissatisfied with the position-angle, I have measured it on five nights, and I took especial pains with the last two, which were very accordant. H. and S. measured this star on two nights, with a difference of 2°·12 between the two measures of position. These differences seem to illustrate the difficulty, so much dwelt upon by Mr. Dawes, of measuring the position when it is oblique to the horizon or the vertical.

Σ 1931 = Dawes 1699. Mags. 6·2, 7·6.
R.A. $15^{h}\ 12^{m}{\cdot}6$; Dec. $+10^{\circ}\ 50'$.

Pos.	Distance.	Epoch.	
°	″		
172·51	13·09	1832·21	Σ on 4 nights.
172·80	13·16	44·92	OΣ on 3 nights.
172·13	13·21	49·79	Dawes on 3 nights.
170·70 (7)	13·59 (4)	80·39	Hunt.
172·34 (6)	13·29 (5)	80·40	„
171·23 (6)	13·65 (4)	80·43	„

* There is a misprint in Mr. Dawes's note; the star was measured on 2 nights.

The air was very unsteady in my first measure, and very tranquil in the second, in which the individual observations were very accordant, both in position and distance. The mean of my measures is Pos. 171°·42, Dist. 13″·51, and though inferior to those of the great masters quoted above, I am still in very good company, as Secchi measured this star on two nights with a difference of 1°·58 in the positions, while his distances (13″·398, 13″·572) favour my larger distances. Even Σ has a difference of 1°·5 between two of his position-measures.

ζ Lyræ = Σ 38 Mens. Microm. Appendix I. = Dawes 2091.
Mags. 4·2, 5·5. R.A. 18^{h} 40^{m}·5; Dec. +37° 28′.

Pos.	Distance.	Epoch.	
°	″		
149·67	43·71	1835·23	Σ on 5 nights.
150·00	43·91	40·71	OΣ on 3 nights.
149·48	43·92	48·56	Dawes on 3 nights.
149·79 (6)	43·90 (4)	79·79	Hunt.

Observations very accordant both in position and distance.

β Lyræ = Σ 39 Mens. Micr. Appendix I. = Dawes 2095.
Mags. 3·0, 6·7. R.A. 18^{h} 45^{m}·4; Dec. +33° 13′.

Pos.	Distance.	Epoch.	
°	″		
149·81	45·77	1835·23	Σ on 5 nights.
149·52	45·85	40·70	OΣ on 3 nights.
149·70	45·93	48·59	Dawes on 2 nights.
149·2	45·83	76·7	Hall on 8 nights.
149·1	45·64	78·39	Burnham.
149·52 (10)	45·95 (5)	78·71	Hunt.
149·04 (7)	45·87 (5)	79·80	„
149·40 (7)	..	79·81	„ Very accordant.
149·32	45·91	..	Mean, Hunt.

This really makes a remarkable series. Mr. Burnham states, 'Monthly Notices,' Vol. xl. p. 525, "The measure by Hall is the mean result of 8 nights." I looked in vain for the small star "almost exactly preceding," mentioned by Burnham, 'Monthly Notices,' Vol. xxxv. p. 47. These measures seem to show that my position-angles are not so reliable as my distances, and that my tendency is to make the latter rather too large.

I shall be grateful for any remarks that your experienced readers may feel inclined to make upon these measures.

Hopefield, West Dulwich,
1880, September 24.

GEORGE HUNT.

Double Stars for November.

THE following are mostly naked-eye stars which have been found double or triple by Mr. Burnham; some of them have long been known as double stars.

Σ 17. R.A. 0^{h} 10^{m}·3, Dec. +28° 38′. Mag. 8, 9·3, 12–13.

AB	28°·9	27″·06	1878·7	β.
BC	265 ·4	2 ·04	·7	„

Σ's distance of AB is 26″·33 in 1830.

δ Andromedæ. R.A. 0^{h} 32^{m}·9, Dec. +30° 12′. Mag. 3, 12·5.

299°·3	27″·86	1878·4	β.

η Piscium. R.A. 1^{h} 25^{m}·1, Dec. +14° 44′. Mag. 3, 4, 11.
"A very fine, but excessively difficult pair" (β).

12°·9	1″·02	1878·7

Σ 171. R.A. 1^{h} 42^{m}·6, Dec. −2° 1′. Mag. 8·5, 8·5, 12–13.

AB	157°·6	27″·89	1829·9	Σ.
	159 ·8	29 ·69	1878·2	β.
BC	316 ·0	3 ·69	1878·2	„

48 Cass. R.A. 1^{h} 52^{m}·1, Dec. +70° 19′. Mag. 5, 7, 13–14.
A very small aperture will show B.

AB	264°·4	1″·04	1878·7	β.
	265 ·1	1 ·05	1878·7	De.
AC	49 ·4	23 ·98	1878·7	β.

There is a larger star at 82°, 45″ (β).

Σ 318 (20 Persei). R.A. 2^{h} 46^{m}·1, Dec. +37° 51′. Mag. A 6, B 7, C 10.

AB	158°·7	0″·34	1878·7	β.
AB & C	236 ·1	14 ·04	1878·1	„

J. GLEDHILL.

Mr. Edward Crossley's Observatory,
Bermerside, Halifax.

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*Meteor Notes for November**.

OUR satellite will be full at 8^{h} 39^{m} on the 16th, so that at the epoch of the Leonids (11th–14th) the sky will be involved in moonlight. For an hour or so preceding dawn on the 13th and 14th, observations may be made under the best conditions, inasmuch as the Moon will have set and the radiant in Leo will have attained a considerable altitude. The shower returned with more than ordinary strength last year, and its ensuing reappearance may be looked for as highly probable. The Andromedes are due at the end of the month; but the apparition of this shower must be regarded as extremely uncertain. It was expected to occur last year with great intensity, the conditions being favourable; but not a single meteor of the stream was observed, though vigilantly watched for at many places. The bright moonlight may have contributed to this result; but it is certain that had the display occurred it could not have escaped partial observation. It is therefore difficult to say whether there will be any marked revival of the shower on the nights of the 25th–28th of the present month; but in any case the sky should be persistently scanned for any of its meteors. It may return with unusual splendour, or it may, as in 1879, give no sign of its existence.

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* See 'Observatory,' Vol. i. p. 227, Vol. ii. pp. 242, and Vol. iii. pp. 201–202, for further notes on November showers.

Numerous showers of Taurids, Aurigids, Ursids, and Camelopardids may be now observed; but they belong to a minor class of systems and will require attentive and long-continued observations to distinguish them. The Taurids are in some years a very active display of bright meteors and easily recognizable from the host of more slender streams which mark this period of the year.

Large meteors may be anticipated on Nov. 1, 2, 4, 6–9, 11–15, 19 and 27.

Recent Observations.—Mr. Corder, at Writtle, observed a fine meteor at about $8^{h}\ 40^{m}$ on Sept. 29. It was first equal to ♃, but when low in the haze it seemed to get brighter, so that it was really = ♀. It was a typical green fireball, with slowish motion and short train of sparks. The path was approximately noted as between Mars and Jupiter, descending towards δ Ceti.

A bright meteor was seen at the Observatory, Dunecht, on Sept. 21, at about $9^{h}\ 14^{m}\ 37^{s}$, G.M.T. It was equal to ♀, and passed through the point α 306° 34′, δ +42° 14′ (1840), moving westwards.

On Oct 11, at $10^{h}\ 24^{m}$, a meteor = ♃ was observed by the writer at Bristol. Path from 250° +39° to 251° +28°. Motion very swift. It left a streak for fully 15 seconds, and presumably belonged to a radiant-point in Auriga. W. F. Denning.

CORRESPONDENCE.

To the Editor of 'The Observatory.'

The Standard Spectrum Scale.

Sir,—

I had supposed that the question of a standard scale for the spectrum was finally settled, and the scale of wave-lengths in fractions of a metre ultimately adopted; but in your August number, Prof. P. Smyth appears to be of a different opinion. If the matter is not settled, allow me to suggest that the natural scale is one that has reference, not to the length of the waves, but to their frequency. I quite agree with Professor Smyth that a scale of wave-lengths is very awkward, owing to the contraction of the spectrum at the violet end; and I therefore consider that the theoretically best scale would be one based on the intervals between successive vibrations, calculated in parts of a second. Such a scale would have this advantage, that the same measures of time are used by all nations, while those of length are not. But the great fact in favour of it is, that a scale based on time is a true scale, while one based on length is not, except only when light *in vacuo* is dealt with. This is rarely done; indeed may we not say, never absolutely? for has it ever been proved that there is such a thing as a vacuum in nature? The wave-length scale is therefore only a conventional one; for when a given wave-line is stated to be of a certain wave-length, it is only meant that the wave would be of that length *if in vacuo*. And no one ever *saw* such a wave of light of that length, for the moment it enters one's eye it is greatly shortened by the density of the eyeball. What the eye really perceives is the number of vibrations in a given time; it is this which remains invariable, whatever the density of the medium; and it is this, there is every reason to believe, which causes the particular colour of each ray. When, too, the position of a spectral line is altered by the motion of the source of light—as, for instance, when the rate or motion of a star towards or from us is attempted to be ascertained—it is not really the *length* of the waves that is altered (this remains entirely unchanged), but it is their *frequency*.

While maintaining, however, the theoretical correctness of this method of measuring the spectrum, I am aware that there is one great practical difficulty in the way of adopting it—we do not know the frequency of vibrations with any accuracy, because we do not know the velocity of light. And I suppose we are not likely ever to know this as accurately as the wave-lengths *in vacuo*.

There is also a small objection, viz., that in the course of ages (if the world lasts so long) the second will alter its length; but the same objection may be said to apply, to some extent, to Prof. Smyth's scale, since the British inch is based upon the second.

I am, Sir, yours truly,

Sunderland, 1880, Oct. 7. T. W. BACKHOUSE.

The Colours of Stars.

SIR,—

I am glad to see that Mr. Gledhill has drawn attention, in this month's number of the 'Observatory,' to the subject of star-colours, and the difficulty of forming useful estimates of these colours, owing to the want of a standard scale and of a well-considered method of observation; and I would venture to ask those who are interested in the matter, whether a standard scale of colours could not be constructed from the materials in Prof. Piazzi Smyth's highly interesting paper in vol. xxviii. of the 'Trans. Roy. Soc. of Edinburgh,' entitled "Colour, in Practical Astronomy, spectroscopically examined; in 1877 and 1878?" It is evidently desirable to refer all simple colours and tints to their places in the

Solar spectrum, which is the only true unchanging standard of colours we possess (at least so far as is at present known), and to have the means of knowing what combinations of simple colours and tints form the various compound colours.

Having formed a scale, it would surely not be difficult to indicate the various colours by suitable names, or by a combination of names and numbers.

Mr. Gledhill justly remarks that "the proper kind and intensity of light used in the observatory to illuminate the scale would have to be considered;" this is, in fact, a point which may be regarded as second in importance to that of constructing a standard scale. My own experiments show that a slight difference in the purity of the light illuminating a scale of colours produces a very sensible difference in the depth of some, at least, of the colours. Using two lights of the same intensity, but differing slightly in purity, the shadows of an object thrown on disks of different colours exhibit, in some cases, very marked differences, the general effect being that the impure light lowers the colours in the scale of refrangibility. It appears, therefore, that in order to obtain the closest possible approximation to the true colours of the stars, a bright white light must be used to illuminate the colours of the standard scale. It will also be necessary to place a screen between the eye and the source of light, and to prevent the effect of contrasted colours, by exposing to the light only those colours in the scale which are similar to that of the star under observation. Estimates of star-colours should be made in a dark field, as estimates made under the coloured illumination commonly used by double-star observers cannot be regarded as entirely trustworthy; and the experience of most observers will probably be found to agree with that of Mr. Gledhill, who says "that it is only in good states of the sky that he can feel any satisfaction in attempting to form a judgment of the colours of double stars."

I am, Sir, yours faithfully,
JOSEPH BAXENDELL.

The Observatory, Birkdale,
Oct. 14, 1880.

The Division in Saturn's Rings.

SIR,—

It appears evident, by a letter from Mr. Pycroft, in the 'Athenæum' of last week, that Mamhead, a village a few miles south-west of Exeter, not (as some have thought) Minehead on the Bristol Channel, was the *locus observandi* of the brothers Ball, who first noticed the division in Saturn's ring in 1665, which is now usually called Ball's division. Will you allow me to point out, in the 'Observatory,' that the drawing which they made does not appear (although referred to) in the 'Philosophical Transactions,' where their observation is recorded?

It would seem to have been omitted through some inadvertence; and it would doubtless be of interest if some one who has access to the original manuscripts and papers of the Royal Society, would, if possible, find it and have it engraved. The record of the observation is in the first volume of the 'Transactions,' and the place is there called Mainhead, which is certainly a misprint.

Yours faithfully,

Blackheath, 1880, October 14. W. T. LYNN.

A Bright Meteor.

SIR,—

I observed a very vivid streak-leaving meteor on the evening of September 13; but I am sorry I cannot give the correct time, as I did not note it. I should say about 8^h^ 30^m^ P.M.

The following is a rough estimate of the path:—

Origin	R.A. $18^h\ 40^m$,	Dec. 33° N.	
End..........	17 20	22 N.	

The meteor had a striking appearance in presence of the Moon at the advanced stage at which it was.

I am, Sir, yours truly,

JOHN ROBERTSON.

Coupar Angus,
1880, Sept. 27.

NOTES.

CHROMATIC ABERRATION OF OBJECT-GLASSES*.—Prof. H. C. Vogel has examined the colour-correction of the objectives by Fraunhofer, Grubb, Schröder, and Steinheil, belonging to the Potsdam and Berlin Observatories, by means of a small direct-vision prism placed in front of the eyepiece. The spectrum of a star viewed in this way spreads out in a trumpet-shape towards the red and violet, since the foci for those rays fall without the visual focus in consequence of the secondary chromatic aberration. Also there will be two points at which the spectrum is reduced to a line, these being the two colours which are accurately brought to the same focus, and between these again the spectrum spreads out slightly. These nodal points shift towards the two extremities as the eyepiece is drawn out, and thus the focus for rays of any colour may be found by noticing when the nodal point falls at that part of the spectrum. The distance through which the eyepiece has been drawn out measures the longitudinal chromatic aberration for this colour. The hydrogen lines in a white star afford convenient points of reference for the various parts of the spectrum, and thus Prof. Vogel has laid down curves showing the chromatic aberration for the different parts of the spectrum in the case of each of the object-glasses examined. From these it appears that Fraunhofer paid more attention to the red rays, neglecting the blue and violet, which latter, as Prof. Vogel explains, would be considerably absorbed by the greenish glass which was then made. With the colourless flint now used the blue rays become more important, and in Schröder's object-glass special attention is paid to these at the expense of the red rays, a fact which is strikingly shown by Prof. Vogel's curves.

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* Monatsbericht k. Akad. der Wissenschaften, Berlin, 1880, April 29.

O'-GYALLA OBSERVATIONS.—The volume published by Dr. N. de Konkoly contains spectroscopic and other observations made during the years 1872 to 1878 at the private observatory established by him at O'-Gyalla, near Comorn in Hungary. The instrument employed consisted of a 10½-inch Browning reflector, a 6-inch Merz refractor, a 3½-inch Transit-Circle, and various spectroscopes, clocks, &c. With this equipment Herr Konkoly has made a variety of physical observations which have been published from time to time in the 'Astronomische Nachrichten' and the 'Observatory,' and are here collected in a quarto volume. It contains observations of the spectra of several Comets, Meteors, the eclipsed Moon, Lightning, the Aurora, Mars, and 160 stars, determinations of longitude, observations of the Transit of Venus in 1874 and of Mercury in 1877, a list of magnitudes of 1840 stars from 1st to 5th magnitude, with approximate positions, for the preparation of star-charts, observations of Sun-spots 1872–1878 made with a 3-inch refractor by the method of projection, and a large number of meteor-tracks observed from 1871 to 1878. In the plates are given drawings of Mars in 1877, of Saturn, and of various Sun-spots and Solar Prominences.

THE LICK OBSERVATORY.—In No. 33 we mentioned Mr. Burnham's visit to Mount Hamilton, California, the site of this observatory, and the favourable judgment he formed, after spending two months there, as to the steadiness of the air and the clearness of the sky. His detailed report is now published, and fully bears out the statement that this peak, 4250 feet above the sea-level, is one of the best astronomical sites in the United States. During the 60 days of Mr. Burnham's stay (Aug. 17 to Oct. 16) there were 42 first-class nights, 7 medium, and 11 cloudy or foggy; and Prof. G. Davidson, of the U.S. Coast Survey, considers that this is not the best time of the year. At Mount Hamilton Mr. Burnham discovered forty-two new double stars with his 6-inch refractor, some of them prominent and well-known stars. With regard to the equipment of the new observatory, Messrs. Alvan Clark and Sons are constructing a 12-inch equatoreal, which will be placed in position early in 1881, and the large refractor, which is to be of 36 inches aperture, has, we understand, been ordered of the same firm.

NEW VARIABLE STARS*.—A remarkable variable of the Algol type was discovered by Herr Ceraski at Moscow on June 23. It is the star B.D. +81°, No. 25, situated in Cepheus; position for 1881, R.A. $0^h\ 51^m\ 48^s$, Dec. +81° 14′·1, Mag. 7·5; and, according to Dun-Echt Circular, No. 10, Prof. Pickering finds its true period to be $2^d\ 11^h\ 47^m$, or nearly two days and a half, being half the period found by Herr Schmidt from his observations at Athens. Herr Ceraski had at first given the period as 10 days, or some submultiple of that interval. Prof. Pickering remarks that the rapidity of the change is probably greater than in the case of any other known variable, the variation exceeding a magnitude in the course of an hour. The total variation is more than two magnitudes and is all comprised within two or three hours of the minimum, the star remaining of its ordinary brightness, 7·5 mag., during the rest of the 2½ days. According to Herr Schmidt's observations, minima may be expected about 9^h G.M.T. on Nov. 2, 7, 12, 17, &c.

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* 'Astron. Nachr., Nos. 2324, 2329, 2332, 2335; Dun-Echt Circular, No. 10.

Herr Dunér finds the star B.D. +37°, No. 2771, 8·7 mag., R.A. $16^h\ 30^m\ 5^s$, Dec. +37° 38′·1, to be variable. On 1879, June 18, it was of 8·3 mag., on 1880, June 18, 9·4 mag., and by 1880, Aug. 3, it had sunk to 11 mag. The change of brightness must therefore amount to at least three magnitudes. Its designation would be V Herculis.

!V

The Old Paramatta Observatory.—In 1821, Lieutenant-General Sir Thomas Macdougall Brisbane, K.C.B., having been appointed Governor of the colony of New South Wales, resolved to avail himself of the opportunity thus afforded for promoting astronomical science in the southern hemisphere, by causing an observatory to be established at Paramatta. With this view Sir Thomas Brisbane made a collection of astronomical books and instruments, and engaged two gentlemen—Mr. Charles Rümker and Mr. James Dunlop—to act as astronomers at the intended observatory. On his arrival in the colony in November 1821, a situation was fixed upon for the observatory near what was then Government House, in Paramatta, and there is no doubt that his desire to assist in the work himself, and look after it, induced him to place the observatory so close to his residence.

The building was begun as soon as possible, and finished on the 2nd of May, 1822. It measured 28 feet on each side, and had a flat roof, above which were two domes 11 feet 6 inches in diameter, one on the north side, the other on the south; under these domes the walls were curved so as to form half of a tower. In each of these curved walls were three windows, the only lights in the observatory. The instruments consisted of a 5½-feet transit instrument by Troughton, a 2-feet mural circle by the same artist, a 16-inch repeating circle by Reichenbach, and a 46-inch equatoreal, a clock by Hardy, and another by Breguet. The whole cost of this equipment was borne by Sir Thomas Brisbane, but was repaid by the colonial Government when he left the colony. Mr. Rümker finally left the colony in 1829, and Mr. Dunlop was then appointed astronomer, a position which he continued to hold until 1842.

The instruments were then removed, and the building allowed to fall out of repair. When Paramatta Park was placed in the hands of trustees, the old observatory was pulled down and trees planted on its site.

A great number of valuable astronomical observations were made at Paramatta, and it is important to mark the exact site of the transit instrument with which these observations were taken; so that at any future time, when it may be necessary to use the observations, the geographical position in which they were made may be determined with all the precision which the then existing state of astronomical science may render possible.

At the instance of Mr. J. S. Farnell, M.L.A., a sum of £150 was placed on the Estimates, and voted for the purpose of erecting a suitable monument to mark the site of the Paramatta Observatory. The following gentlemen were appointed trustees of the work:—Mr. James Squire Farnell, M.L.A., Mr. James Barnett, Colonial Architect, and Mr. H. C. Russell, Government Astronomer. In execution of their trust, they have had erected a veined white marble obelisk, exactly in the position occupied by the transit instrument in former years. On the east side of the obelisk an outline diagram of the transit instrument has been engraved, and the following inscription:—"An astronomical observatory was founded here May 2, 1822, by Sir Thomas Macdougall Brisbane, K.C.B., F.R.S., Governor of New South Wales. This obelisk was erected in 1880, to mark the site of the transit instrument in that observatory."

On Monday, March 15, at the invitation of Mr. Farnell, the trustees, accompanied by Mr. Richards (Government Printer), proceeded to Paramatta to inspect the work, and see that the contractor had placed the obelisk in its proper position. Mr. Russell furnished a plan of the old observatory, drawn to scale by the late Rev. W. B. Clarke. By means of this the distance of the old transit instrument from the walls was found. Although the walls are gone the foundations are still there, and the measures from these showed that the obelisk is correctly placed. A photograph of it was then taken, copies of which will be distributed to all astronomical observatories.

Mr. Tebbutt says:—"I am glad that justice has at length been done to the memory of Sir Thomas Brisbane, and that an obelisk has been reared on the site of the old Paramatta Observatory to commemorate the work of that enthusiastic astronomer and his associates. To every enthusiast in the science, the little plot of ground on the elevated portion of the Paramatta Park, near the railway line, must be a spot of great interest. Often as I have stood there in the quiet solitude of the place have I called up to memory the patient toiling of those three repesentatives of the stars—the devoted Brisbane, Rümker, and Dunlop. Rümker afterwards became the distinguished director of the Hamburg Observatory, and was succeeded after his death by his son. Ten years ago a portion of the roofless walls of the Paramatta Observatory was still standing; the semicircular portions forming the chambers for the equatoreal and the repeating circle had, however, totally disappeared, and nothing but the piers of the mural circle remained to testify to the astronomical character of the ruins. The whole affair was fast sinking into oblivion, and it was under these circumstances that in a paper which I had the honour of writing for the Government in 1870, on Astronomical Science in the colony, I drew public attention to the ruins, and suggested both that an obelisk should be erected on the spot and that the geographical position of the Observatory should be determined with all the precision of our modern instruments. No one, therefore, more than myself has cause to rejoice at the step taken by the Government to preserve the memory of Sir Thomas Brisbane among the general public of New South Wales."—*Sydney Mail.*

THE DIAMETERS OF VENUS AND MARS.—The fifteenth publication of the 'Astronomische Gesellschaft' is entitled 'Untersuchungen über die Durchmesser der Planeten Venus und Mars nach Heliometermessungen, &c., von E. Hartwig,' Leipzig, 1879, 4to. The heliometers which were used on the German Venus expeditions, and afterwards deposited at the Strassburg Observatory for a detailed investigation of their constants, have, by the author, been used to take measures of the diameters of Venus and Mars during the years 1876–78. Venus was measured with two heliometers, and from all the observations, the diameter at distance unity ($D+dD$), and the constant error of the measures (i), were found by a number of equations of condition, of which every observation furnishes one of the form

$$d = \frac{1}{r}(D+dD)+i \quad \text{or} \quad d-\frac{1}{r}D=\frac{1}{r}dD+i;$$

where D is an approximate value (17″·50), and r the distance of Venus from the Earth. Herr Hartwig has, in addition to his own, discussed all published observations by other astronomers (many of them reduced anew), and the result is that nearly all measures are subject to rather large constant errors, as the following table shows:—

A.—*Heliometer.*

		i.	Prob. Error.	Aperture.
		″	″	mm.
1820.	Brandes	−1·096	±0·134	76
1847–49.	Wichmann	−0·436	—	158
1857.	Winnecke	−1·088	—	162
1861–63.	Main	−1·182	·091	190
1876–77.	Hartwig	−1·233	·057	76

B.—*Double-Image Micrometer* (Airy).

		i.	Prob. Error.	Aperture.
		″	″	mm.
1862–65.	Kaiser........	−0·772	±0·042	189
1840–52.	Main	−0·50?	—	170
1873.	J. Plummer ..	−0·546?	—	152

C.—*Rochon's Micrometer.*

1810–15.	Arago	−1·519	±0·105	162

D.—*Wire Micrometer.*

1833–36.	Mädler	+0·651	±0·073	97
1854–57.	J. Schmidt ..	+0·116 +0·776	0·245 0·511	108*
1860–63.	Mädler	−0·254	—	244
1838–39.	Galle	−0·86	—	244
1871.	Vogel	−2·56	—	293·5

The negative sign of the constant error for all double-image observations is very remarkable, and it appears that telescopes of all apertures make the diameter as measured in daylight too small, while there does not seem to be any law depending on the aperture. In order to try whether a darker background would make the diameter turn out greater, observations were made in twilight on five evenings in March, 1879, and the resulting diameter was 0″·41 greater than when measured in full daylight. The greater sharpness and clearness of the image on the darker background explains probably this phenomenon. In the case of the wire micrometer, the difficulty in making the contact of the sharply seen spider-line and the far less distinctly seen edge of the planet, must necessarily allow a far greater range for individual errors, and in the opinion of the author this micrometer is not to be recommended for diameter determinations. In conclusion, the mean of the results found at Oxford, Leiden, and Strassburg, 17″·552, is adopted as the most probable value. This result is 0″·60 and 0″·65 greater than those found by Auwers and Tennant during the transit on December 8, 1874.

In an appendix are given extracts from the observer's diary concerning the visibility of the un-illuminated part of Venus. The dark part was repeatedly seen in geocentric distances (Venus − Sun), ranging from 5° to 22°·8.

The second part of Herr Hartwig's work contains his researches on the diameter of Mars. His and his predecessors' measures were reduced in the same manner as those of Venus. The constant error is generally very small, and does not show any regularity. From Arago's, Bessel's, Kaiser's, Main's, and Hartwig's measures, the following result for distance unity is derived: Polar Diam. =

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* Powers 120 and 90 respectively.

9″·349 ± 0″·010, or by simply taking the mean of the four determinations, 9″·352.

The ellipticity of Mars has not yet been found in a satisfactory manner. The Strassburg observations gave the two diameters 9″·311 and 9″·519. Bessel found no difference whatever, but the other observers have; $\frac{1}{96}$ is the mean of Encke's, Galle's, and the above-named observer's results. The opposition in 1879 was very favourable for the determination of the ellipticity, as well as of the position-angle of the axis of rotation, as shortly after the culmination the polar and equatoreal diameters made equal angles with the vertical, whereby personal errors depending on this angle would be eliminated.*—*J. L. E. Dreyer.*

DOUBLE STARS.—Volume xliv. of the Memoirs R.A.S. contains a very extensive work by Mr. Burnham—'Double Star Observations made in 1877–78, at Chicago, with the 18½-inch refractor of the Dearborn Observatory, comprising: I. A Catalogue of 251 New Double Stars with Measures; II. Micrometrical Measures of 500 Double Stars.'

Up to a few years ago the Dearborn refractor, with which Clark discovered the companion of Sirius in 1862, was kept perfectly idle, the original dome being a mechanical failure, only fit to act as an extinguisher. It has, however, since, in the hands of Mr. Burnham, done excellent work. Confining himself to the subject of double stars, this distinguished observer has in the course of six or seven years, first working with a 6-inch, afterwards using the 18½-inch refractor, made this subject his own to an extent which is truly surprising. Though many observations are yearly being taken of double stars by a good many observers, the study of these important objects is by no means being furthered to the extent one should have expected from the vast amount of time and labour expended. As Mr. Burnham justly remarks, in the preface to the paper we are here considering, many observers have gone on from year to year observing over and over again the same familiar stars, of which Castor, γ Virginis, ε Lyræ may be cited as examples. Couples which small instruments of three or four inches aperture would show sufficiently well, are thus again and again being observed with fine refractors of six or seven inches aperture or upwards, while a very great number of stars are hardly ever if at all looked for. "Omit the observations of Dembowski and O. Struve, and our knowledge of nine-tenths of the double stars would not be materially advanced in the last thirty years." Mr. Burnham has from the beginning shown that he does not follow the ordinary beaten track, and his numerous discoveries of close and difficult pairs have proved him to be an unusually sharp-sighted and attentive observer. In nine previous lists he had given the places of 482 new double stars, mostly rather difficult pairs, which few would have discovered with a 6-inch refractor. The present (tenth) list of 251 new objects raises the total number of new double stars discovered by Mr. Burnham to 733; of these 251 stars, 75 pairs are less than 1″ apart. His observations of old double stars embrace chiefly those which require a large aperture to be brought out well, or which have not been recently observed by others. Particular attention has also been paid to certain difficult pairs discovered by Mr. Alvan G. Clark.

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* [Prof. C. A. Young's 'Measures of the Polar and Equatoreal Diameters of Mars' ('Observatory,' No. 39, p. 471) were made at the opposition of 1879.—ED.]

Mr. Burnham has already, in the 'Monthly Notices' for December, announced his further discovery of the duplicity of fifteen naked-eye stars, among which the quadruple system 86 Virginis and the triple system B.A.C. 4531 deserve special notice.

No. 5 of the publications of the Cincinnati Observatory contains 'Micrometrical Measurements of 1054 Double Stars from January 1st, 1878, to September 1st, 1879.' These observations are partly made with a view to the preparation of a catalogue of all double stars between the equator and 30° south declination, partly in order to re-examine such objects as Mr. Burnham has found to need re-observing. These latter stars are not limited to the southern hemisphere. A few others have been entered in the working lists chiefly to aid in the investigation of personal equation between the director, Mr. Ormond Stone, and Mr. H. A. Howe. A great deal of trouble has been bestowed on the investigation of this equation, which for position-angle is expressed by the formula $\epsilon=\alpha+\omega\beta+\omega^2\gamma+\ \ldots$, where α, β, γ, &c., depend on the observer and the angle with the vertical, while ω is the reciprocal of the visual angle v, the latter being found by multiplying the distance in seconds of arc by the ratio between the power used and a certain standard power. In observations of distance any apparent displacement caused simply by personal peculiarities is supposed to produce an effect inversely proportional to the magnifying-power; if μ be the reciprocal of the power, the equation in distance is therefore $\epsilon'=\mu\ (\alpha'+v\beta'+v^2\gamma'+\ .\ .\ .)$. The mean results have also been compared with W. Struve's 'Mensuræ Micrometricæ.' Though the discovery of new double stars has only been considered a secondary object, nearly 200 new pairs have been detected during the progress of the work at Cincinnati. The measures are all given in full detail and occupy 151 pages; the mean results are given separately. The whole forms a carefully prepared volume printed with the elegance usual in American publications.

The systematic errors in measures of double stars have been investigated by Professor Thiele in a paper: 'Castor, Calcul du mouvement relatif et Critique des observations de cette étoile double' (Copenhagen, 1879). The author has chosen Castor partly because the orbital motion is slow and can be represented by a simple formula of interpolation, partly because it has been very frequently measured by almost all observers of double stars. First, three normal places were formed which represented the distances and position-angles observed since 1815, and an angle of position from Bradley's and W. Herschel's measures. From these and the ratio of the sector to the time formulæ for computing distance and position-angle as functions of time by means of the eccentric anomaly were derived, and an ephemeris computed from 1718 to 1719. With this ephemeris all individual observations by every observer were compared and the mean error determined for every observer within such a period during which he might be supposed not to have changed his way of measuring. Every observer is now followed from one period to another and the changes in the mean error determined, whereby variations in his systematic error would reveal themselves.

Dr. Seeliger has made an elaborate examination of Mädler's measures ('Astronomische Nachrichten,' No. 2288). Mädler's distances appear to be considerably less accurate than O. Struve's, while his position-angles do not appear to have larger mean errors than those of O. Struve. The results of a direct comparison between Mädler's and O. Struve's measures are rather puzzling, the quickly moving binaries showing no regularity whatever in the rather considerable difference of distances, while pairs moving more slowly or not at all show errors varying more gradually according to the distance. Considering the great number of measures Mädler took, they would certainly be worth a thorough examination.

Dr. Doberck publishes elements of 36 Andromedæ, γ Leonis, Σ 3062, OΣ 298, 4 Aquarii and μ^2 Herculis, also approximate elements of α Centauri (Astr. Nachr.).—*J. L. E. Dreyer.*

HARTWIG'S COMET.—A bright comet, just visible to the naked eye, with a tail about 2° long, was discovered by Dr. Hartwig at Strassburg on September 29, and picked up independently by Prof. Harrington at Ann Arbor on Sept. 30, and by Mr. Baxendell at Southport on Oct. 3. As usual in such cases, there was a mistake in the telegram announcing the discovery, the declination being given as 200 145 instead of 29° 45′, and the time of observation 7549 instead of $7^h\ 49^m$. In the telegram from the Smithsonian Institution, Washington, two dates were mentioned, viz. Oct. 1 the date of despatch, and Thursday (Sept. 30) the date of discovery; and it was doubtful to which of these dates the place given referred. According to the code adopted by the Smithsonian Institution, it should have referred to Washington midnight of the date of despatch, Oct. 1; but as this convention has never been adhered to, it seemed more likely that the position of the comet was given for the date of discovery, Sept. 30; and this turned out eventually to be the case. Mr. Baxendell's telegram, "Last night bright comet near Alpha Coronæ moving rapidly eastwards," though coming somewhat late, gave in a few words, without a possibility of mistake, more useful information than both the other long telegrams put together. In view of the annoying mistakes which invariably occur in these telegrams, we are glad to hear that the question of the transmission of news of astronomical discoveries on some uniform and simple system has been taken up by the German astronomers, and that Dr. Gould, who has lately been on a visit to Europe, has undertaken to lay their views before the astronomers of the United States. Hitherto the facilities so liberally given by the telegraph companies have been utilized solely for the somewhat uninteresting announcements of the discoveries of asteroids.

The spectrum of this comet was examined at Greenwich on Oct. 7, 11, and 12, and was found to consist of the three usual cometary bands; and a number of measures were made of the position of the brightest band as compared with the bands in the spectrum of alcohol vapour (in a vacuum-tube) λ 5198, and of the Bunsen-burner flame λ 5165. The measures, however, are not sufficient to decide which of these two bands is really coincident with the cometary band. Unfortunately cloudy weather at Greenwich prevented any observation till Oct. 7, and the comet had by that time greatly diminished in brightness.

Prof. C. A. Young has spectroscopically observed the comet on three evenings, and finds that it gives the same spectrum as all the others he has tried—sensibly identical with that of the bluish part of the Bunsen-burner flame, so far as can be made out with one prism.

Prof. Winnecke * considers it highly probable that this comet is identical with that of 1506. The observations of the latter are perfectly represented by a path deduced from the elements of Hartwig's comet, taking the date of perihelion passage as 1506, July 1.

Mr. Hind points out that Hartwig's comet did not pass anywhere near the place of Swift's lost comet of August 11 (R.A. $11^h 28^m$, Dec. 68° N.), and that therefore it could not have been identical with that object.

Swift's New Comet.—The following telegram respecting a new comet was received by the Astronomer Royal on Oct. 13:—"Smithsonian Institution, Washington, 12th Oct. 1880, 10 o'clock A.M. Large comet, Swift, Monday, twenty-one thirty, north eighteen; motion slow, probably north-west." Nothing further has been heard of this comet, and it has not been seen in Europe. It is to be hoped that it will not be altogether lost through cloudy weather, as was the case with the comet picked up by Mr. Swift on August 11 and not since seen.

Double Stars †.—Mr. Burnham calls the attention of observers to the stars ζ Sagittarii (P. 60°, D. 0″·5) and δ Equulei (P. 30°, D. 0″·35). The former was discovered by Prof. Winlock twenty years ago, but it has only been measured once (by Prof. Newcomb in 1867) previous to Mr. Burnham's recent measures. The obser-

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* Circular No. 1 of the Starsburg Observatory.

† Astron. Nachr., Nos. 2338, 2340.

vations indicate rapid orbital movement, and it should be carefully watched by southern observers. The close pair of the triple star δ Equulei seems to have a period of 13 or 14 years; and it seems certain that it is measurable, with any good instrument of ten inches aperture and upwards, during at least one year in every six. It is now probably at the maximum distance, and will become practically single in two years, perhaps less.

Gresham Lectures.—Lectures on Astronomy will be delivered by the Rev. E. Ledger at Gresham College, Basinghall Street, at 6 p.m., on November 8, 10, 11, and 12. Subject:—Saturn's Rings and Satellites, Uranus, Neptune. The Lectures are free to the public.

Mr. Turnbull of Toronto has arranged a form of terrestrial globe which is useful for showing the changes of the seasons, for finding the duration of twilight, and for solving various problems. It is generally similar to the precessional globe referred to at the May meeting of the R. A. S. ('Observatory,' No. 38, p. 438), the pole of the equator being mounted in gymbals about the pole of the ecliptic. The latter is fixed and the ecliptic is represented by the wooden horizon.

We learn that a large refractor of 23 inches aperture is being made by Alvan Clark and Sons for the Halsted Observatory of Princeton College, New Jersey. With this fine instrument Prof. Young proposes to take up spectroscopic work, particularly stellar; and in such able hands we may look for great results. It is high time that one of the large refractors of these days should be devoted to Astronomical Physics.

We regret to have to record the death of Prof. Benjamin Peirce of Harvard University, who died on Oct. 6, in the seventy-second year of his age.

Prof. H. Draper announces, in the 'American Journal' for November, that during the night of September 30, 1880, he succeeded in photographing the bright part of the nebula in Orion in the vicinity of the trapezium. The photographs show the mottled appearance of this region distinctly. They were taken by the aid of a triple objective of 11 inches aperture, made by Alvan Clark and Sons, and corrected especially for the photographic rays. The equatoreal stand and driving-clock he constructed himself. The exposure was for 50 minutes. He intends at an early date to publish a detailed description of the negatives.

Two new minor planets have been discovered, one (No. 219) by Herr Palisa at Pola on Sept. 30, the other (No. 220), of the ninth magnitude, by Prof. Peters at Clinton, New York, on Oct. 11.

A new 10-inch equatoreal, with an object-glass by Merz, has been presented to the Geneva Observatory by its Director, Prof. Emil Plantamour. It is to be devoted to observations of the major planets and their satellites, of parallax of stars, and of double stars, with occasional observations of minor planets. Editor.

Ephemeris of Schäberle's Comet 1880 b*.

By M. G. Bigourdan.

12^h Paris M.T.	R.A. h m s	Dec. ° ′ ″	Log Δ.	Brightness.
Nov. 1....	5 28 26·9	− 6 11 56	0·20349	1.22
3....	5 22 21·0	7 28 15	0·20253	1·21
5....	5 16 5·7	8 43 36	0·20225	1·20
7....	5 9 42·3	9 57 34	0·20269	1·18
9....	5 3 12·2	11 9 48	0·20385	1·16
11....	4 56 36·9	12 19 53	0·20573	1·13
13....	4 49 58·1	−13 27 31	0·20831	1·11

Correction to the Ephemeris :— $+1^s$ in R.A., $+2'{\cdot}7$ in Dec.

The brightness at the date of discovery (April 6) is taken as unity. The actual brightness appears to be decidedly less than the theoretical.

Elements and Ephemeris of Hartwig's Comet 1880 d†.

Dr. M. W. Meyer, Assistant at the Geneva Observatory, has computed the following elements from observations at Kiel Sept. 30, Leipzig Oct 3, and Geneva Oct. 9 :—

Per. Pass. 1880, Sept. 6·9817 Berlin M.T.
Long. (Per. − Node) 323° 8′ 39″ Inclination 142° 2′ 39″
Long. Node 45 39 23 Log. Per. Dist. 9·54782
Mean Equinox 1880·0.

Ephemeris for 12^h *Berlin M.T.*

	R.A.	Dec.	Log. Δ.	Log. *r*.
Nov. 1 ..	18^h 20^m 0^s	+10° 10′·9		
3 ..	24 16	9 41·3	0·18641	0·13754
5 ..	28 15	9 14·6		
7 ..	32 0	8 50·3	0·22128	0·15944
9 ..	35 34	8 28·4		
11 ..	38 56	8 8·5	0·25296	0·17996
13 ..	42 12	7 50·4		
15 ..	45 18	7 34·0	0·28191	0·19924
17 ..	48 18	7 19·3		
19 ..	51 10	7 5·9	0·30843	0·21744
21 ..	53 58	6 53·6		
23 ..	18 56 39	+ 6 43·1	0·33283	0·23466

Astronomical Memoranda, 1880, *November.*

Sun. Nov. 1, sets 4^h 31^m, rises 18^h 58^m; Nov. 11, sets 4^h 14^m, rises 19^h 16^m; Nov. 21, sets 4^h 1^m, rises 19^h 33^m; Dec. 1, sets 3^h 52^m, rises 19^h 48^m.

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* Comptes Rendus, Vol. xci. No. 15.

† Astron. Nachr., No. 2342.

Equation of Time:—Sun *before* Clock, Nov. 1, $16^m\ 19^s$; Nov. 11, $15^m\ 46^s$; Nov. 21, $13^m\ 50^s$; Dec. 1, $10^m\ 35^s$.

Sidereal Time at Mean Noon:—Nov. 1, $14^h\ 44^m{\cdot}6$; Nov. 11, $15^h\ 24^m{\cdot}0$; Nov. 21, $16^h\ 3^m{\cdot}5$; Dec. 1, $16^h\ 42^m{\cdot}9$.

Moon.	*sets.*			*sets.*			*rises.*	
	h	m		h	m		h	m
Nov. 1..			Nov. 11..	14	16	Nov. 21..	7	58
2..			12..	15	29	22..	9	5
3..	4	33	13..	16	42	23..	10	13
4..	5	23	14..	17	54	24..	11	22
5..	6	26	15..	19	3	25..	12	32
6..	7	40	16..	*rises.*		26..	13	45
7..	9	2	17..	4	15	27..	15	0
8..	10	24	18..	5	1	28..	16	21
9..	11	44	19..	5	54	29..	17	43
10..	13	1	20..	6	53	30..	19	5

New Moon, Nov. 2, $3^h\ 55^m$; First Quarter, Nov. 8, $20^h\ 20^m$; Full Moon, Nov. 16, $8^h\ 39^m$; Last Quarter, Nov. 24, $14^h\ 5^m$.

Mercury is an evening star at greatest elongation (23° 14′ E.) on Nov. 2, when he sets 40^m after the Sun, his declination being 24° S. He is in inferior conjunction on Nov. 23.

Venus is an evening star. Diameter:—Nov. 1, $11''{\cdot}7$, Dec. 1, $13''{\cdot}2$. Illuminated portion of Disk $0''{\cdot}847$ on Nov. 15.

Nov. 1, R.A. $16^h\ 25^m{\cdot}5$, Dec. 22° 40′ S., tr. $1^h\ 41^m$, sets $5^h\ 40^m$
Dec. 1, 19 6 ·6 24 32 S., 2 24 6 9

Jupiter is in Pisces. Diameter:—Nov. 1, $45''{\cdot}5$; Dec. 1, $42''{\cdot}1$.

Nov. 1, R.A. $0^h\ 44^m{\cdot}4$, Dec. 3° 3′ N., tr. $9^h\ 58^m$, sets $16^h\ 16^m$
Dec. 1, 0 37 ·6 2 28 N., 7 53 14 9

Daily rate of rotation $870^\circ{\cdot}6 = 2^r\ 150^\circ{\cdot}6$; Hourly rate $36^\circ{\cdot}28$; Period $9^h{\cdot}924$.

Saturn is in Pisces, about 13° E. by N. of Jupiter.

Nov. 1, R.A. $1^h\ 34^m{\cdot}5$, Dec. 6° 53′ N., tr. $10^h\ 48^m$, sets $17^h\ 27^m$
Dec. 1, 1 27 ·6 6 18 N., 8 43 15 18

	Outer Ring.		Inner Ring.		Ball.
	Maj. Axis.	Min. Axis.	Maj. Axis.	Min. Axis.	Diam.
Nov. 10	$44''{\cdot}63$	$10''{\cdot}43$	$29''{\cdot}68$	$6''{\cdot}93$	$17''{\cdot}8$
30	43 ·64	9 ·91	29 ·02	6 ·59	17 ·4

The south side of the rings is visible, the elevation of the Earth above their plane being 13° 31′ S. on Nov. 10, and 13° 8′ S. on Nov. 30, and of the Sun 14° 45′ S. and 15° 1′ S.

Neptune is in opposition on Nov. 4, and may be found about 50′ *sp σ* Arietis.

Nov. 1, R.A. $2^h\ 44^m{\cdot}5$, Dec. 14° 0′ N., tr. $11^h\ 58^m$
Dec. 1, 2 41 ·1 13 46 N., 9 57

Phenomena.

	G. M. T. h	m	
Nov. 1	12	0	J. ii Tr. I.
	12	13	J. i Tr. I.
2	9	26	J. i Oc. D.
	11		☿ elong. E.
	12	18	J. i Ec. R.
3	6	30	J. ii Oc. D.
	6	39	J. i Tr. I.
	6	54	J. iii Tr. I.
	8	52	J. i Tr. E.
	9	21	J. iii Tr. E.
	9	40	J. iii Sh. I.
	10	29	J. ii Ec. R.
4	6	47	J. i Ec. R.
	10		♆ in oppos.
6	7	10	ν^1 Sagitt. Oc. D. 37°.
7	6	35	57 Sagitt. Oc.* 202°.
9	8	56	c^2 Capric. Oc. D. 193°.
	11	12	J. i Oc. D.
10	8	25	J. i Tr. I.
	8	39	κ Aquarii, Oc. D. 90°.
	8	47	J. ii Oc. D.
	10	18	J. iii Tr. I.
	10	38	J. i Tr. E.
	12	48	J. iii Tr. E.
	13	4	J. ii Ec. R.
11	5	39	J. i Oc. D.
	8	40	κ Piscium, Oc. D. 95°.
	8	42	J. i Ec. R.
	9	13	9 Piscium Oc.* 32°.
	9	46	κ Piscium Oc. R. 328°.
12	5	5	J. i Tr. E.
	6	10	J. ii Tr. E.
14	5	57	J. iii Ec. R.
16	13	0	J. i Oc. D.
17	6	55	υ^1 Tauri Oc. D. 78°.
Nov. 17	7	51	υ^1 Tauri Oc. R. 306°.
	8	27	υ^2 Tauri Oc. R. 275°.
	10	12	J. i Tr. I.
	11	7	J. ii Oc. D.
	12	26	J. i Tr. E.
18	7	27	J. i Oc. D.
	10	38	J. i Ec. R.
19	5	54	J. ii Tr. I.
	6	53	J. i Tr. E.
	6	55	[1970] Oc.* 1°.
	8	34	J. ii Tr. E.
	8	52	η Gemin. Oc. D. 94°.
	9	57	η Gemin. Oc. R. 267°.
20	5	7	J. i Ec. R.
	8	16	ζ^2 Gemin. Oc. D. 71°.
	9	14	ζ^2 Gemin. Oc. R. 281°.
21	4	58	J. ii Ec. R.
	5	57	J. iii Oc. R.
	7	43	J. iii Ec. D.
	9	58	J. iii Ec. R.
23	7		☿ Inf. ☌ ☉.
	12	8	*h* Leo Oc. R. 257°.
24	12	0	J. i Tr. I.
25	9	16	J. i Oc. D.
	12	34	J. i Ec. R.
26	6	27	J. i Tr. I.
	8	19	J. ii Tr. I.
	8	41	J. i Tr. E.
	11	0	J. ii Tr. E.
27	7	3	J. i Ec. R.
28	6	58	J. iii Oc. D.
	7	34	J. ii Ec. R.
	9	35	J. iii Oc. R.
	11	46	J. iii Ec. D.

* A near approach.

The angles at Oc. D. and R. are reckoned from the *apparent* N. point towards the right of the Moon's inverted image. EDITOR.

G.M.T. of Transits of Red Spot across Central Meridian of Jupiter.

	h		h		h
Nov. 1..	4·5	Nov. 11..	12·6	Nov. 21..	10·9
2..	10·3	12..	8·5	22..	6·7
3..	6·1	13..	4·3	23..	12·5
4..	11·9	14..	10·1	24..	8·3
5..	7·7	15..	6·0	25..	4·2
6..	13·5	16..	11·7	26..	10·0
7..	9·4	17..	7·6	27..	5·8
8..	5·2	18..	13·4	28..	11·6
9..	11·0	19..	9·2	29..	7·5
10..	6·8	20..	5·1	30..	13·2

EDITOR.

Publications received:—R. S. Ball, *Elements of Astronomy* (Text-Books of Science: Longmans, London, 1880)—*Radcliffe Observations*, 1876 (J. Parker, Oxford, 1880)—*Greenwich Observations*, 1878 (H.M. Stationery Office, London, 1880)—*Bulletin of the Philosophical Society of Washington*, Vols. i., ii., and iii. (Washington, 1874–1880)—Prof. Pickering, *Dimensions of the Fixed Stars, with especial reference to Binaries and Variables of the Algol type* (Proc. Amer. Acad., Cambridge, U.S., 1880)—W. Huggins, *On the Photographic Spectra of Stars* (Phil. Trans., Part ii., 1880)—*On the Spectrum of the Flame of Hydrogen* (Proc. R. S., No. 205, 1880)—William L. Elkin, *Ueber die Parallaxe von α Centauri* (Karlsruhe, 1880)—S. W. Burnham, *Report to the Trustees of the "James Lick" Trust of Observations made on Mt. Hamilton* (Chicago, 1880)—*An Examination of the Double-Star Measures of the Bedford Catalogue* ('Monthly Notices,' Vol. xl. No. 8)—E. B. Knobel, *Note on Mr. Burnham's paper entitled "An Examination of the Double-Star Measures, &c."* ('Monthly Notices,' Vol. xl. No. 8)—Prof. H. C. Vogel, *Ueber einer einfache Methode zur Bestimmung der Brennpunkte und der Abweichungskreise eines Fernrohr-objective &c.* (Monatsbericht k. Akad. Berlin, April 29, 1880)—Dr. F. Terby, *Aspect de la Planète Mars* 1879 *et observations de la tache rouge de Jupiter et des taches de Venus* (Bulletins Acad. Roy. de Belgique, March 1880)—Capt. Abney, *On the Reversal of the Developed Photographic Image* (Phil. Mag., Sept. 1880)—D. P. Todd, *Speculative and Practical Search for a Trans-Neptunian Planet* (Amer. Journ., Sept. 1880)—*An Attachment to Equatoreal Mountings to facilitate Sweeping in R.A.* (Proc. Amer. Acad., May 1880)—Ch. Fievez, *Le Spectre du Magnesium en rapport avec la constitution du Soleil* (Bruxelles, 1880)—Asaph Hall, *Address before American Association, Section A* (Cambridge, 1880)—David Gill, *The Earth's Distance from the Sun* (Address to S. African Phil. Soc. 1880, July 30).—W. Ellis, *Relation between Diurnal Range of Magnetic Declination and Horizontal Force and the Period of Solar Spot frequency* (Phil. Trans. 1880, Part ii.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

No. 44. DECEMBER 1. 1880.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, 1880, November 12.

Edwin Dunkin, Esq., F.R.S., *Vice-President*, in the Chair.

Secretaries: W. H. M. **Christie**, M.A., and
J. W. L. **Glaisher**, M.A., F.R.S.

The Minutes of the Meeting of June 11 were read and confirmed.

Mr. Christie announced that 146 presents had been received since the last Meeting of the Society, including a copy of the Works of Laplace, presented by the French Academy; 11 volumes of the 'American Ephemeris,' presented by Prof. Newcomb; and also a photograph from a drawing by the Rev. Cooper Key of a spot on Jupiter which seems to be somewhat similar to the red spot at the present time, presented by Prof. Piazzi Smyth.

The thanks of the Meeting were voted to the respective donors.

The Astronomer Royal. Before commencing my description of an equatoreal sextant by Flamsteed, I wish to say that, as I was coming away from the Observatory, I was struck with the idea that amidst the great number of descriptions on the colours of the stars no appeal has been made to Admiral Smyth's 'Sidereal Chromatics.' It is by far the most complete and elaborate book upon the subject that I have yet seen, and I have brought a copy with me. Amongst other things it contains a very careful set of exhibitions of colours which it was intended might be used as standards to which to refer estimates of colour. I will hand it round to the meeting.

The principal object of my address at this time is to call the attention of the Meeting to two pictures. Many years ago, perhaps forty years ago, there were presented to me by my friend Francis Baily two engravings evidently made by Flamsteed, and evidently intended as parts of an elaborate description. They have letters of reference; and I have no doubt that in the first instance there was a printed and careful description of them; but Mr. Baily told me that he had never been able to obtain another copy, and I have never seen or heard of another. I have considerable reason to believe that the copies of these two pictures which I hold at the Observatory of Greenwich are unique; and I am anxious that they should not remain in that state. Photography at the time I speak of was not known, but photography has acquired great perfection now, and I have had photographs taken of them by the Woodbury Company, and they are taken with a degree of exactness, even to the minutest parts and hatchings of the engraving, that I could not have conceived of before. I have therefore taken the opportunity of exhibiting them to the Society. I am sorry to say that I cannot leave them in their possession, for I have no authority to take another copy, but they can be lent as long as the authorities of the Society think proper to keep them.

I will now mention what was the singularity of that instrument, why it was necessary then, and why it is unnecessary now. You will perceive that it forms a turning-point in astronomy. We all know that at the present time it is easy enough to get the north-polar distances of objects, and so it always was. There was no difficulty in the most distant stages of astronomy when you could get, in a position near the meridian, something like a graduated circle to take altitudes in that meridian. Thus there was no difficulty in getting north-polar distances; but right ascensions have always been the trouble, especially whilst astronomy was in a very crude state, and when there were no transit-clocks. In the time I speak of, down to and including Flamsteed's time, and the time of Halley, there were no clocks on which the slightest reliance could be placed. Attention is called by Mr. Baily to a particular passage in the account of Halley's observations, by which it appears that the clock was wound up simply by pulling a string; and, altogether, the clock was no better than those little clocks which we have for alarums and the like. A transit-clock which could go with accuracy for several hours was totally unknown. In the absence of clocks upon which any reliance could be placed, it is not easy to see, in the first instance, how differences of right ascensions could be got. They were, however, got in this way:—Knowing the polar distance of stars, and taking the angular distance from one star to another, you can compute the difference of right ascension, and so you can go spanning all round from star to star completely round the celestial globe; and that was the way in which differences of right ascension were got, by measuring those angular distances from star to star. The question is, what sort of instrument would you use for that purpose? Of course it would be an instrument capable of measuring angles; but how would it be carried? We know at a certain time not far preceding the time of Flamsteed how it was carried. There is a very admirable work by old Tycho, Tycho's 'Astronomiæ Instauratæ Mechanica,' in which there are engravings representing a great number of instruments, and one of them is an instrument for this purpose. It is a frame turning upon a vertical axis, and with an arc (whether you would call it a quadrant or a sextant I do not know) whose plane could be turned in different directions, so that you could make the plane pass through the two stars, and you could thus read the angular distance which was to be measured, and the problem was solved. Flamsteed improved upon this by using not a vertical axis, but a polar axis (and it is in that state that the instrument is represented here). It will be seen in the engraving that nearly every detail of construction is traced so well that a modern instrument could be made like it if it were so desired. There is a polar axis which projects beyond its bearing so far that it is, perhaps, a little unsteady; but that depends upon the stiffness of the axis. The polar axis has at its top a piece which I should almost call a T-head, provided I give some latitude to that expression. If it had been a T-head strictly meant to give the power of varying the position of the plane of the quadrant or sextant by allowing that plane to turn upon the pivots at the ends of the T-head, then, inasmuch as the extent of the angle measured by the instrument is limited, it could not have passed through certain stars which it was desirable to reach; and, accordingly, the top of this piece is not a T-head, except you call it a sort of italic *T*. It is a *T*-head which admits of being inclined, and there is a graduated wheel and a screw acting in a very delicate way upon it, by which the position of the *T*-head could be inclined to the polar axis. Then this *T*-head carries, by proper pivots at its ends, the plane of the sextant. Members of the Society are well aware that the idea of using a complete circle had not entered people's heads at that time, wonderful to say, and their instruments were very limited in arc. Flamsteed's was a sector of 60°, as far as I can judge. The sextant turned by pivots upon the ends of that inclined *T*, and carried two telescopes which could be used in the ordinary way. The radius of the sextant was 7 feet. The first volume of Flamsteed's 'Observations' is entirely occupied with measures of distances made in that way and with the consequent determination of differences of right ascension. Of course it was desirable not to use this for every object. It could be used for the great stars, which are limited in number; but it was not to be supposed that every little object was to be treated in the same way. For little objects Flamsteed used the transit-instrument and a bad clock; but that bad clock would go pretty well for a couple of hours, and then he arrived at another star whose place was pretty well determined. As I mentioned, this, as far as I know, is the latest instrument that was made on that principle. It was, I suppose, in the time of the unhappy circumstances into which Flamsteed's obstinacy led astronomy—at the time when he speaks of having made a sacrifice to heavenly truth by burning every thing he could—that the instrument might have been destroyed. A description of it, no doubt, was printed (from the references which still appear upon the engraving). I suppose all the printed copies were destroyed, for there is not a trace of them anywhere that I am aware of. Flamsteed was succeeded by Halley, who relied solely upon the principal stars which had been determined by Flamsteed, filled up, where necessary, by the use of that curious transit of his (still preserved at the Royal Observatory), which has two axes of unequal length.

There was another man who reformed astronomy, and who has not received the proper credit for it. It was the clockmaker Graham, who produced clocks which would go not only for hours, but for days, and for many days. In them he introduced two great principles, one of which was the compensation-pendulum, which was first made in the form of a gridiron-pendulum (subject only to the fault that if it were not properly made some of the rods might stick a little in their transom-bars), but which was afterwards abandoned by Graham for the mercurial compensation. The other was the dead-beat escapement. The advantages of these instruments were realized very quickly; and they were adopted everywhere; and astronomy, from that time, became a different science. It became a science exclusively of meridional observation for those principal stars as well as for the small stars, using the transit-instrument and the clock for the determination of the differences of right ascension of the great stars as well as of the little stars. I think it is due to the memory of Graham to say that he himself must have carefully considered the theory of the action of the maintaining power on the vibrations of the pendulum. Nobody before his time seems to have had an idea that for the proper going of a clock-pendulum it is necessary that the impulse should be as nearly as possible at the centre of the arc of vibration. No theory, as far as I know, ever was published by Graham himself (though he evidently had it in his own head), nor by any other person, till the one that was published about fifty years ago in the 'Cambridge Transactions.' The establishment of instruments immediately put on a different shape. It assumed at once the form which it has kept, down to the present time. Advantage was taken of it by Bradley; and Bradley worked astronomy in a way which has excited the admiration of astronomers. But if you ask me who was the great reformer of astronomy, I would answer, it was not Bradley; it was Graham. He was a clockmaker, a man of very superior talent and acquirements, who was known not only as a clockmaker, but as a maker of instruments (quadrants, zenith-sectors, standards of length, and others) of great completeness, the accuracy of which has never been surpassed; and he was the first man to discover the diurnal changes of magnetism and the irregularities in these diurnal changes; and he was certainly the leading man of science in his day. He was the only working manufacturer who has been buried in Westminster Abbey; and on the occasion of his funeral, was followed by all the members of the Royal Society.

Regarding this instrument of Flamsteed's as the last of the ancient class, and a very fine specimen indeed, and adverting to the change which took place after it under the auspices of Graham, which have been maintained to the present day, I thought that these engravings might be introduced to the Society as calling their attention to one of the most important epochs in modern astronomy. I will leave the pictures here for as long as any body likes to look at them.

Mr. Knobel. The Astronomer Royal has called attention to the antiquity of right ascensions. It may be a point of some small interest to note that the earliest catalogue of right ascensions with which we are acquainted was published a great many years before Tycho, and was of the epoch 1280, when we have a catalogue of 180 stars arranged in right ascension, in that case being determined not from the equinoctial, but from the solstitial colure*. These were probably determined by an angular instrument similar to those which were used by the astronomers at that particular date. As to the Astronomer Royal's remark that he has seen no reference to Admiral Smyth's 'Sidereal Chromatics,' I may mention that quite recently I had to make a selection of references to the work in question in my Notes on the Bedford Catalogue.

Capt. Noble. With reference to the 'Sidereal Chromatics,' I think it is better known and more widely distributed than Sir George Airy appears to imagine; but in attempting to use it myself, I find this difficulty, that one looks at it generally by the light of a bull's-eye lantern, and the colours then look very differently from what they do in the daytime; and it is exceedingly difficult to refer a colour which you see at night to what I may call its legitimate colour by daylight.

Mr. Ranyard. As to the 'Sidereal Chromatics,' some time ago I purchased the remainder of the impression, some 80 or 90 copies; I think there are now about 20 which I left with the Society for anybody who is working at the subject of the colour of the stars, and who likes to ask for them. I conclude that the value of the book is now much less than it was, for eye-estimations of colour are now superseded by other and more accurate determinations. In fact the spectroscopic classification of stars has taken the place of mere eye judgments.

The Astronomer Royal. It contains a collection of a great number of observations of a great number of stars by several different persons; and it is that, I think, which gives it its peculiar value.

Mr. Sadler. I think that both with regard to the collection of Sestini's and of Admiral Smyth's observations one certainly could not by any means compare the colours of a double star with Admiral Smyth's disks as viewed with a bull's-eye lantern.

The Vice-President. Perhaps I may be permitted to say that this work of Admiral Smyth on 'Sidereal Chromatics' was distributed in a great measure by Admiral Smyth himself, who presented a large number of copies to astronomers. I had a copy presented to me at the time of publication; and I have no doubt that all the principal astronomers of the day had copies presented to them. It is a work which is, I think, of some value; and I quite agree with Sir George Airy, who now draws attention to it, because the observations were considered important at the time at which they were made. Perhaps we ought not to value it so much for what it is worth now as for what it was worth when it was published. With regard to the Astronomer Royal's remarks on Flamsteed's arc, we must thank him for the interesting statement which he has made. When he first showed me the very interesting photographs which he has had taken, I was really astonished at their beauty. Every minute detail is given. Every little spot that is in the original plate is reproduced exactly in the photograph which the Astronomer Royal now exhibits to the Meeting. With regard to the instrument itself, the statement which the Astronomer Royal has given is most interesting to us all, because it shows the history of observation from almost the beginning of the construction of astronomical instruments. He has told you how right ascensions were observed long before clocks were invented. We must all feel a great interest in the remarks which he has made to night. Not only are they interesting with respect to the old sector-arc, but they also show that those old astronomers, whom some of us are apt to look upon almost as astrologers, knew a great deal more than we are sometimes inclined to give them credit for; and the amount of originality which they exhibited in all their observations and inventions may well astonish us who are living in the nineteenth century. I am sure you will join me in thanking the Astronomer Royal for his interesting statement. (Applause.)

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* Aboul Hhassan's Table of the co-ascendants of 180 stars, translated by Sédillot.

Mr. Neison read a note on Hansen's value of the semidiameter of the Moon, in the course of which he said:—

I have now come forward to point out that the value given in my former paper is in error to the extent of about $\frac{6}{10}$ of a second. This correction necessitates no change in any of the statements I have advanced. It only deals with an actual constant; but it is necessary that these things should be pointed out rather than there should be any doubt or uncertainty as to the value of these important quantities. I wish to express my regret to the Fellows of the Society, especially to Prof. Pritchard, that I should have misled them in this matter for any short period.

Mr. Knott read a paper on the observations of Ceraski's variable star in Cepheus exhibiting the light-curves for two minima on Oct 23, $11^{h}\ 27^{m}$, and Nov. 2, $11^{h}\ 0^{m}$ G.M.T. These showed that the star remained for about 2 hours at its minimum, and that the decrease and increase each occupied about 2 hours, the whole variation extending over 6 hours out of the period of $2\frac{1}{2}$ days. When near minimum, the star was slightly ruddy; and as it regained light, it appeared white or bluish white.

Mr. Knott added:—I hoped, if Mr. Hind had been present, that he would have made a few remarks upon the star. It has been observed at Dun Echt, Lord Lindsay's observatory; and I have had one or two interesting letters from Dr. Copeland about it, which I have not brought with me because I thought Dr. Copeland would refer to his own observations. Mr. Hind, from some calculations of some other observations, had deduced a period of $2^{d}\cdot 4908$, or something like that.

Mr. Stone. There have been a number of observations at the Radcliffe Observatory. The weather has been too cloudy to enable one to speak with certainty; but the period undoubtedly is something like that.

Mr. Knott. I noticed the change of colour at the minimum which was noted by Dr. Copeland. I do not know whether it was due to absorption.

Mr. De la Rue. When it changed its colour, do you imagine that there was a dense nebulous mass of gas around it?

Mr. Knott. There was something of that kind perhaps.

Mr. Stone. Might I ask whether there was not a slight variation about the time of maximum?

Mr. Knott. I have not observed any.

Mr. Stone. Our observations were very much broken by bad weather, so that I could not speak positively upon the matter.

Capt. Noble. May I be permitted to offer one suggestion to Greenwich or to Oxford or anywhere where they have sufficient instrumental means, that some attempt should be made to determine the parallaxes of stars such as Algol and this star, which is of such a short period? It would be an interesting thing to ascertain the scale of the system with reference to the theory which Prof. Pickering has recently advanced in explanation of these variable stars of very short period. The variation is supposed to be caused by a dark globe comparatively close to the primary and revolving round it in a very short period. If that theory will hold water, I think it will be an essential element in determining its feasibility if we could get the scale of the system.

A Fellow. May I ask if the star has been examined with the spectroscope?

Mr. Knott. It has been examined with the spectroscope in Lord Lindsay's observatory. At about minimum the blue of the spectrum died down and the red was intensified. The blue was more intense when the star was near maximum.

Mr. Neison. It is rather an objection to these views of Prof. Pickering's, that they are opposed to the theory of gravitation. If you can imagine a body with a certain orbit and another body revolving round it in such an orbit that it takes two days and a half to go completely round it, and that body is to produce any effect upon the appearance of them, they must both be nearly of the same size; so that instead of revolving round one another, they would revolve round their common centre of gravity; with regard to what Mr. De la Rue suggests as to a dense mass of gas it is quite conceivable that there might be a volume of gas of so slight a density that it would have its mass so very small in comparison with the central star that you would not get such periodical variations in position.

Mr. Ranyard. Prof. Pickering has thrown out a most interesting idea with regard to the possibility of determining the mass of these non-luminous objects by means of spectroscopic measures of the shift of the lines of the bright star when it is on opposite sides of its orbit.

Mr. Brett. If there were such a displacement of the star, it would subtend so small an angle that nobody could ever discover it.

Mr. Common read a paper on the mounting of his 3-feet reflector, and a note on the nebula near Merope, in which, referring to a remark by M. Tempel that he had used a telescope of insufficient power to show the nebula, he explained that the telescope used was his 3-feet reflector. He also exhibited a photograph of Jupiter showing the shadow of the third satellite on the disk of the planet.

Mr. Brett. As the subject of reflecting telescopes is now before us, all those observers who have used them know that there is one great objection to them, owing to the deposition of dew, as it is called, on the small mirror. The dew must be either falling upon the little mirror or rising to it, or else it must be floating in some medium of warm vapour in which the mirror condenses the moisture upon itself. At all events, the little mirror is covered with small globules of moisture which prevent you from seeing any thing. If the dew is falling, it seems to me that it cannot fall upon the little mirror, because it faces the earth. If dew is rising it cannot deposit on it because it is enclosed in a tube, the bottom of which is the great mirror. If the moisture is floating in the air that surrounds the little mirror there is no reason for it to be deposited, because the temperature of the little mirror would be the same as that of the space within which it stands. Therefore the deposit of moisture on the little mirror is not accounted for according to the usual explanation. It appears to me that the true explanation of it is simply that the observer acts like a cauldron of boiling water and produces a great cloud of steam. The litttle mirror is naturally colder than the observer, and of course a great deal of this steam must be deposited upon it as well as on the telescope. The only cure that I have found to answer in preventing this is to look to windward. If the observer stands with his face towards the wind, this cloud of moisture which emanates from himself will be blown away and not deposited on the little mirror, and he will be able to observe with perfect comfort. The greatest of all reflecting-telescope observers, Mr. Lassell, used to carry in his pocket a collection of small pieces of metal, which he used to slip in behind the small mirror in a little cell for that purpose, so as to raise the temperature of the little mirror to that of himself; and in practice that has answered very well. There is no such contrivance in my own telescope; but I have arranged to place the eyepiece in the leeward side of the telescope, and it has answered every purpose in preventing the deposition of dew, as it is called.

Mr. Common. There is a great deal of truth in what Mr. Brett says. I mentioned in my paper the use of the telescope during one winter; and I suggest some improvements. Amongst others I think it would be much better to have a screen of some kind for the observer that would run along the same platform as the telescope, not only as a protection for the observer from the inclemency of the weather, but also as a protection from the observer's own heat. There is no doubt that draughts of air do come from the observer's body. Of course Mr. Brett's experience as to whether observers steam or not is greater than mine. I have found that generally, in observing, one's condition is any thing but a steaming one. With regard to the small mirror, when the observer has to look over to the leeward and to look downwards, he is liable to cause dew; and to prevent that I have carried two insulated wires, which distribute enough heat at the back to keep the temperature a little above that of the atmosphere; but the best thing used was a cap with a circular hole for the rays of light to come out. That gives the most efficient protection against the steam of the observer that I know, or at least against the falling of dew.

The Vice-President. Any one looking at these beautiful drawings will see that Mr. Common has taken a great deal of trouble and pains to get such good results with his telescope. With regard to the steaming process, if I may be allowed to say so, I have frequently steamed while making astronomical observations. I should advise Mr. Common to come to Greenwich some close night like the present one, and observe for one hour with the altazimuth, so that his set of observations are got in a reasonable time; and I am certain that he will steam pretty well.

Capt. Noble read a note on the peculiar colour of the shadow of Jupiter's second satellite in transit.

Mr. Campbell. I observed the satellites of Jupiter on the same evening as Capt. Noble; their positions were as described by him. The difference of colour struck me; but I thought I had read about it somewhere, probably in Webb's 'Celestial Objects.'

Mr. Knobel. About five or six years ago, Mr. Flammarion published, in the 'Comptes Rendus,' a precisely similar observation, relating to the transit of Shadow ii, 1874, March 25.

Mr. Christie read a paper by Mr. Baxendell on a new nebula of large size in Aquarius, somewhat resembling that in the Pleiades referred to by Mr. Common.

Capt. Noble asked what aperture was used.

Mr. Ranyard. The aperture of the telescope used would not matter much for a large nebula. For examining such nebulæ we are in a very unfortunate position compared with the observers in Italy and America, where they have such clear skies, the sky making much more difference with regard to the light of large faint nebulæ than the aperture of the telescope.

Mr. Christie read a paper by *Mr. Burton* and *Mr. Howard Grubb* on a new form of "Ghost" micrometer, in which the image of spider-lines or of a reticule is formed in the field of view by reflection.

Mr. Bidder. Messrs. Burton and Grubb have been good enough to refer to the form of micrometer which I exhibited in this room some years ago. No doubt the principle of their micrometer is essentially the same as mine. I suppose every parent has a bias in favour of his own bantling; but I am bound to say that I think they have not improved the form. One peculiarity of mine which they refer to as, in their opinion, a disadvantage (I gather that they have never worked with my micrometer) is, to my mind, one of the greatest luxuries and advantages attending it, and that is the power of moving the rectangular prism by which the rays are turned in the direction of the eye. In fact, it does away with all necessity for a slipping-piece or any similar adjustment, because I have screws by which I can move the prism so as to bring the wire in contact with the star which I wish to measure without any interference with the telescope itself. The luxury and comfort of that is only conceivable by those who have used it. With regard to their objections to the variable scale in the form which I described, I do not understand them at all—practically when you have got the instrument into adjustment there is no variation of the scale, or at least no variation that is appreciable. I have had it mounted now for some years; I have used it frequently, and, as far as I am competent to judge, the facility and accuracy of measurement are very great. I do not say that because it is my work, but because I have tried it and found it to be what I describe. I place my micrometer outside the tube of the telescope, and then by two rectangular prisms I bring the image of the wires into the field.

Mr. Downing read a paper upon the determination of the diameter of Mars at the mean distance of the Earth from the Sun.

Mr. Stone read a paper on the determination of the coefficient of the parallactic inequality in the expression for the Moon's longitude.

Mr. Neison. As to Mr. Stone's objecting to what Mr. Campbell and myself have said, we assumed that he had done the one thing he said he had done and nothing more. Now it seems he has done something else, and therefore the corrections that we have applied in our paper are unsatisfactory. But we put them on a purely hypothetical basis. As regards the 45-year inequality, when you come to compare the series of calculated values with the actual results, you find that they only differ by about $\frac{1}{10}$ of a second of arc, whereas if you compare them with one constant value they differ by a quantity considerably over a second of arc. It is true when we come to a change in the instruments and take the later observations and compare them with the calculated values, we do not get the same accordances. It is quite possible that these divergences may be due to some instrumental changes; but if you find these accordances between the calculated values and observations for a period of 95 years and then discordances for a period of 5 years, you cannot say that the inequality does not exist. These inequalities are confirmed by later observations.

The Vice-President. This is a most valuable paper, and most of us will read it with great pleasure when it is in print. Some of us have not been able to follow it exactly, but when it is printed I am sure it will point out some very important results. Let us, if possible, get the truth of the matter; that is what we are desirous to do. If Mr. Neison can, on the other side, bring forward any proposition we shall be glad to receive it—although it may not exactly agree with the results of Mr. Stone's investigation—as it is important for us to find out the truth.

Mr. Christie. The Astronomer Royal has received a telegram from the Smithsonian Institution, Washington, announcing that the comet discovered at Dun Echt by Dr. Lohse on November 7 is the same as that which was found by Mr. Lewis Swift on Oct. 10, and is identical with Comet iii. 1869. It would therefore be a comet of 11 years' period, which is an important discovery.

The following gentlemen were balloted for and duly elected Fellows:—Rev. F. B. Allison, B. F. Cobb, H. A. Severn.

The meeting adjourned at ten o'clock.

The Group ε Lyræ.

In preparing the present diagram, the chief object has been so to arrange the results of past observations as to fairly represent our present knowledge of this group, and to combine with that a usefulness for reference by subsequent observers.

In constructing the map from which the diagram given in No. 41 was copied, micrometer measures were made of the principal stars, and these positions were used as starting-points in fixing the places of the fainter ones. No other course was available, because many of the latter class were too small to bear the least illumination of the field. From the original map so laid down the following measures have been taken; but although they cannot claim the full authority of *direct* measures, except in the case of the brighter stars, yet they may be of some little value for comparison with those of others, and may serve to give an idea of the approximate accuracy of the diagram.

The order of the letters affixed to the stars for easy reference is partly that of discovery; K is a star first mentioned by Dawes, L is Lassell's star, and M is one first recorded by Sadler and Ward. The remainder are lettered in groups as they occur round the brighter stars, the preceding ones being taken first.

In respect to magnitudes, only approximations can be offered, as these are simply estimations and not measures.

Positions measured from G.

Star.	Authority.	Pos.-Angle.	Distance.	Mag.
		°	″	
A..........	Lamont	335	117	
C..........	„	186	92	
E..........	„	92	67	9·5
F..........	„	224	50	10·5
G	„	0	0	12
H	„	241	80	14
I..........	„	267	76	15
J..........	„	266	144	
K	Dawes	306	113	13
L..........	Lassell	336	24	
M	Sadler & Ward	125	59	15
N	Pratt	307	68	15 to 17
O	„	328	65	„
P..........	„	343	75	„
Q	„	241	52	„
R	„	263	35	„
S..........	„	198	62	„
T..........	„	179	67	14
U	„	173	85	15 to 17
V	„	33	102	14
W	„	60	80	15 to 17
X	„	82	79	„
Y	„	86	109	14

During the observations made in constructing the map, it was continually noticed that G was much less bright than F. K was quite an easy object. H, Y, V, T,were readily seen by attention, H being the brightest, and the rest in descending order. I and M were next in order of brightness. The remainder required close attention and the exclusion of the larger stars from the field. The star J was always seen, but was not included in the diagram, because it is outside the circle referred to in No. 41, and within which the observations were rigidly confined*. Lassell's L was for some reason overlooked, a slight omission not difficult to account for, its existence being unknown at the time the observations were in progress. This it will be noted is the only star, whose position was previously well determined, which was unobserved.

It is necessary to note that, by a printer's mistake, N.P.D. was inserted at the side of the diagram in the September number, instead of N. Dec., which the numbered scale represents.

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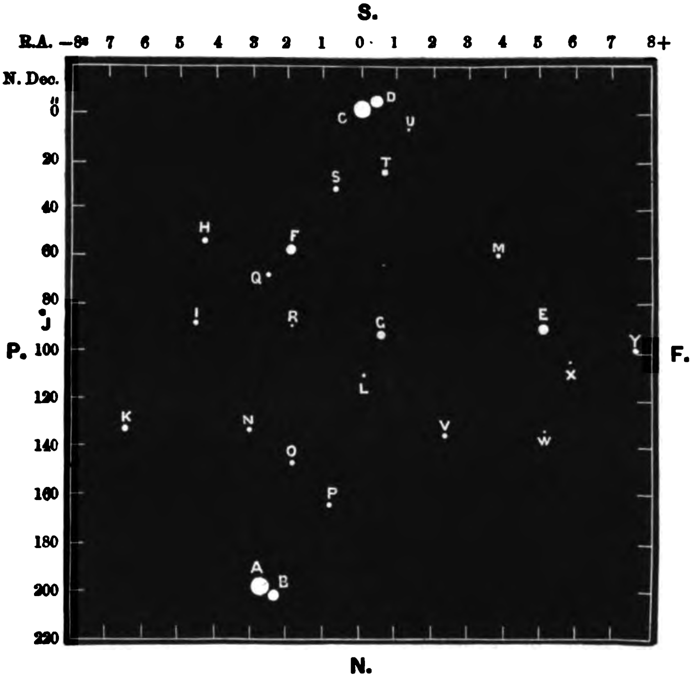
* Its position is now indicated, just outside the diagram.

The history of the principal stars in this group is as follows:—On the 29th of August 1778, Sir William Herschel first saw ϵ^1 and ϵ^2. He writes:—"Aug. 29. A very curious double-double star. At first sight it appears double at some considerable distance, and by attending a little we see that each of the stars is a very delicate double star." He calls them "5 et ε Lyræ, Fl. 4 and 5" (Phil. Trans. vol. 72).



In his MSS. the following note, with a diagram, is found:—"Nov. 29, 1782. Inter ε and *e* Lyræ (Mayer's *e*) the 20 feet shows me two small stars [F, G] I have never before taken notice of with the 7 feet. That which is below [E. mag. 9½] I have often noticed."

Then comes the observation of Sir John Herschel and Sir James South:—"*Debilissima inter* 4 (ε) *and* 5 *Lyræ.* Twenty-feet reflector. Equal, or nearly so; each of the 15th or 20th magnitude. Its existence cannot be even suspected with either of the equatoreals. The seven- and ten-feet reflectors (the former of 6, the latter of 9 inches aperture) in like manner fail to give any indication of it. ...... The 20-ft. reflector, however, shows a double star, whose distance is one-fourth that of ε from 5 (*i. e.* 53″) in the middle between them." (Phil. Trans. 1824, pt. 2.)

Dawes, in 1830, saw the *debilissima* [F, G] steadily with 3·8 inches, and also several much fainter stars immediately preceding: he assigns a mag. of 10½ to the faint pair. In 1833 he saw and marked down the approximate positions of AB, CD, E, F, G, H, I, J, K.

Lamont, in 1837, with the 11·2-in. refractor at Munich, made measures, and constructed a diagram, which shows AB, CD, E, F, G, H, I, J. Lamont appears to have been the first to make careful measures of the faint stars.

Lassell, observing with the 2-ft. Newtonian at Bradstones, near Liverpool, in 1856, detected a very faint star "north of the most following of the 'debilissimæ,' and about one-fifth of the way towards the northern double" (Monthly Notices, vol. xvii.). This is L on the diagram.

In 1871–2, Wilson and Seabroke examined the group, and made some measures of angle and distance.

In 1873, Dr. Prince drew attention to the relative brightness and positions of E, F, G, (Monthly Notices, vol. xxxiv.).

In 1876, Sadler and Ward detected a very faint star, marked M on the diagram.

Other faint stars are recorded as having been seen by Cooper Key, Grover, Holden, and other observers (see 'Astronomical Register,' 1864, 1871, &c.).

In conclusion, the stars may be grouped historically as follows:—

AB, CD, E, F, G, discovered by Sir William Herschel.
F, G, H, I, J, K, seen independently by Dawes, and H, I, J, K, first detected by him.
H, I, J, seen independently by Lamont.
L, first noted by Lassell.
M, detected by Sadler and Ward.
E, seen by all, of course.

The remainder were first carefully aligned or measured by Pratt, some of them having been previously seen by Cooper Key, Grover, Squire, Holden, and others.

H. Pratt.
J. Gledhill.

The Advance of Astronomy.—Part II.

The present condition of astronomy, with its vast and rapidly increasing store of accurate observations, offers many interesting subjects to the theoretical astronomer. The observations of the stars are now so numerous, and have been so fully reduced and criticised, and the time during which the observations have been made is so extended, that we shall soon have excellent data for a new and very exact determination of the constant of precession. The orbits of the planets and the moon, and their masses, are now so well known that little uncertainty can arise from this source; and by taking into the calculation a great number of stars in different parts of the heavens, we may be able to determine the motion of the solar system in space, as well as the constant of precession. The constant of aberration also needs a new determination; and since this constant is so closely connected with the theory of light and its velocity, and the methods of its determination are still under discussion, it would be well if several astronomers could determine this constant independently. The value we now use was found by W. Struve from prime-vertical observations, and is apparently very accurate; but no astronomical constant should depend on the work of a single astronomer with a single instrument, when it can be determined so easily and by other methods. The old method of finding the value of this constant from the eclipses of Jupiter's satellites may yet give us a trustworthy value. The value of the other constant necessary for the reduction of observations, that of nutation, must be nearly that found by Peters in his well-known investigation of this question. This value may be verified by a new series of observations of Polaris, or of the declinations of stars situated so that this constant has its full influence on the reductions.

There are many subjects in astronomy that need investigation, but in most cases the labour required is very great, and the completion of the work would occupy a long time. This follows of course from the fact that, with the refinement of observations and their exact reduction, many small terms must be considered which formerly could be neglected. The lunar theory has been a vexed question for the last two centuries, and may remain so for a long time to come. This will no doubt be the case until some able astronomer, with the will and perseverance of Delaunay, shall undertake its complete revision. This question should now be looked on as a purely scientific one, and its definite solution should be undertaken. The theory should not be patched up by guesswork to fit the observations, but should be carried out with the utmost rigour. This is a problem to which a young and able mathematician may well devote his life, and we must expect its solution from some such clear-headed devotee of science. Several of the planetary theories need a new investigation, and some of them are already in the hands of able astronomers. That of Mercury is especially interesting in connection with the intra-Mercurial planets, and it is to be hoped that Leverrier's theory of this planet may soon have a careful revision.

Again, among the secondary systems, the satellites of Jupiter and Saturn offer many interesting questions to the astronomer. At present the satellites of Jupiter demand a more complete theory, and new tables of their motions. Corrected elements of these satellites may be required for reducing observations of their eclipses, and for deriving a new value of the constant of aberration. These satellites form a peculiar and interesting system, and their theory is so complicated that the labour of correcting their elements and forming new tables would be great, but still within the power of a persevering astronomer. The recent discovery of the connection of comets with streams of meteors has given additional interest to cometary astronomy, and there is plenty of hard work to be done in reducing observations, in computing perturbations, and in deducing the best orbits of the comets. The periodical comets have another interest, since they may give us information concerning the matter filling space. It seems to be probable from different reasons, such as the consideration of the light of the stars, that there must be matter spread throughout the celestial spaces; but the only heavenly body that has directly given us information on this subject is Encke's comet, which has a period of 3⅓ years. For a long time the motion of this comet was very completely computed by Encke, whose calculations show very strong proof of a resisting medium. These calculations were continued by Von Asten, whose early death prevented him from finishing his work, and the theory of this comet is left in an unsatisfactory condition. It is very desirable that the motion of this comet should be completely investigated, and although the method of the special perturbations of the elements followed by Encke is probably the best that can be used, still in such a case it would be well to apply various methods. Here again, on account of the frequent returns of the comet, the labour of computation is very great, and probably would be enough fully to occupy the time of one astronomer. The interesting questions connected with the motion of this comet ought to induce some one to undertake this laborious work, and these questions are so important that two or three astronomers might well be employed on its theory.

The methods of astronomy have now become so well established, that the future advancement of the science is assured, especially since long intervals of time give an increased value to observations. Yet we may hope for improvement in instruments, for the introduction of new methods of observing, for better trained and more efficient astronomers; and perhaps also the rapid advancement of the physical sciences may furnish us with new and more powerful methods of investigation. There is an intimate relation between the instrument-maker and the astronomer, and they should understand each other better than is generally the case. It may seem a small matter that the divisions of a circle, or of a scale, should not be too finely or too coarsely cut, that the reading-scale should not be placed in an inconvenient position, and that the illumination of the instrument should be carefully studied and brought under the control of the astronomer; but these are really essential points, and, if not rightly arranged, are certain to weary the observer and to impair the quality of his work. Such mistakes will not be remedied until the makers better understand the uses of an astronomical instrument, and have correct ideas of the ends to be attained. Since our American opticians have placed themselves at the head of their craft, we may hope that our instrument-makers will do likewise, and that they will soon be able to furnish us with the best instruments of precision.

There is one point to which astronomers should give more attention, and from which we may reasonably hope that great advantages to astronomy may come; and that is to the selection of sites for new observatories. It is possible, perhaps probable, that our instruments may be greatly enlarged and improved, and that important discoveries and improvements in the manufacture of optical glass may be made; but it seems certain that we have within easy reach very decided advantages for astronomical work by the choice of better positions for our instruments. Very few American observatories have been established for the purpose of doing scientific work, or with much thought or care for their future condition; but generally they are built in connection with some college or academy, and are the product of local and temporary enthusiasm, which builds an observatory, equips it with instruments, and then leaves it helpless. The atmosphere that surrounds us, and its sudden changes of temperature, are the great obstacles to the good performance of a telescope; and the larger the instrument, and the higher the magnifying-power, the more serious are these hindrances. Now, with our present means of travel, we can easily place our instruments at an altitude of eight or ten thousand feet, and above a large part of the atmosphere. In this way we may be able to do with small instruments what at common altitudes can be done only with large ones; and when possible it is always better to use small instruments, since they are more easily handled, and are relatively stronger and better than large ones. Uniformity of temperature may be secured by seeking locations in the tropical islands, or on coasts like that of California, were the ocean winds keep the temperature nearly uniform throughout the year. At great altitudes we may secure a clearness of vision that would be of the greatest value in the examination of faint objects; and by this means, and by persevering and continuous observation, interesting discoveries may be made. It is a matter of course that, except in the case of comets, the future discoveries in astronomy will belong to faint and delicate objects; but these are interesting, and should not be neglected. A uniform temperature, which secures good definition and steady images of the stars, is necessary for accurate determinations of position, and for all measurements of precision. This condition is especially important in such work as that of stellar parallax, the determination of the constant of aberration, and wherever the yearly change of temperature may act injuriously. In the selection of better sites for observatories, I think we have an easy means of advancing astronomy.

As this science grows and expands, it will become more and more necessary to study the economy of its work, in order that astronomers may bestow their labours in the most advantageous methods, and may rid themselves of all cumbersome and time-consuming processes. The manner of publishing observations has already been much abbreviated, and improved I think, by some of the European astronomers, and this change seems destined to become universal. As the positions of many objects are now well known, the need of printing all the details of the observation, such as the transits of the wires, the readings of the micrometers, etc., is very slight; and this printing may be safely abandoned. Even this change will lead to a great saving in the time and cost of printing. But this will necessitate a more complete discussion of the work, and a more careful examination of the instruments—things to be desired, since they tend to lift the observer out of his routine, and make him a master of his business. There are objections to this change, and some of them are real, such as the importance of publishing a complete record; but this is overestimated, I think, since the original records ought always to be referred to in case of doubt; and other objections are factitious, such as the need of publishing a large and showy book in order to impose on the public.

We may hope also for improvements in theoretical astronomy, and for the better training and preparation of students of this science. I know that it is sometimes said that theoretical astronomy is finished, and that nothing more can be done. Such assertions come from professors who are old and weary, or from those young men who tire out early in life; but they are wrong. The improvements that Hansen has made in the theory of perturbations, and Poinsot's study of the theory of rotation, show what careful investigation may do, and assure us of further progress. It must be confessed that some of the astronomical work done in our country bears evidence that the astronomers did not understand the correct methods of reduction, and much of it shows evidence of hasty and ill-considered plans. This is perhaps a natural condition for beginners, but we trust that it has been outgrown. An actual need for the astronomical students of our country is a good book on theoretical astronomy, similar to Pontécoulant's work, in which the whole subject shall be presented in a complete form, such as we find in the 'Mécanique Céleste,' together with an account of the improvements made by Gauss, Poisson, Hansen, and others. There is no American book of this kind; and the English works are too partial, designed apparently to fit the student for college examinations, and not to give him a complete knowledge of the science. Such a book has hardly been attempted in our language, unless that of Woodhouse may be an exception; and it may be a long time in coming, since it requires a man qualified to do the work, and will involve an expense of labour in the preparation, and of cost in publishing, such as few are willing to incur. In the mean time it is far better for the student to go directly to the writings of Lagrange and Laplace, of Gauss and Poisson and other masters, rather than to spend time in reading second-rate authors who endeavour to explain them. And generally this will be found the easier way also, since the student avoids the confused notions and symbols, and the grotesque expressions and egotism of small men, and is lifted into the region of ideas and invention.

In presenting his exposition of the nebular hypothesis, which has since become so celebrated, Laplace says—"I present this hypothesis with the distrust which every thing ought to inspire that is not a result of observation or of calculation." It is a singular fact that, among all the writings on the nebular hypothesis, I have never seen a reference to this presentation of it by its most distinguished advocate; and yet this is the true spirit of scientific astronomy. Laplace did not wish to exempt his own theories from criticism, and neither should any one. In astronomy there is no final human authority, no synod or council, but simply an appeal to reason and observation. If a theory or a discovery be true, it will stand the test of observation and of calculation; if false, it must pass away to that Miltonian limbo where so many things have gone and are going. The question is sometimes asked, Of what use is astronomy? and the reply generally made is that it has conferred great benefits on navigation and on commerce, since it is by means of his astronomical knowledge that the sailor determines the position of his ship on the ocean. There is a truth in this reply, but it is only partial. The great value of astronomy is that it is really a science, and that it has broken the path and led the way through which all branches of science must pass if they ever become scientific. It is the spirit of honest, unrelenting criticism, and of impartial examination, that finally eliminates error and awards to every one his just due, that makes astronomy honourable and attractive; and it is by cultivating this spirit that astronomy confers its chief benefit, for it is this that shall break in pieces and destroy all false assumptions in science and in philosophy. ASAPH HALL

The Spectrum of Hartwig's Comet.

IN the last Number of this journal, Von Konkoly, of O' Gyalla, publishes observations upon the spectrum of this comet, which in some respects differ considerably from the results of my own measures. I therefore present my data, in hopes that the cause of the difference may, if possible, be detected, and the truth arrived at.

My observations were made with a single-prism spectroscope of the ordinary chemical form. The prism is of 60°, of white flint glass, the telescope and collimator each have an aperture of 31 millims., and a focal length of about 330 millims. The magnifying-power used was about 17, and the dispersion was sufficient to divide the D lines conspicuously.

There is a scale which can be illuminated at pleasure, visible by reflection from the surface of the prism in the usual way. The eyepiece has an occulting bar, movable by a graduated micrometer-screw, and a steel pointer of Vogel's pattern, either of which can be used at pleasure. A comparison prism attached to the slit gives the means of determining the constants of the scale at any time by bringing known spectra into the field.

The view-telescope is movable by a tangent-screw, and in observation was always so placed as to *bring the line measured into the centre of the field of view*, an essential precaution in order to avoid the errors of instrumental parallax arising from the variations in the focal length of the lenses for light of different colours. The spectroscope was attached to the 9½-inch equatoreal of this observatory.

The observations were made on the nights of Oct. 2, 3, and 7.

On the first night the spectroscope was not fully adjusted, the comparison prism and electrode-holders not being satisfactorily arranged. Nothing further was done than to satisfy ourselves that the spectrum was of the usual type, agreeing sensibly with that of the Bunsen-burner flame. There was a considerable continuous spectrum from the nucleus. The bands, with a narrow slit, were very well defined at the lower (less refrangible) edge, and showed a strong tendency to break up into lines. I could not, however, hold any of the lines steadily enough for a satisfactory pointing.

On Oct. 3 every thing was in order, and a number of readings were made on each of the three bands of the comet spectrum, and on certain lines of comparison spectra selected to determine the scale-constants.

For the comet spectrum I obtained :—

Lower edge of lower band, scale-reading 70·07 (mean of 3).
„ „ middle band, „ 80·90 (mean of 5).
„ „ upper band, „ 78·35 (mean of 3).

In no case did any reading differ more than $\frac{1}{10}$ of a division from the mean as given.

For the scale-constants I obtained :—

Lower band, Bunsen flame	(λ=5635),	scale 68·3
Middle band, „	(λ=5165),	„ 81·0
Upper band, „	(λ=4740),	„ 98·2
D, middle of pair „	(λ=5892),	„ 62·0
Lower magnesium line	(λ=5527),	„ 71·1
b_1, magnesium line	(λ=5183),	„ 80·7
F, hydrogen β	(λ=4861),	„ 92·4

(I have used the wave-lengths for the Bunsen-burner spectrum given by Dr. Watts in 'Nature' for May 8, 1879.)

The readings on the comet were made by bringing down the occulting bar until the edge of the band just remained visible, and then illuminating the scale. Of course the observations could not be made very rapidly in this way, as it always required two or three minutes for the eye to recover its sensitiveness after each scale-reading. The reference spectra were sufficiently bright to admit of direct comparison with the scale.

From these data I deduce, by a graphical process :—

Lower edge of lower band in comet-spectrum λ=5564
„ „ middle „ „ 5169
„ „ upper „ „ 4737

The *possible* error for the middle band cannot, I think, exceed 10 units, and for the other two about 15 or 20.

Von Konkoly resolved the lower band, and gives determinations of the wave-lengths, both of the lower edge of the band and of the line observable in the band.

His numbers run as follows, taking the mean of his three closely accordant measures :—

Lower edge of lower band	$\lambda=5610$
Line in lower band	5492
Lower edge of middle band	5165
Lower edge of upper band	4857

As regards the middle band our agreement is practically perfect; but there is a difference of 56 units between us for the lower band and 120 units for the upper. It is impossible to form a judgment as to the cause of the discrepancy without knowing more about the manner in which the wave-length constants of the O'-Gyalla instrument were found. The fact that readings are given upon the spectrum of a Geissler tube containing C_2H_2 suggests the idea that these constants may depend upon the tube-spectrum; and then the question arises whether the tube may not, under the circumstances, have been giving the *first* and not the usual *second* spectrum of carbon. If so, our results would be perfectly accordant. As my own scale-values depend upon the spectra of magnesium and sodium, as well as upon that of the Bunsen flame, I do not see how they can well be wrong.

I may add that in 1871 I obtained from Encke's comet the values 5575, 5174, and 4702, though I should not claim for them quite the same precision as for the present determination.

On Oct. 7 I attempted to make a closer determination of the wave-lengths by using a grating in place of the prism, but the comet was too faint. The bands were visible, but it was necessary in order to see them to open the slit too widely to permit of accurate measurement.

As to the question whether the spectrum of the comet is identical with that of carbon, and if so, with which of the carbon spectra it agrees, I can only say that while, according to my measures, the correspondence of the middle and upper bands with the *first* carbon spectrum is satisfactorily close, the lower band presents a serious discordance, so great that I should hesitate to ascribe it to mere error of observation.

C. A. Young.

Princeton, U.S.,
Nov. 9, 1880.

The variable Star h No. 78 near the Trapezium of Orion.

Mr. Gledhill has very seasonably drawn attention to this object in the last Number, and it is to be hoped that observers will follow his suggestions as to making systematic observations of it during the winter. Of such a minute object it is difficult to decide as to its supposed variability unless several simultaneous observations should be made of it; for, as instanced by Mr. Gledhill, it is recorded as visible at Pulkowa on one occasion when it was invisible to Bond at Cambridge, U.S.A. For the last six years I have had in progress a survey of the nebula and stars surrounding θ Orionis, which I intend to make as complete as possible. During that time I have observed *h* 78 at times, but have never yet satisfied myself that it was really variable. If the night is suitable I expect to see it, but a really transparent atmosphere is necessary. On Nov. 3 I saw it immediately I turned my telescope (8·15-in. With) on the nebula, the air being very transparent but slightly unsteady.

It will be necessary to note that Sir John Herschel's α and β are the two *following* stars of the trapezium and that his 78 is *slightly following* a line drawn through these two stars and produced in a north-easterly direction about as far as the space between the two stars. A short distance north of his 78 is 75, a little following a line through γ and δ, the two preceding stars of the trapezium. Observers who may turn to the "Cape Observations" must remember that Herschel's drawing is reversed left and right, in accordance with the image in his (front-view) form of reflector, so that the preceding occupies the position of the following side as usually observed. On Nov. 3 I saw 75 very easily; it was much brighter than 78, although Herschel carefully estimated them both as 18 mag. while he was at the Cape. Close to 75 are two exceedingly small stars forming with it an equilateral triangle. On Nov. 3 I saw these more easily than 78. They were frequently seen by Bond and are given in his map (Annals of Harvard Coll. Observatory, vol. v.). The power I used on Nov. 3 was 220.

18 Preston St., Brighton. HENRY PRATT.

Double Stars for December.

THE following pairs have been very recently measured by the Rev. Jevon J. M. Perry with his fine $18\frac{1}{4}$-inch reflector or by myself with the Bermerside $9\frac{1}{3}$-inch refractor. These measures may be interesting to other amateurs just now. See also 'The Observatory,' Vol. ii. p. 269, Vol. iii. p. 245.

Σ 2. R.A. $0^h\ 2^m{\cdot}5$, Dec. $+79°\ 1'$. Mag. 6·3, 6·6.

This star was examined on two good nights, but no *certain* elongation could be seen.

Σ 13. R.A. $0^h\ 8^m{\cdot}9$, Dec. $+76°\ 17'$. Mag. 6·6, 7·1.

95°·3 0″·7 1880·79 Gl.

OΣ 4. R.A. 0^h 10 ·4, Dec. $+35°\ 48'$. Mag. 7, 8.

152°·3 Elongated. 1880·83 Gl.

OΣ 12. R.A. $0^h\ 25^m\cdot 1$, Dec. $+53^\circ\ 52'$. Mag. 6, 6.

142°·0	0″·51	1880·83	P.
138 ·2	0 ·4	1880·81	Gl.

OΣ 20. R.A. $0^h\ 48^m$, Dec. $+18^\circ\ 32'$. Mag. 6, 7.

180° ±	Separated.	1880·83	P.
187 ±	Not separated.	1880·83	Gl.

Mr. Perry finds this far more difficult than 85 Pegasi.

OΣ 38. R.A. $1^h\ 56^m\cdot 3$, Dec. $+41^\circ\ 44'$. Mag. 5, 6.

95°·3	..	1880·79	P.
101 ·0	..	1880·80	„
94 ·8	0″·4	1880·80	Gl.
104 ·3	..	1880·83	„

The following interesting pairs have also been examined on fine nights:—

δ Cygni: the colours always appear as follows:—A bright yellow or whitish yellow; B dusky yellow.

γ^2 Andromedæ: A clear blue always; B bright yellow or yellowish.

85 Pegasi. This most difficult object is no doubt beyond the power of our instrument, except perhaps on the very finest nights. Mr. Perry, however, can both see and measure it. He writes, "1880·8378, Position 324°·5, distance 0″·72. This is a very difficult object, but with the full aperture and a power of 600 the companion was clearly seen."

J. Gledhill.

Mr. Edward Crossley's Observatory,
Bermerside, Halifax.

Meteor Notes for December *.

The Geminids recur on the 9th–12th and should be looked for after midnight on those dates, when the Moon will have set and the radiant attained a fair elevation. The position of this radiant is close to α Geminorum, and there is a contemporary shower further south. On about the 8th there is a conspicuous shower of swift streak-leaving meteors from a point at 145° +7° between Leo and Sextans; and there are now many radiants in Ursa Major and Ursa Minor, Cancer, Leo, and Leo Minor which cannot fail to be revealed by long-continued observation.

Large meteors should be expected on the nights of the 8th–9th, 11th–12th, and 21st.

* See 'Observatory,' Vol. i. p. 259, Vol. ii. pp. 270–271, and Vol. iii. pp. 246–249, for further notes on December showers. It is proposed with the present month to discontinue this series of papers. Intending observers will be able to refer back to the repertory of meteor-showers they contain for such information as is required.

Recent Observations.— Col. Tupman has reduced the observations of the fireball of 1880 Aug. 12, $8^h\ 17^m$ G.M.T., from which it appears that "it was first seen from York at the height of 72 miles over Watlington (Oxon), and last seen from Cookham near Maidenhead, at the height of 11 miles over a point 21 miles from Aberystwith, in the direction N. 80° E. Total length of path 142 miles. Parabolic duration 7⅓ seconds. 88 miles of the path were seen from York, with an estimated duration of 2½ to 3½ seconds. The Cookham and York observations define the radiant at 317° +4° and the earth-point from York S. 55° W., 16 miles N. of Aberystwith. The apparent radiant being nearly opposite the Sun, the aberration of the radiant is at the maximum 45° for a parabolic orbit, with the following elements:—

	°
Long. of Node	140·5
Inclination	19·5
Long. of Perihelion	45·4
Log. Perihelion distance	0·551
Motion direct.	

"The aphelion is situated in ecliptic longitude 225½°, latitude +19°.

"The longitude of the apparent radiant-point being nearly $= \odot + 180$, it follows that six months afterwards when $\odot = 320\frac{1}{2}$, which occurs Feb. 9, the Earth again nearly intersects the meteor orbit. If a stream of meteors be moving in such an orbit their radiant-point would be, Feb. 9, 327° −34°."

Mr. H. Corder, at Chelmsford, observed 70 meteors between Oct. 30 and Nov. 4. The majority of these were slow ones from Taurus and Musca.

Mr. E. F. Sawyer, at Cambridgeport, Mass., noted 104 meteors between Sept. 23 and Oct. 9. The chief radiant was one of slow meteors from near γ Arietis at 24° +17°, which furnished 10 paths, with an average length of 7° and duration about 1 second. Mr. Sawyer points out that this radiant nearly coincides with that of the bright comet of 1769, Sept. 28, 24½° +17°. He had previously noticed the shower in 1878 Sept. 30–Oct. 2 at 23° +17° (7 paths).

Several meteors of rather exceptional brilliancy have recently been recorded.

I. Oct 9, $10^h\ 25^m$. An apparently stationary meteor, > 1st mag., near γ Orionis and of a deep orange colour. While noting exact place, it very slowly began to descend towards the horizon, occupying 6 seconds in traversing an observed path of 10°. It was only 3rd mag. at disappearance. Course from 76° +5° to 76½° −5°. It was one of the slowest meteors ever seen by the observer, and its motionless aspect, when first noticed, was very singular.—*E. F. Sawyer*, Cambridgeport, Mass.

II. Oct. 29, $9^h\ 50^m$. A fine meteor observed here with a track from about 31° −1½° to 16° −17°. At the moment preceding its extinction its brightness considerably exceeded that of Jupiter. Colour a brilliant bluish white. Duration 1½ sec.—*J. Baxendell*, Southport.

This meteor was also recorded at Bristol by the writer. It gave two bright flashes and left a broken streak for 25 secs. Time $9^{h}\ 50^{m}$, path 38° +18° to 26° +22°, length 12°. Radiant-point at 46° +15°, from the combined paths.

III. Oct. 30, $7^{h}\ 20^{m}$. =Sirius. A yellow fireball with train of sparks. Path 103° +55° to 84° +35°.—*H. Corder*, Chelmsford.

Also recorded at Bristol. Time $7^{h}\ 24^{m}$. =♃; path 337° +36° to 343° +8°; length 28°; not very swift. Radiant-point at 185° +68° near χ Draconis.

IV. Oct. 30, $7^{h}\ 55^{m}$. =♂. Orange. Quick meteor with a fine streak. Path from about 330° +57° to 270° +37°.—*H. Corder*, Chelmsford.

V. Nov. 1, $7^{h}\ 40^{m}$. =1st mag. Bluish white. Path 277½° +39° to 267° +26°; 1 sec.—*J. Baxendell*, Southport.

VI. Nov. 1, $10^{h}\ 50^{m}$. =♃. Bluish white with train of orange flakes. Path 275° +56° to 257° +43°. Radiant same as No. II., 46° +15°.—*H. Corder*, Chelmsford.

VII. Nov. 3, $9^{h}\ 58^{m}$. =♃. A green fireball with yellow spark train Duration about 4 secs. Path 100° +26° to 116° +26°.—*H. Corder*, Chelmsford.

This meteor was also observed by Mr. Christie at Greenwich at $9^{h}\ 55^{m}$. It was =♃, with path from 109° +25° (approx.) to 124° +23°. Duration about 1 sec. The meteor died away in a dull red streak.

VIII. Nov. 3, $10^{h}\ 39^{m}$. A sudden meteoric flash = Sirius or ♃, apparently stationary, but not well seen, at 28°—18°.—*H. Corder*, Chelmsford.

IX. Nov. 4, $9^{h}\ 59^{m}$. Large meteor lighting up the whole sky with green flashes, and leaving four short streaks. The path was pretty accurately noted from 69° −4° to 72° −9°. It shows a radiant with a 1st-mag. green meteor and the green fireball in Gemini (No. VII.) at 55° +17°, and so belonged to the Taurids.—*H. Corder*, Chelmsford.

X. Nov. 8, $14^{h}\ 0^{m}$. A large meteor >♃ shot nearly in a line from 5½° +22° to 344° +32°. It left a short tail of sparks following immediately in its wake.—*F. C. Dennett*, Southampton.

XI. Nov. 17, $7^{h}\ 47^{m}$. Very fine meteor >♃. Duration 4 secs. It left a thick train of sparks. Path from 344° −1° to 321° −14°, =26°. Seen by the writer at Bristol.

It was recently pointed out ('Observatory,' Meteor Notes for Oct. and Nov.) that large meteors should be expected on Oct. 29, Nov. 1, 2, and 4, and these fireball-epochs are now abundantly confirmed by the preceding observations. The Taurids and an equally imposing shower from between Taurus and Aries (and S. of Musca) supply most of the very fine meteors seen at this period.

W. F. Denning.

CORRESPONDENCE.

To the Editor of 'The Observatory.'

Colours of Double Stars.

Sir,—

I am much obliged to Mr. Gledhill for drawing attention, in your October number, to this important subject, as well as for the list of colour-estimates of δ Cygni which he has there given. That which blocks the way of every theorist, and renders his results uncertain, is, as he truly says, the want of a definite scale of tints. I would suggest that such a scale is at hand, ready numbered, in the solar spectrum. The tint of any line is uniform, or at least varies but infinitesimally, and in carefully constructed grating instruments would form an unvarying standard. The personal colour equation of the observer would be eliminated, it would equally affect the star and the spectrum, and all indefinite words, as greenish, &c., would be done away with. This scale could be adopted with equal ease in all parts of the world, thus securing uniformity.

How important the results of systematic work in this direction would be, may be inferred from M. Niesten's suggestive treatment of the subject. It has been too long neglected, and would yield results of the highest value if pursued. Stars whose spectrum and its variations cannot be observed, owing to their faintness, might, in great numbers, yield information bearing directly on the great problems now opened up by spectroscopic work.

Trusting that this subject will receive attention,

I am, Sir, yours faithfully,
Edward B. Kirk.

Saltcoats, Ayrshire, N.B.
1880, October 26.

Visibility of Venus near Conjunction.

Sir,—

In the Supplementary No. of the 'Monthly Notices,' recently published, we have a continuation of the contributions of Messrs. Sayce and Bosanquet on the subject of ancient Babylonian astronomy. The question of the visibility of Venus when in proximity to the Sun is brought forward. It appears that only in two cases throughout the genuine portion of an inscription on the planet, is the interval between disappearance at sunset and reappearance at sunrise stated to have been 7 days, but that "throughout the portion which we consider not genuine, this interval is uniformly stated to be 7 days" (p. 576, Supplementary No.); and again, "We beg to call the attention of astronomers in tropical countries to the question at what distances from the Sun Venus can actually be seen at sunrise or sunset. But in the meantime we consider the interval of 7 days as highly doubtful, and casting a doubt upon an observation in each case where it occurs. Its uniform occurrence in the spurious part of the tablet is in accordance with this opinion."

In connection with this, it may be worth stating that on the evening of February 17, 1878, I noticed Venus distinctly with the naked eye only 4 days before conjunction*, but I am unable to state how many days afterwards it was visible the other side the sun. As this was in our own country, the planet should be seen a little nearer conjunction in tropical lands.

Faithfully yours,
S. J. JOHNSON.

Abbenhall Rectory,
Gloucester, Nov. 13.

Jupiter's Northern Hemisphere.

SIR,—

By this I mean that portion of the planet north of the great equatoreal belt. This region seems usually to be the most undisturbed portion of the planet, but, just now, is an object of great interest.

During the early part of the present season this region was noticed to be crossed about midway (apparently) between the N. equatoreal belt and the pole by a beautiful pink belt, somewhat similar to the red spot in tint, though perhaps lighter and more vivid. A little N. of this a dark belt was visible, bounding the polar cap, which in a fine air sometimes appeared composed of several belts. This belt has at times appeared blotched or broken, instances occurring on July 22, long. 67°; Aug. 10, 26°; Sept. 5, 236°, 256°, and 326°; and on Sept. 18, in long. 100°. On Sept. 27 a break was observed in the red belt above mentioned, long. 347°, and on Sept. 30 this same belt was seen to be considerably enlarged from long. 60° to 112°. But the most singular phenomena are to come. About midway between the N. equatorial belt and the red belt before mentioned there has been a belt of a fine azure tint which has been visible, if I interpret my drawings aright, since 1877, though at that time it appears to have been situate somewhat further north. Between this belt and the equatoreal zone, in a moderately fine air, there has been situate a very fine belt of a similar colour. Between the blue belt and the red one, on the finest evenings, another blue or grey belt, still finer, has been visible.

At midnight on Oct. 17 I noticed two singular dark spots on the blue belt, as nearly as I could estimate in long. 98° and 118°. I had not another opportunity to observe them until Oct. 23, when I was surprised to find that the long. of the *preceding* spot was 57°. By Oct. 29 the *p.* spot had reached long. 354°. Between Oct. 30 and 31 their speed seemed to slacken slightly, but since that date it has seemed to increase rather than diminish. The mean rotation of these spots between Oct. 23 and Nov. 17 appears to be $9^h\ 37^m\ 39^s{\cdot}36$. Since first observed, *i. e.* in exactly one month, they appear to have travelled round the entire planet, with the exception of some 11° of longitude—which means a mean motion of something very like 327 miles an hour, taking into consideration the latitude of the spots. These spots are doubtless those mentioned by Dr. Schmidt.

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* [*Cf.* 'Observatory,' No. 42, p. 573, "Visibility of Venus in the Daytime," by Harry L. Baldwin.—ED.]

I am, Sir, yours very truly,
FRANK C. DENNETT.

Southampton, Nov. 18.

[The dark spots above mentioned seem to have increased in size and number and now present the remarkable appearance of a belt of black dots extending right across the disk.—ED.]

The Markings on Jupiter.

SIR,—

There is a conspicuous white spot visible, just S. of the Jovian equator, which has maintained a permanency and distinctness of form quite unusual with these objects. It was first noticed on Sept. 18 at $11^h\ 50^m$ in crossing the central meridian of Jupiter; and it has been reappearing since at intervals of $9^h\ 50^m\ 5^s$. It has a rapid motion, relatively to the position of the red spot, from west to east around the planet. On Nov. 19 it was on the central meridian at $9^h\ 23^m$, and in precisely the same longitude as the middle of the red spot, yet on the 17th it followed the red spot by 30^m. Let us compare the observed times of a few recent transits, which will sufficiently show the remarkable differences of motion:—

	Red Spot.		White Spot.		Difference.	
	h	m	h	m	h	m
Nov. 2........	10	26	14	15	+ 3	49
3........	6	12	9	47	+ 3	35
8........	5	21	7	48	+ 2	27
17........	7	44	8	14	+ 0	30
19........	9	23	9	23	0	0
20........	5	14	5	2	− 0	12

Thus the swifter movement of the white spot has enabled it to overtake the red, after gaining on it about $13\frac{1}{2}^m$ per day.

I have been observing two other white spots on the equator with the idea of ascertaining whether they participated in the same rapid motion, and find the results are perfectly accordant. In one case the observed period was $9^h\ 50^m\ 6^s$, and in the other $9^h\ 50^m\ 3^s$. These periods are subject to some slight corrections; but it is impossible they can be very far wrong.

Last night the bright spot first referred to had nearly disappeared. If visible, it should be near the central meridian of Jupiter at the following times:—

	h	m
Dec. 1....	6	37
2....	12	7
3....	7	48
4....	13	18
5....	8	59
Dec. 6....	4	38
7....	10	9
8....	5	49
9....	11	19
10....	6	59

I am, Sir, yours faithfully,
W. F. Denning.

Bristol, Nov. 27, 1880.

Bright Spot on Saturn.

Sir,—

Observing Saturn on Nov. 17, $9^h\ 10^m$, I obtained several steady views of a bright spot on the S.S.W. margin of the Ball. It rapidly became fainter, and at $9^h\ 20^m$ had disappeared, having probably been carried round the limb by the effects of rotation.

Definition was very sharp. I saw Encke's division in the outer ring (A). It was plainest in the west ansa.

Prof. Asaph Hall, from observations of a brilliant spot from Dec. 7 to Dec. 13, 1876, determined the rotation-period of Saturn as $10^h\ 14^m\ 28^s{\cdot}8$; and it would be interesting to learn whether the present spot can be identified with that seen at Washington in 1876.

My telescope is a Browning-With reflector, of 10 inches clear aperture; power 450 is generally most effectual on Saturn.

I am, Sir, yours faithfully,
W. F. Denning.

Bristol, Nov. 20, 1880.

Bright Meteor.

Sir,—

On the evening of Nov. 17, at $6^h\ 54^m$ G.M.T., a brilliant streak-leaving meteor, much exceeding Jupiter in magnitude, was observed here in the constellation Auriga. Origin of path R.A. 6^h, Dec. 33° N.; place of extinction R.A. $6^h\ 50^m$, Dec. 30° N.

The places above are only approximate (especially the origin) on account of all the small stars being obliterated by strong moonlight. The direction of flight was nearly at an angle of 45° to the vertical, as seen from here.

I am, Sir, yours truly,
John Robertson.

Coupar Angus,
1880, Nov. 20.

NOTES.

Solar Physics.—Professor Balfour Stewart and Mr. Dodgson have sent a preliminary report to the Royal Society ('Proceedings,' xxix. p. 303), in which they investigate the diurnal ranges of temperature at Kew, Toronto, and Utrecht, and the diurnal ranges of magnetic declination at Kew and Prague. The results are, that the ranges of temperature exhibit certain common periods (around 24 days); that similar phases appear to occur at Toronto eight days before they occur at Kew, and at Kew one day before they occur at Utrecht. The declination-ranges at Kew and Prague exhibit certain common periods, which may be regarded as the same, or very nearly the same, as the above-mentioned meteorological periods, the similar magnetic phases appearing to occur at Kew about one day before they occur at Prague. Sun-spot records from 1857–68 appear to show certain solar inequalities very like the magnetic and meteorological inequalities in point of period.

M. Janssen continues his photographic observations of the surface of the sun, and has, in the 'Annuaire du Bureau de Longitude' for 1879, and in the 'Report of the British Association,' given an account of recent progress in solar physics, particularly of what he supposes to be a new discovery—the maximum of photographic intensity of the spectrum for iodide of silver near G, which with short exposures makes the spectrum very nearly monochromatic. As remarked in the *British Journal of Photography*, No. 1009 (vol. xxvi.), this is by no means a new discovery, but a fact known since the earliest days of photography.—*J. L. E. Dreyer.*

SUPPOSED INTRA-MERCURIAL PLANETS.—In the 'Astronomische Nachrichten,' No. 2254, Professor Peters discusses some of the so often quoted observations of black round spots passing over the Sun, nearly always made by obscure amateurs—spots which have never revealed themselves to the regular Sun-observers, nor appeared on the photographs of the Sun, which have now, during not a few years, been taken, on every clear day, at several observatories. After having alluded to the spot seen by Weber, which, but for its being found on the Greenwich and Madrid photographs, would now have been considered a well-authenticated apparition of an intra-mercurial planet, seen by an experienced observer, Professor Peters especially examines the observation by Lummis, on March 20, 1862, which is considered as very certain by Leverrier. By referring to his own diary, and to Professor Spörer's observations, he shows how two ordinary spots have been noticed by Mr. Lummis, first one, and then twenty minutes after the other, which he thought was the same as the first one, which had moved in the meantime. He shows how the second position was vitiated by an error, which being removed, the estimated distance of 12′, which was in contradiction to the sketch sent by Mr. Lummis to Mr. Hind, comes out all right.

Almost simultaneously with the publication of Professor Peters's article, another observation of a spot in transit by De Vico on July 12, 1837, preserved by De Cuppis (who saw a similar spot himself in 1839), has been announced in the *Comptes rendus*, vol. 88, No. 9.—*J. L. E. Dreyer.*

COMETS.—In a short communication to the British Association Mr. G. J. Stoney put forward the hypothesis, that the bright lines in the spectra of comets are due to the Sun's light falling upon the compound of carbon in them, and rendering it visible in the same way that light renders other opaque bodies visible, the vapour being opaque in reference to the particular rays which appear as bright lines in the spectrum. The visibility of comets he attributes to phosphorescence, the conditions for the production of which are present in the very attenuated vapour.

Professor H. A. Newton communicated at the same meeting a short paper on the direct motion of periodic comets of short period. If these comets originally came from interstellar space, they must have been turned into the short orbits by coming very near to a large planet, the disturbing influence of which diminished their velocity. It is now shown in the paper that nearly all comets which are thrown into an orbit of short period in this way will have a direct motion in the latter. This is in accordance with experience, as only Halley's comet and the Leonid-comet (1866 i) have retrograde motions.—*J. L. E. Dreyer.*

ZONE OBSERVATIONS.—Dr. Auwers has published, for the zone-committee of the Astronomische Gesellschaft, an important work, 'Fundamental Catalog für die Zonen-Beobachtungen am nördlichen Himmel.' (Leipzig, Engelmann, 4to.) This catalogue of 539 stars, on which the places of all the stars between −2° and +80° Decl. down to the 9th mag. inclusive will depend, is founded on the following systems of standard places:—Pulkowa, 1845 and 1865; MS. of Pulkowa Observations, 1869–74; MS. of Bradley's Stars from Greenwich Observations 1836–72, on the system of the First 7-year Catalogue; Greenwich 9-year Catalogue; Harvard College Observatory, Vol. x.; Engelmann's Declinations, 1866–70; Leiden Declinations, 1864–70.

At the Berlin meeting of the Astronomische Gesellschaft reports on the Zone-observations were read. These will, as usual, be found in Part IV., for 1879, of the Vierteljahrsschrift, from which the following account is taken:—

80°–75°.	Kasan. Observations finished, except a few in Zone 75°–76° yet to be made; reductions far progressed; printing commenced.
75–70.	Dorpat. Only 920 observations yet to be made: more than half the reductions to 1875·0 finished.
70–65.	Christiania. Zones finished; about 90 per cent. reduced.
65–55.	Gotha (formerly Helsingfors). Zones finished; reductions far advanced.
55–50.	Cambridge, U.S. Observations finished; reductions progressing well.

50°–40°. Bonn. About 5000 single observations remain to be made; reductions far advanced.
40–35. Lund. Observations commenced in September, 1878; the reductions are progressing with the zones.
35–30. Leiden. Finished; Vol. 5 of the 'Annalen,' containing the second half of the zones, is in the press.
30–25. Cambridge (England). About 4000 single observations to be made yet. The mean places are calculated up to the end of 1874, apparent places to the end of 1876.
25–20. Berlin. Not yet commenced.
20–15. Berlin. Finished, but not yet reduced.
15–10. Leipzig. Zones finished; R.A.'s almost all reduced to 1875·0; zeros for declinations partly computed.
10–5. Leipzig. Has been commenced.
5–1. Albany. Observations were commenced in August, 1878; The influence of magnitude on the transit has been investigated by wire-screens, as suggested by Mr. Gill. Correction to observed transit $= -0^{s}{\cdot}009\,(M-4{\cdot}0) - 0^{s}{\cdot}00037\,(M-4{\cdot}0)^2$, where M is the magnitude.
+1 to −2. Nicolajeff. Much interrupted by the Russo-Turkish war.—*J. L. E. Dreyer.*

COLOURS OF STARS.—Julius Schmidt has, in continuation of his researches on the colours of stars, published an extensive series of colour-estimations, made chiefly with the finder of the Athens refractor, from 1872–78. Only for Arcturus has he been able, with any certainty, to find a variation of colour. It is mostly very bright stars he has observed, and he investigates the difference in the estimation, according to whether the finder or the refractor was used, and finds a greater difference the nearer the colour is to white.—*J. L. E. Dreyer.*

ON THE COLOURS OF STARS*.—Since 1866, Herr Schmidt of Athens has regularly made eye-observations of the colours of stars, estimating the amount of red on a scale from 0 to 10. In his latest communication he discusses (1) the change of colour with increasing zenith-distance and (2) the change of colour of variable stars. To determine the first of these he has made numerous observations of Venus from Z.D. 66° 55′ to Z.D. 89° 57′ and finds the curve which expresses the colour on his scale at any Z.D. from 72°·5 to 89°·5.

As regards variable stars, he finds that those which he discusses, viz. δ Cephei, η Aquilæ, ζ Geminorum, β Lyræ, Mira Ceti, κ, U, and γ′ Sagittarii, are very slightly redder at minimum than at maximum; but the change of colour indicated is almost insensible and has no practical value. Herr Schmidt, however, does not include in this investigation any of the variables of the type of α Orionis or η Pegasi. His conclusion is that there is no ground for assuming a change of colour with change of brightness, and that to investigate this matter properly other means of observation are necessary. He considers that the true colour of a star can only be determined when a firm foundation has been laid for a scale of colours, and that this must, in his opinion, have reference to the spectrum.

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* Astron. Nachr., No. 2334.

ASTRONOMICAL CONSTANTS.—"Répertoire des constantes de l'Astronomie, par M. J. C. Houzeau," occupies 271 pages in 4to of the 'Annales de l'Observatoire Royal de Bruxelles,' Nouvelle Série, t. i. This is a most wonderfully useful publication, and it is hard to understand how astronomers hitherto have managed to get on without such a handbook; it is only a great pity that it has not been published in the shape of a handbook, but in a place where it will not be easily accessible to many people. It gives in twenty chapters, chronologically arranged, lists of all the different values of astronomical constants, which have been published from the earliest times and down to 1877, in all cases describing the way of finding the particular value and adding exact references, by means of which any one can find out for himself all about any value. And more than that, the titles of the principal books and memoirs relating to any subject within the range of the repertory are also given, but completeness is not aimed at in this respect. As far as we have been able to ascertain, the lists of constants are very complete and very correct, and will save from oblivion many a thing which has been often hidden by its author in some obscure corner of the literature.—*J. L. E. Dreyer.*

THE CORK OBSERVATORY.—The new observatory of Queen's College, Cork, though only furnished with comparatively small instruments, promises to become one of the most remarkable astronomical institutes existing, on account of the uniqueness and refined construction of its instruments. As these have been described at some length in the Proceedings of the Royal Dublin Society, by their constructor, Mr. Howard Grubb, it will here only be necessary to note, that the principal instruments are to be an 8-inch equatoreal, a 5-inch transit-circle, with 20-inch circles of glass, and a 4-inch siderostat of novel construction. The telescope of this instrument points towards the south pole, and carries outside the object-glass a plane silvered mirror, which by the rotation of the tube round its optical axis (either by hand or clockwork) will keep the object under examination in the centre of the field. The equatoreal, for which Mr. Grubb obtained the Gold Medal at the Paris Exhibition, is furnished with all the latest improvements, including arrangements for reading both circles from the eye-end, electrically-controlled clock-movement, a duplex micrometer, like the one recently made by Mr. Grubb for the University Observatory, Oxford, &c. It is to be hoped that with such an equipment the Cork Observatory will take its place among the active astronomical establishments of this country. —*J. L. E. Dreyer.*

DUNSINK OBSERVATIONS.—Nearly the whole of Part III. of the 'Astronomical Observations and Researches made at Dunsink' is devoted to annual parallax. The first paper contains a discussion of observations of the planetary nebula H. IV. 37 from August, 1871, to August, 1872, by Dr. Brünnow. The nebula has in the centre a well-defined point resembling a star of the eleventh magnitude. This was compared in declination with a star of the tenth magnitude *np*, and the parallax was found to be perfectly insensible, a result which agrees well with that of a similar series of observations by Professor Bredichin, of Moscow.

A new determination of the parallax of 61 Cygni, by Dr. Ball, forms the subject of the next paper. This determination differs from others of the same object in one particular, viz. that the preceding star instead of the following one has been observed. The resulting parallax is 0″·465, in pretty good accordance with the results of Bessel (last three months 0″·54), Struve (0″·51), and Auwers (0″·56). [Dr. Ball has given the substance of this paper in the 'Observatory,' No. 17, vol. ii. p. 137.]

A third paper, also by Dr. Ball, describes the first results of a series of reconnoitring observations in search of stars with a large annual parallax. These observations are only intended to reveal large parallaxes (0″·7 or more), and each object is only observed twice a year with six months' interval, when it is 90° from the Sun, and at the two extremities of the major axis of the parallactic ellipse. The paper contains the discussion of the observations of forty-two objects, chiefly red and variable stars. The result is that in almost every case is the parallax certainly less than 1″, and most probably does not exceed 0″·5. These objects will therefore not be observed any further, but it is Dr. Ball's intention to continue this kind of observations until about 1200 objects have been examined; his working list contains red and variable stars, stars with a large proper motion, and others chosen for various reasons, nearly all north of the 30° parallel.—*J. L. E. Dreyer.*

DETERMINATION OF ORBITS OF COMETS AND PLANETS.—The second volume of Oppolzer's 'Lehrbuch zur Bahnbestimmung der Kometen und Planeten' appeared in October last, nearly ten years after the first volume. Such a work was indeed a desideratum. Though no student of astronomy ought to neglect reading the 'Theoria Motus' and Olber's celebrated 'Abhandlung,' still there are so many other papers with which it is necessary to become acquainted before attempting to compute the definite orbit of a planet or a comet, and so many little dodges to learn, that a book like Oppolzer's really supplies a great want. The book enters into the most minute details, and gives examples fully worked out to an extent which neither Watson's excellent 'Theoretical Astronomy,' nor the shorter, though in some parts more comprehensive, 'Theoretische Astronomie' by Klinkerfues, have attained.—*J. L. E. Dreyer.*

THE SUN-SPOT PERIOD[*].—Prof. Spoerer has discussed Carrington's and his own observations of Sun-spots from 1854 to 1878 and obtained curves which agree remarkably with those deduced by Prof. Wolf by a different mode of treatment from his relative numbers. He has also combined with these Wolf's relative numbers for the years 1752 to 1853, and from these 11 periods has obtained two formulæ to express the spot-frequency about the times of maximum and minimum respectively. First, however, out of the 22 groups of observations about minimum and maximum, he rejects 5 near minimum and 6 near maximum which he considers abnormal, and from the remaining 6 groups near minimum deduces the formula, $n = 10{\cdot}7 + 6{\cdot}0\, t^2 + 0{\cdot}38\, t^3$, where n denotes the 'relative number' of spots, and t represents the number of years reckoned from a minimum; whilst from the 5 groups near maximum he obtains $n = 100 - 6{\cdot}14\, t^2 + 0{\cdot}61\, t^3$, t being in this case the number of years from maximum. The mean length of the Sun-spot period is found to be 11·328 years, and the maximum occurs 4·72 years after the minimum. For the curve of spot-frequency *throughout* this period we have the empirical formula:—$n = 55{\cdot}4 + 45 \sin(A - 75^\circ) - 6 \sin(2A + 28^\circ) - 2 \sin(3A - 40^\circ)$, where $A = \dfrac{t}{11{\cdot}328} \times 360^\circ$ reckoned from minimum. This gives a flatter curve at maximum than Wolf's mean curve. Prof. Spoerer gives a comparison between Wolf's epochs of maximum and minimum from observation and his own values calculated with a mean period 11·328 years, from which it appears that the last maximum in 1870·8 was 1·0 year *before* the mean epoch, and the recent minimum, in 1878·9, 0·5 of a year *after* the mean, the interval from maximum to minimum being 8·1 years, whilst the mean interval is 6·6 years. According to Prof. Spoerer's formula, the next maximum should occur in 1883·1.

THE OXFORD UNIVERSITY OBSERVATORY.—Prof. Pritchard's Annual Report, recently published in the 'Oxford University Gazette,' shows no falling off in the activity of this Observatory. The principal work of the year has been the completion of the investigation of the Lunar Physical Libration from measurements of a large number of photographs of the Moon taken with the De La Rue reflector. The result of this laborious discussion is to establish the existence of small but sensible inequalities in the Moon's rotation, thereby affording a general confirmation of the investigation of Bouvard, Nicollet, and Wichmann. Besides this research, series of measures of the positions of forty stars in the Pleiades, and of 250 stars in the cluster 39 Messier, have been made. All the comets visible during the last twelve months have been observed, as well as the outer Satellite of Mars and the double stars ξ Ursæ Majoris and 70 Ophiuchi. Prof. Pritchard has also delivered lectures on various subjects, and has, with Mr. Grubb's assistance, devised a cheap chronograph and equatoreal, and a simple form of precessional globe.

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[*] Astron. Nachr., No. 2335.

THE PARALLAX OF α AND β CENTAURI.—M. Moesta has deduced the following results from his zenith-distance observations at Santiago de Chile in the years 1860–64:—

	α_2 Centauri.	β Centauri.
Parallax......	$0''.521 \pm 0''.066$	$0''.173 \pm 0''.07$
Aberration....	$C + 0.703 \pm 0.140$	$C + 0.114 \pm 0.17$
Proper Motion	-0.393 ± 0.040	

C is Struve's constant of aberration, which thus represents the observations of β Centauri within the limits of probable error. The results for α Centauri, however, give an aberration $0''.7$ greater, from which M. Moesta infers that the velocity of the light-waves emitted by this star in the years 1860–64 was retarded.

COMET 1880 *e* (SWIFT'S).—The Comet discovered by Prof. Swift on Oct. 10 was not seen in Europe till it was picked up as a new object by Dr. Lohse at Lord Lindsay's Observatory, Dunecht, on Nov. 7. It had, however, been observed in America on Oct. 21, 25, and 28, and from these observations an orbit was computed by Mr. S. C. Chandler, jun., and published in a circular of the Boston 'Science Observer,' which showed that this comet was identical with Comet iii. 1869. This would make it a periodical comet of 11 years. Dr. K. Zelbr and J. von Hepperger have computed, from observations on Oct. 25 and 28, and Nov. 7 and 9, a set of elements closely agreeing with those found by Mr. Chandler. The following comparison shows the agreement between these elements and those computed by Dr. Bruhns for Comet iii. 1869:—

	Comet iii. 1869.	Comet 1880 *e* (Swift's).
Per. Pass.	1869, Nov. 20·85426	1880, Nov. 8·3220, Berlin. M. T.
Long. Per.	$41^\circ\ 17'\ 12''.5$	$42^\circ\ 7'\ 4''.4$
Long. Node......	292 40 28 ·8	295 36 54 ·1
Inclination	6 55 0 ·0	7 22 5 ·3
Log. Per. Dist. ..	0·042416	0·042122
Mean Equinox ..	1870·0	1880·0

It is worthy of note that Dr. Bruhns calls attention to the indications of a deviation from a parabolic orbit shown by the observations of Comet iii. 1869.

Swift's comet is now receding rapidly from the Earth and the Sun. It had been approaching both the Earth and the Sun in the beginning of November, and its theoretical brightness was at a maximum on Nov. 15, when it was nearly three times as bright as at the date of discovery (Oct. 10). But Prof. Swift considered on Nov. 1 that it was certainly fainter than he had previously seen it, so that the intrinsic brightness must have considerably diminished. The comet has a very diffused and ill-defined disk, several minutes in diameter, and was on Oct. 31 a faint object with a 3-inch telescope. It is now growing rapidly fainter by increase of distance.

COMET 1880 *d* (HARTWIG'S)*.—Prof. Winnecke finds that this comet may have been identical with the comets of 1382, 1444, and 1569, as well as with that of 1506, and he is thus led to suggest a period of 62⅓ years as probable. With this period an ellipse has been calculated, which represents tolerably well the observations on Sept. 29, Oct. 14 and 24; but further discussion will be required to decide whether the period is really one of 62⅓ years. Prof. Winnecke calls attention to the circumstance that the node of this comet falls very near that of Mercury and that the respective radii vectores are then nearly equal, as likely to give rise to very large perturbations by this planet. Perhaps it is to Mercury that the elliptic path of this comet is due.

A NEW planetary nebula has been discovered by Dr. Copeland at Dunecht, using Prof. Pickering's method of search. Its position for 1880 is R.A. $21^h\ 2^m\ 11^s{\cdot}8$, Dec. 47° 22′·2 N., or Pos. Angle 240°, Dist. 22″ from B.D. + 47° No. 3289. It is small, binuclear, and gaseous.

THE minor planet picked up by Prof. Peters on Oct. 10 is identical with that discovered by Herr Palisa on Sept. 30.

EDITOR.

Minima of Ceraski's new Variable in Cepheus†.

By Dr. JULIUS SCHMIDT.

1880.	G. M. T.				
Dec. 2	$8^h\ 54^m{\cdot}7$	Dec. 12	$8^h\ 13^m{\cdot}8$	Dec. 22	$7^h\ 32^m{\cdot}8$
7	8 34 ·2	17	7 53 ·3	27	7 12 ·3

The period is $2^d\ 11^h\ 49^m{\cdot}756 = 2^d{\cdot}492886$, and the range of magnitude from about 7·5 to 9·0.

The place of the variable is R.A. $0^h\ 51^m\ 48^s$, Dec. 81° 14′·1 N.

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* Circular No. 2 of the Strassburg Observatory.

† Astron. Nachr. No. 2346.

Astronomical Memoranda, 1880, *December*.

Sun. Dec. 1, sets $3^{h}\ 52^{m}$, rises $19^{h}\ 48^{m}$; Dec. 11, sets $3^{h}\ 49^{m}$, rises $20^{h}\ 0^{m}$; Dec. 21, sets $3^{h}\ 51^{m}$, rises $20^{h}\ 6^{m}$; Dec. 31, sets $3^{h}\ 58^{m}$, rises $20^{h}\ 9^{m}$.

Equation of Time:—Sun *before* Clock, Dec. 1, $10^{m}\ 35^{s}$; Dec. 11, $6^{m}\ 17^{s}$; Dec. 21, $1^{m}\ 25^{s}$; *after*, Dec. 31, $3^{m}\ 31^{s}$.

Sidereal Time at Mean Noon:—Dec. 1, $16^{h}\ 42^{m}{\cdot}9$; Dec. 11, $17^{h}\ 22^{m}{\cdot}3$; Dec. 21, $18^{h}\ 1^{m}{\cdot}7$; Dec. 31, $18^{h}\ 41^{m}{\cdot}2$.

Moon.	*sets.* h	m		*sets.* h	m		*rises.* h	m
Dec. 1..			Dec. 11..	15	44	Dec. 21..	9	9
2..	4	6	12..	16	53	22..	10	17
3..	5	18	13..	17	59	23..	11	28
4..	6	39	14..	19	0	24..	12	39
5..	8	4	15..	*rises.*		25..	13	55
6..	9	28	16..	3	47	26..	15	14
7..	10	49	17..	4	45	27..	16	35
8..	12	7	18..	5	48	28..	17	52
9..	13	20	19..	6	54	29..	19	0
10..	14	33	20..	8	1	30..	19	55

New Moon, Dec. 1, $14^{h}\ 56^{m}$; First Quarter, Dec. 8, $6^{h}\ 38^{m}$; Full Moon, Dec. 16, $3^{h}\ 36^{m}$; Last Quarter, Dec. 24, $6^{h}\ 57^{m}$; New Moon, Dec. 31, $1^{h}\ 56^{m}$.

Mercury is a morning star, at greatest elongation (21° 5′ W.) on Dec. 11, when he rises in the E.S.E. at $17^{h}\ 55^{m}$, more than 2^{h}, before the Sun, and is very favourably situated for observation. He may then be picked up near β Scorpii, being about 2° *n p* this star on Dec. 11, 19^{h}, and about 1° *n f* on Dec. 13, 19^{h}.

Venus is an evening star. Diameter:—Dec. 1, $13''{\cdot}2$, Dec. 31, $15''{\cdot}5$. Illuminated portion of Disk 0·773 on Dec. 15.

	R.A.	Dec.	tr.	sets
Dec. 1,	$19^{h}\ 6^{m}{\cdot}6$,	24° 32′ S.,	$2^{h}\ 24^{m}$,	$6^{h}\ 9^{m}$
31,	21 37 ·4	16 5 S.,	2 56	7 36

Jupiter is stationary in Pisces. Diameter:—Dec. 1, $42''{\cdot}1$; Dec. 31, $38''{\cdot}1$.

	R.A.	Dec.	tr.	sets
Dec. 1,	$0^{h}\ 37^{m}{\cdot}6$,	2° 28′ N.,	$7^{h}\ 53^{m}$,	$14^{h}\ 9^{m}$
31,	0 42 ·0	3 6 N.,	6 0	12 19

Daily rate of rotation $870^{\circ}{\cdot}5 = 2^{r}\ 150^{\circ}{\cdot}5$; Hourly rate $36^{\circ}{\cdot}28$; Period $9^{h}{\cdot}925 = 0^{d}{\cdot}4135$.

Saturn is stationary in Pisces, about 12° E. by N. of Jupiter.

	R.A.	Dec.	tr.	sets
Dec. 1,	$1^{h}\ 27^{m}{\cdot}6$,	6° 18′ N.,	$8^{h}\ 43^{m}$,	$15^{h}\ 18^{m}$
31,	1 25 ·8	6 16 N.,	6 44	13 18

	Outer Ring. Maj. Axis.	Outer Ring. Min. Axis.	Inner Ring. Maj. Axis.	Inner Ring. Min. Axis.	Ball. Diam.
Nov. 30	$43''{\cdot}64$	$9''{\cdot}91$	$29''{\cdot}02$	$6''{\cdot}59$	$17''{\cdot}4$
Dec. 20	42 ·28	9 ·51	28 ·12	6 ·33	16 ·6

The south side of the rings is visible, the elevation of the Earth above their plane being 13° 8′ S. on Nov. 30, and 13° 0′ S. on Dec. 20, and of the Sun 15° 1′ S. and 15° 17′ S.

Neptune may be found about 1⅔° *sp* σ Arietis.

Dec. 1, R.A. $2^h\ 41^m{\cdot}2$, Dec. 13° 46′ N., tr. $9^h\ 57^m$
31, 2 39 ·1 13 37 N., 7 57

Phenomena.

		G. M. T. h	G. M. T. m	
Dec.	2	11	6	J. i Oc. D.
	3	8	17	J. i Tr. I.
		10	31	J. i Tr. E.
		10	47	J. ii Tr. I.
	4	5	34	J. i Oc. D.
		8	59	J. i Ec. R.
	5	4	59	J. i Tr. E.
		5	7	J. ii Oc. D.
		10	10	J. ii Ec. R.
		10	39	J. iii Oc. D.
	9	5	53	J. iii Sh. I.
		8	18	J. iii Sh. E.
	10	10	8	J. i Tr. I.
		12	23	J. i. Tr. E.
	11	6	9	101 Pisc. Oc.*
		7	26	J. i Oc. D.
		7	55	104 Pisc. Oc.*
		9		☿ elong. 21° 5′ W.
		10	55	J. i Ec. R.
	12	6	52	J. i Tr. E.
		7	36	J. ii Oc. D.
		10	39	B. A. C. 782 Oc. D. 146°.
	13	5	0	ζ Ariet. Oc.*
	13	5	24	J. i Ec. R.
	7	54		τ^1 Arietis Oc. D. 109°.
	14	3	50	A Tauri Oc.*
		5	17	J. ii Tr. E.
	16	1	45	Eclipse of Moon.
		5	33	
		4	29	J. iii Tr. I.
		7	11	J. iii Tr. E.

		G. M. T. h	G. M. T. m	
Dec.	16	9	55	J. iii Sh. I.
		17	51	η Gemin. Oc. D. 133°.
		18	25	η Gemin. Oc. R. 202°.
	17	12	1	J. i. Tr. I.
		16	57	ζ Gemin. Oc. D. 89°.
		18	0	ζ Gemin. Oc. R. 235°.
	18	9	19	J. i Oc. D.
		13	13	*g* Gemin. Oc. R. 225°.
	19	6	29	J. i Tr. I.
		8	44	J. i Tr. E.
		10	7	J. ii Oc. D.
	20	7	20	J. i Ec. R.
	21	5	9	J. ii Tr. I.
		7	51	J. ii Tr. E.
	23	8	23	J. iii Tr. I.
		11	6	J. iii Tr. E.
	25	11	14	J. i Oc. D.
	26	8	23	J. i Tr. I.
		10	38	J. i Tr. E.
	27	5	42	J. i Oc. D.
		6	8	J. iii Ec. R.
		9	15	J. i Ec. R.
	28	5	7	J. i Tr. E.
		7	45	J. ii Tr. I.
		10	28	J. ii Tr. E.
	30	7	18	J. ii Ec. R.
	31	1	40	Eclipse of Sun.
		3	28	

* A near approach.

A partial eclipse of the Sun, visible in the British Isles and Western Europe, occurs on Dec. 31. At Greenwich the Eclipse begins at $1^h\ 39^m.7$, and ends at $3^h\ 28^m.2$ G.M.T. Magnitude of the Eclipse (Sun's diameter = 1) 0·388.

There is also a partial eclipse of the Sun on Dec. 1, invisible at Greenwich.

A total eclipse of the Moon occurs on Dec. 16. Magnitude of the Eclipse 1·390. The Moon rises at Greenwich at $3^h\ 46^m$.

	Penumbra.	Shadow.	Total phase.	
First contact ..	$0^h\ 31^m.5$	$1^h\ 44^m.5$	$2^h\ 54^m.0$	G.M.T.
Last Contact ..	6 46 ·1	5 33 ·1	4 23 ·6	

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G.M.T. of Transits of Red Spot across Central Meridian of Jupiter.

	h		h		h
Dec. 1..	9·1	Dec. 11..	7·3	Dec. 21..	5·6
2..	4·9	12..	13·1	22..	11·4
3..	10·7	13..	9·0	23..	7·2
4..	6·6	14..	4·8	24..	13·0
5..	12·4	15..	10·6	25..	8·9
6..	8·2	16..	6·5	26..	4·7
7..	4·1	17..	12·2	27..	10·5
8..	9·9	18..	8·1	28..	6·3
9..	5·7	19..	4·0	29..	12·1
10..	11·5	20..	9·7	30..	7·9
				31..	3·8

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